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STRATEGIC MINERAL SUPPLIES*

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SOON after the War Production Board was set up, early in 1942, it issued a pamphlet called "Federal Aids for War Mineral Production," with the subtitle "How to Get Help from the Federal Government for Development and Increasing Output of Mineral Properties." It pointed out that ores and minerals were urgently needed for America's war effort. Ores of 25 different metals, from aluminum to zinc, and 23 miscellaneous minerals, from arsenic to zircon, were enumerated. In carrying out its responsibilities to obtain these required ores and minerals the War Production Board coordinated into the war mineral program the United States Geological Survey, the Bureau of Mines, the Reconstruction Finance Corporation, and the Board of Economic Warfare. I propose to outline only the impact of the war on the activities of geologists, official and unofficial, in connection with the procurement plans for obtaining from domestic sources the necessary mineral supplies.

There were four jobs that these geologists had to do:

The first task was to determine the availability of raw materials, without which no weapons of war can be made. Authoritative information on how much ore is available is fundamental for any procurement plan, as well as for any intelligent national mineral policy. To obtain this information most of the actual and potential producing properties, scattered throughout the 48 states and Alaska, had to be inventoried by geologists. Their estimates of ore available were expressed in three categories: (1) measured ore, which is computed from known dimensions and whose grade is known from detailed sampling; (2) indicated ore, which is computed partly from measurements and partly from dimensions based on geologic evidence; and (3) inferred ore, estimated chiefly on geologic evidence and based on few if any samples. Only by detailed geologic examination in the field can this information be got; and at the same time the vital matter of the rate at which the ore can be produced at given prices of the metal can be obtained.

A second duty that geologists have been performing is geologic supervision at operating properties. Large corporations have resident geologic staffs—Anaconda Copper Co. at Butte, Montana, for example, has a staff of 35 geologists, but the smaller companies cannot afford them. As an unprecedented war measure, the U. S. Geological Survey supplied many of these mines with resident consulting geologists, either from its own staff or from the universities of the country. These geologists were then left

* From an address before the Yale Chapter of Sigma Xi, April 1945.

at the mines to cope with the geologic problems as they arose. Their principal problems were to discover more ore and to find the missing segments of ore bodies that had been dislocated by faults and become lost, as it were.

The third job undertaken by geologists was to find more ore in the known producing districts. By an intensive study of a producing district, in which innumerable openings disclose the ore bodies in three dimensions, the special features can be ascertained that determine where the ore is and why it is where it is. When these idiosyncrasies of ore occurrence peculiar to the individual districts are grasped, they can be used as guides in the search for more ore. This method has to date been by far the most successful procedure in finding new supplies of ore.

The fourth duty laid upon the geologist is the most difficult of all; it is the one generally expected of him by the layman; namely, the finding of new districts. We now have a vast body of information concerning the occurrence of the 92 chemical elements in the earth's crust and how they have at some places become concentrated into bodies of economic value. But it is not yet known *why* any mining district is located where it is. Let me illustrate. The world's supplies of the chief industrial metals—copper, zinc, lead, tin, tungsten, and molybdenum — are to be regarded as minor by-products of mighty geologic processes. The sequence of events that leads to the forming of an ore body of these metals can be summarized thus: First there was a downwarping of the earth's crust, forming a narrow trough hundreds of miles long—a geosyncline, Dana called it. It slowly deepens, and as it does so marine sediments accumulate in it, keeping it filled up nearly to sea level. In the fullness of time—I mean fullness in the geologic sense, many million years—when strata on the order of 40,000 feet in thickness have accumulated in the geosyncline, a mountain-making revolution sets in. The great pile of strata in the geosyncline is subjected to immense lateral pressure; the strata are folded and the folds are crowded together, and a mountain range is born. Under the additional load thus put on the crust by the closely appressed strata, the bottom of the folded tract sinks deeper into the subcrust. Simultaneously, enormous volumes of molten rock-matter, hundreds or thousands of cubic miles in extent, rise from the depths and invade the newly formed mountain range. Eventually the molten rock matter cools down and solidifies as granite. Erosion, attacking the mountain range thereafter, strips off the covering rocks and eventually exposes the granite core. Around high points on the granite core there may be a clustering of metal-bearing veins, and when these are found by the prospector, a new mining district springs into existence. Technically, we express these ideas by saying that ore deposits of certain kinds are related in time and space to the invasion of the earth's crust by masses of molten granite. Intrusive granites are not only common the world over but they range in age from very old—2 billion years—to very young. The earth has shown no marked decrease in its power to generate granite masses, nor has there been any enfeeblement in the ore-bringing powers of these granites; in other words, the largest and richest ore districts were not necessarily formed early in the history of our planet and smaller and poorer deposits formed as time went on. On the contrary, the world's richest metal-producing district, in the sense that it contains the greatest concentration of metals within a small area, is Butte, Montana, and the ore was formed at the beginning of geologic modern times, some 60 million years ago.

At this point we are confronted with a

great gap in our knowledge. We do not know why certain granite intrusions were productive of ore deposits and why others were sterile. New England affords us a striking example: not once, but three times during its geologic history has it been invaded by great volumes of molten granite, but all three of these invasions produced no ore—they were sterile granites. To account for the extraordinary difference among granites as ore-bearers is a major problem in theoretical geology. When this problem is solved by more research, a by-product will be a great advance in the practical art of ore discovery.

The large number of metals and minerals required in the war effort were classified in three categories: (1) strategic minerals, which are necessary but cannot be produced within our own country no matter how great the price stimulation; (2) critical minerals, which are necessary and can be produced by price stimulation, and (3) essential minerals, which are necessary and of which we have an ample supply. These distinctions are important, and as I shall attempt to show, are rooted in the fundamental geology of the substances. Tin and nickel are strategic metals, and no amount of money or political pressure can produce them from our own territory in more than minute amounts. Nickel indeed poses this unsolved problem: Why is nine-tenths of the world's known supply of nickel localized at Sudbury, Ontario, and most of the remainder in New Caledonia? Since all three classes of minerals were necessary to the war effort, these terms tended to lose their sharpness of definition; all war minerals were likely to be called strategic minerals.

Owing to war demands, chiefly in airplane construction and in incendiary bombs, magnesium developed faster than any other metal in history. Its production increased more than fiftyfold in the four years between 1939 and 1943. Five sources of raw material were available, two of which are inexhaustible (true of few other mineral resources). These two sources are sea water, which contains 0.13 percent Mg, and dolomite, which contains 13 percent, or 100 times as much as sea water. Now, the distribution of dolomite throughout the United States has long been known to geologists, and when a representative of one of the Government agencies visited Yale to find out whether we knew where there is an available supply of dolomite 99 percent pure in eastern New York or Connecticut, he was told by a geologist attached to our department that such a deposit occurs at Wingdale, Dutchess County, New York, and, soon after, a magnificent magnesium reduction plant was built and put into operation.

In response to the demands of war a complete inventory of the dolomite resources of the country was made, which necessitated accurate sampling and tonnage measurements. Although dolomite is an abundant rock, making up formations hundreds of square miles in extent, yet the number of deposits becomes surprisingly small when we demand 99 percent purity, accessibility, nearness to transportation, and availability of a large supply of electric power.

Aluminum is highly important as a war metal. Our supply of aluminum ore (bauxite) has come mainly from Surinam in South America. Early in the war submarine sinkings greatly jeopardized the flow of this ore to our country; in fact, according to Alan M. Bateman, of the Bureau of Economic Warfare, they actually succeeded in stopping the flow for a time. Our only important domestic source of aluminum ore of metallurgical grade is in Arkansas. To meet the war crisis production was stepped up in Arkansas fifteenfold: from 400,000 tons a year to 6,000,000 tons. As this rate of production would have de-

pleted the known deposits in less than two years, a vigorous campaign of exploration for more ore was begun by the Geological Survey and of verification by the Bureau of Mines. That is, the geologists predicted, on the basis of their theories of how the aluminum ore had been formed, where the ore lies under a cover 60 to 100 feet thick, and the engineers, by putting down drill holes, proved that the ore is there; this was very gratifying teamwork. In this way many million tons of ore were found. As soon as the danger of submarine sinkings was ended, foreign ore, which is of higher metallurgical grade than the Arkansas bauxite, was again imported; consequently, our own source of fairly high grade aluminum ore has been saved for postwar use. So relatively small are our known domestic reserves of bauxite, however, that it is the part of wisdom to provide reserves of substitute materials. In line with this, Congress has forehandedly appropriated large sums to the Bureau of Mines to study the extraction of aluminum from high-alumina clays and feldspar rocks. Reluctantly, I leave the subject of aluminum ore, for the principles of its origin are so well determined and the ore itself is so inconspicuous and downright unimpressive that future geologic exploration will probably discover great additions to the proved reserves of aluminum ore, if not in the United States then in the rest of the world.

Vanadium is another important war metal, normally obtained from Peru. A remarkable discovery of vanadium ore in Idaho, of a kind never found before, was made by one of the Federal geologists, W. W. Rubey. The geologist himself modestly says it was an accident, but it is one of those accidents, as Pasteur said, that happens only to the trained mind. An immense tonnage of vanadium ore has been proved to occur in Idaho. The metallurgy of extracting the vanadium has not yet been perfected, perhaps I should say fortunately, for the vanadium now remains in the ground, where it is available for a future national emergency. However, the prevalent idea is that mineral deposits should not remain in the ground. All past history shows that no known mineral deposit will be saved as a reserve for the benefit of future generations if that deposit will yield a profit by taking it out of the ground. Furthermore, the engineer can show, by the aid of the inexorable logic of mathematics, that the maximum profit is won by extracting a mineral deposit as rapidly as possible, thus converting it into money, and this money can be put into the bank where it will earn interest in perpetuity without its owner doing any further work. This logic ignores the national welfare, but money talks, powerfully.

Many other strategic and critical minerals have undergone extraordinarily interesting developments during the war, but I shall mention only the pegmatites from which mica, beryl, tantalite, and columbite can be obtained. Pegmatite is the name given to certain remarkable tabular rock masses of coarse or gigantic grain size occurring as dikes and veins around the periphery of granite masses. The largest known crystals occur in pegmatites, the world's record being a crystal of the lithium mineral spodumene 42 feet long occurring in the Black Hills of South Dakota. Pegmatites are numerous (New England is full of them), but few of them carry anything of value. Furthermore, the valuable minerals are erratically, capriciously, and unpredictably distributed throughout a given pegmatite. The result is that the mining of pegmatites is a headache; and mining corporations generally leave them alone; they leave them to be worked by the little fellow who has invincible optimism and does not mind going broke. Although pegmatites are of great interest to geologists and have been much studied, no

experienced geologist would venture, even under pressure, to forecast how much valuable mineral content any given pegmatite might have. But because pegmatites are the sole sources of vitally needed mineral commodities, the problem had to be attacked.

Mica is a top-priority mineral. Every spark plug, every motor and generator brush, and all radio condensers require mica, and no substitute has been found. Our main source is India, and for a time the mica was flown in by airplane. To stimulate domestic production, the Government set up the Colonial Mica Corporation. This corporation caused a great expansion of mining; from less than 50 mines in 1942 to 789 in 1944, but all of these were small-scale operations based on pegmatites long known, mainly in North Carolina and New England. Colonial Mica, i.e., the Government, paid on the average \$6.00 per pound of mica and sold it at an average of \$1.60 a pound. That amounts to a bonus of \$4.40 a pound. In spite of this powerful stimulation, scarcely any new deposits of exceptional grade have been found.

Tantalum is a rare element, the demand for which has greatly increased during the war—in radio grids and for other purposes, the use of tantalum wire in surgery in sewing nerves being a remarkable development. Tantalum was imported mainly from Western Australia; now from Brazil. Columbium, which occurs with tantalum in the solid-solution series tantalite-columbite and was until recently regarded as a nuisance, is now in demand for jet-propelled planes as the only metal that will stand the high temperatures.

Beryllium is the last of this geochemical group of elements I propose to mention; it is obtained from the pegmatite mineral beryl, which is its sole commercial source the world over. Beryllium is lighter than aluminum and confers remarkable properties when alloyed with copper; it is therefore a metal of great interest. Recently, when I was in Washington, I was told that at a committee meeting, a common event in that city, a naval officer was so impressed by the accounts of the remarkable properties of beryllium that he exclaimed, "Now we will have to build our destroyers with beryllium." Alas, he had no geologic inhibitions! He was unaware, like many others, that mineral procurement is not solely a matter of technology and economics, but is ultimately controlled by geologic laws.

The Oil Situation

The impact of the war on the oil situation has been tremendous. The importance of petroleum and its products in waging a modern war was epitomized by Marshal Foch: "In war a drop of gasoline is worth a drop of blood." That remark has always seemed to me to cut like a two-edged sword: while it emphasizes the supreme importance of gasoline, it also emphasizes the infinitesimal value of human blood in modern warfare. At the end of World War I Lord Curzon said, "The Allies floated to victory on a sea of oil." Such was the recognized importance of oil in World War I. In World War II the gasoline needs at the time of writing were already 80 times greater than in the previous war. Every American soldier overseas required more than 50 gallons of petroleum products per week, and this figure takes no account of the enormous needs of the Navy nor of those of our Allies. To meet these insatiable demands domestic production has been speeded up and the output reached a new peak in 1944—1,700 million barrels (a barrel being 42 gallons)—11 percent greater than that of the preceding year.

Besides this speeding up of the oil production, other things have been done. For example, there was the Canol Project which has attracted much attention.

Why did the Army in face of much opposition back the project so firmly? The Army was evidently convinced by its consulting geologist that oil was there, and so the pipeline was constructed. At any rate, oil flowed at the rate of a million barrels a year from the field to the refinery at Whitehorse, Yukon Territory, some 600 miles distant. Despite assertions to the contrary, the oil flows even at the winter temperatures of 20° below zero F. Reserves at Canol are conservatively estimated at 60 million barrels. Private interests are exploring beyond the limits of the pool. Much larger reserves will have to be found in order to justify taking the oil to seaboard through a larger pipeline after the war.

The enormously accelerated rate at which we were compelled to draw off the oil from its underground sources is the main cause of the present concern as to the future of our oil supplies. Our proved reserves of oil, 20½ billion barrels, are the largest in the history of the industry. (By "proved reserves" we mean the oil that can be estimated with some accuracy as the result of drilling.) In the case of a particular oil field its reserves are regarded as proved if (1) the size of the field is known because sufficient bore holes have been put down to outline the field, (2) the thickness of the oil-bearing beds is known, and (3) the volume of the pore-space in these beds is known. From these figures we can compute the volume of the oil in the ground; but the recoverable oil, which is the oil that present practice can obtain at the earth's surface, is only a fraction of the oil in the ground. It is between 15 and 40 percent, depending, among other things, on the rate of recovery, which should not exceed a certain optimum rate. Consequently, an "exhausted oil field" still holds about 75 percent of its original content of oil. Colonel Seller's famous remark "There's millions in it" is a gross understatement: when the appropriate technique is devised to move this reluctant oil to the earth's surface, there will be literally "billions of dollars in it."

The wartime rate of extracting our oil is too fast for efficient operation. One way to maintain efficient operation is to find more oil fields and so distribute the load more widely. Therefore, 24,345 wells were completed during 1944, an increase over the previous year; 5,000 of these were exploratory wells. Although 1,700 million barrels of oil were taken out of the ground in 1944, the year ended with a gain of 500 million barrels of proved reserves. Unfortunately, most of this gain was the result of extensions of fields already found in previous years and therefore it cannot be considered wholly new discovery. On the other hand, the size of the newly discovered fields has been disappointingly small. The downward trend, which began in 1937, in the rate of discovery of new reserves causes the present discussion of the danger of an impending shortage of domestic oil.

It is in the finding of oil that geology has achieved its most brilliant practical results. The birthday of the oil industry of the United States was in 1859, when Colonel Drake, of New Haven, Conn., brought in the pioneer well in western Pennsylvania; that well was 69½ feet deep and yielded 25 barrels of oil a day. The boom was on! By 1861, that is, only two years later, geologists had already outlined the fundamental principles that govern the accumulation and occurrence of oil in the strata. But for nearly 50 years these geologic principles were not believed by the managements of oil companies; in fact, they greatly preferred their own theories; perhaps understandably, since certain powerful geologists, such as J. P. Lesley, strongly opposed the acceptance of the prevailing geologic explanations. However, after the turn of the present century accep-

tance was rapid and practically all oil companies now have geologic staffs; in fact, oil geologists are by far the most numerous of the species, the American Association of Petroleum Geologists alone enrolling 4,000 of them. Recently two exploratory wells, one in California and the other in Texas, attained depths exceeding 16,200 feet—considerably more than 3 miles! This achievement measures not only the great technological advance that now makes it possible to bore to this extreme depth, but also the strong conviction necessary to plan and to carry out such a project; a deep well of this kind costs $600,000 to $800,000 or more, and no oil company would expend such sums except for powerfully logical or—dare I say—geological reasons.

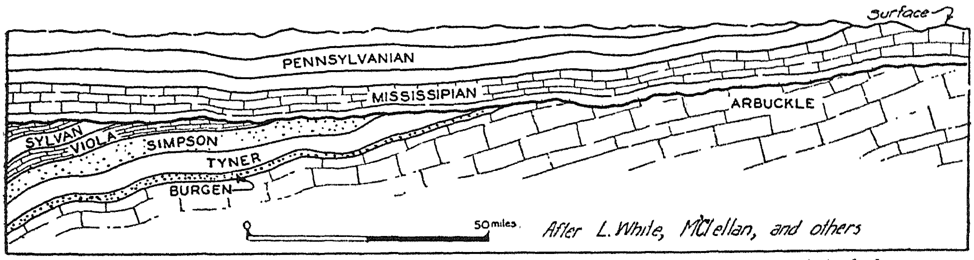
I have already mentioned that the amount of new oil discovered since 1937 has been disappointingly small. So far a total of more than 3,000 oil fields have been found in the United States; of these, 100 rank as major fields. By a major field we mean one from which the total oil produced will exceed 100 million barrels; and it is the great falling off in the number of major fields discovered that is causing so much concern. The rate of discovery has not fallen off for lack of exploratory drilling, for the amount of wildcatting, as the industry calls it, is as high or higher than it ever was. In the mining industry wildcatting is a term of great opprobrium, but in the oil industry it is one of high esteem. A wildcat is an exploratory hole drilled completely outside of known fields. About 75 percent of all wildcats are now drilled on geologic advice and the other 25 percent are drilled for nontechnical reasons; on "hunches," "doodlebug" indications, and for other equally recondite reasons; 20 percent of the holes drilled on technical advice are successful, as shown by statistical evidence, whereas only 4 percent of those located without technical advice are successful.

The problem confronting us is to find oil fast enough so that our reserves will not be required to produce beyond the maximum efficient rates. It must be pointed out that no method yet devised, geologic or geophysical, finds oil directly; what is found by the oil-hunting methods is a likely trap, or reservoir; but until the reservoir rock is actually perforated by the drill no one knows whether it contains oil.

Additional reserves of oil within the United States may be sought (1) in regions that have not yet been thoroughly examined, but of such territory little is left, so intensive has the search been; (2) in strata much deeper than those in which oil has already been found, as indicated by the steadily increasing average depth of exploratory holes, now between 4,000 and 5,000 feet deep; and (3) below an unconformity in a second layer of geology, in which the oil occurs in an entirely different way from that in the overlying layer.

In the accompanying figures such "layers of geology" are shown in profile. Figure 1 gives a cross-section through a region in which two such "layers of geology" are present, in each of which the oil occurs according to different rules. Manifestly, the oil in the lower layer is a very effectively concealed resource. Figure 2 shows a still more complex condition, in which there are three superposed layers of geology, each characterized by its particular mode of occurrence of oil; and Figure 3, which illustrates in a general way the conditions in northern Louisiana and southern Arkansas, shows four superposed layers, each separated by an unconformity. Each unconformity, in brief, represents tilting of a series of strata and uplift above sea level, beveling by erosion, submergence of the beveled strata beneath the sea, and the deposition of a new series of strata on the surface worn across the older strata.

Other valuable mineral resources may



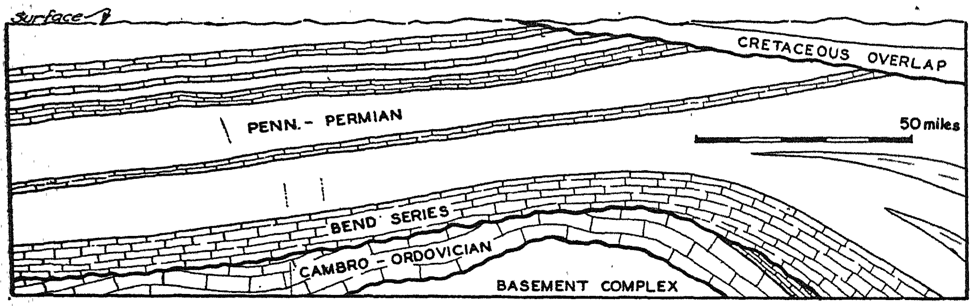
Courtesy of A. I. Levorsen

FIG. 1. TWO LAYERS OF GEOLOGY
IN SOUTHEASTERN KANSAS AND NORTHEASTERN OKLAHOMA. THE TWO LAYERS ARE SEPARATED BY AN UNCONFORMITY (HEAVY LINE); THEIR GEOLOGY (PETROLEUM AND GENERAL) DIFFERS GREATLY.

be concealed below an unconformity, as illustrated by the famous United Verde copper deposit in Arizona. From left to right, Figure 4 shows five stages in the geologic history of the deposit: (1) the ore body as it was shortly after it was formed in Precambrian time, more than a half billion years ago; (2) slicing of the ore body into two segments along a fault and separating them by 2,400 feet; (3) erosional reduction of the area in which the two segments occur to a flat surface; (4) deposition of limestone on the outcrops and subsequent flooding by basalt lava; and (5) present condition, in which one segment of the ore body is exposed at the earth's surface and the other segment, which is below the unconformity, remains concealed under a cover of limestone and basalt. The exposed ore body, the United Verde, one of the largest pyritic copper deposits in the world, was found in the early eighties, but more than 30 years elapsed before the concealed ore body, the United Verde Extension, was found. The finding of that bonanza ore body in 1914 is significant in another respect: it is the only major discovery of copper ore in the United States during the past 40 years.

To return now to the matter of the undiscovered oil reserves. Finally, undiscovered oil may occur in what are termed stratigraphic traps, few of which, in the light of present scientific knowledge, can be found by rational methods. In fact, the greatest of all oil fields, the East Texas field, in which the oil is in a stratigraphic trap, was discoverered by pure wildcatting.

In the last two decades geophysics has been enlisted in the search for oil. The seismograph, the gravity meter, the magnetometer, and the galvanometer are the chief instruments used. Geophysical methods are a form of geologic explora-

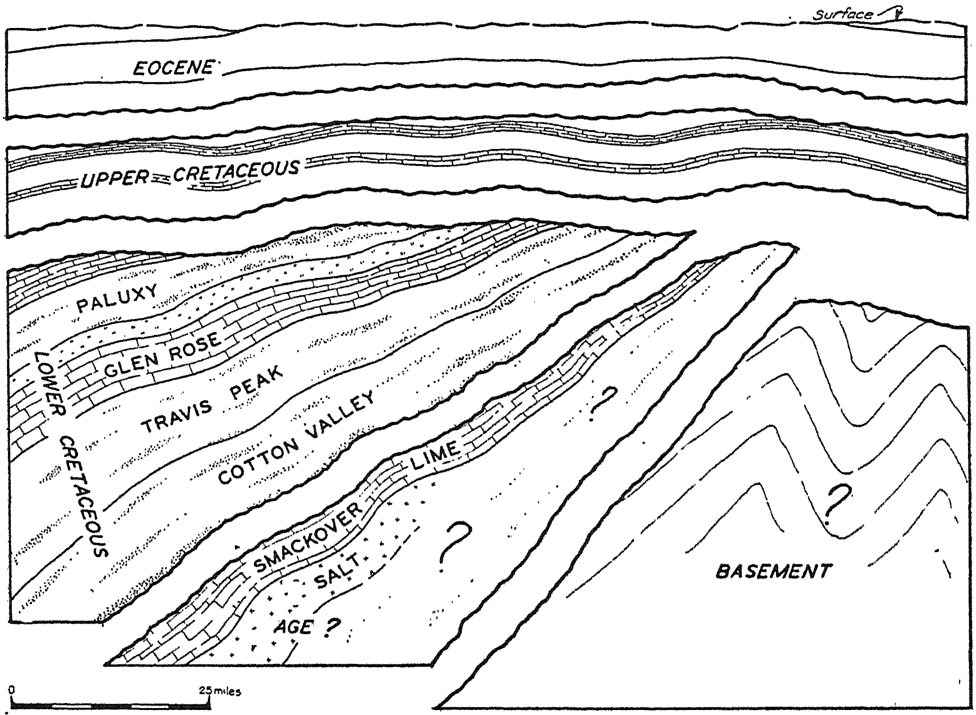


Courtesy of A. I. Levorsen

FIG. 2. THREE LAYERS OF GEOLOGY
IN NORTH-CENTRAL TEXAS. THE MODE OF OCCURRENCE OF PETROLEUM DIFFERS IN EACH LAYER.

tion. All of the geophysical prospecting methods "suffer from the disadvantage that they measure only differences in physical properties and do not reveal directly the presence or precise nature of the deposit under consideration." If no geologic information is available in regard to an area that is being prospected by geophysical methods, the geophysical results can be interpreted in several different ways, but if something in regard to the geology is known, the interpretations are thereby restricted. Consequently, the success of these methods depends on how closely geologists and geophysicists collaborate.

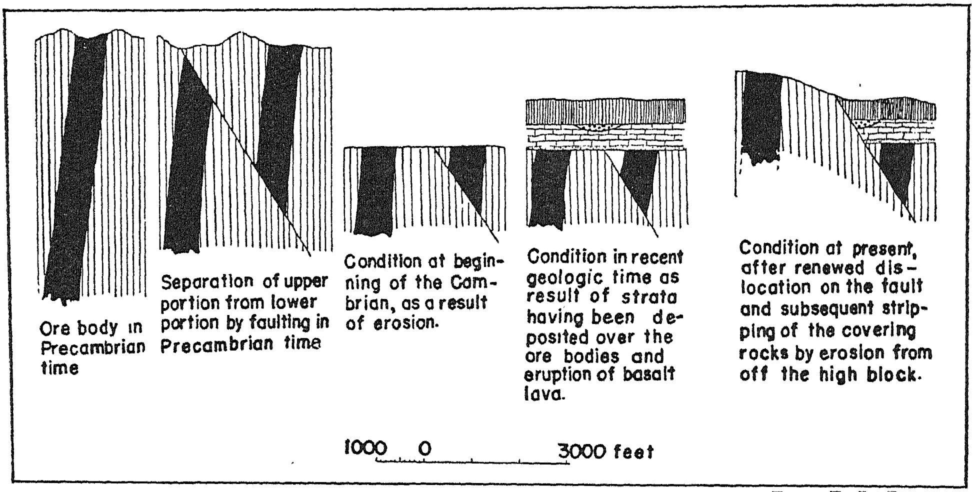
Brilliant results have been obtained by geophysical methods, but the law of diminishing returns has already begun to operate. It is therefore the conviction of leading petroleum geologists that what is urgently needed in order to find the undiscovered oil is more geology, "but on a much higher scientific level than heretofore." In short, not only the easily found oil fields have been discovered, but also many that required extraordinary skill. Finding the remainder will require a greatly improved "art of discovery," which should inevitably result as a by-product of more research. The oil companies have large forces of geologists and geophysicists engaged in searching for oil, however. Nevertheless these men must concentrate their efforts on what appears likely to



Courtesy of A. I. Levorsen

FIG. 3. FOUR LAYERS OF GEOLOGY

IN NORTHERN LOUISIANA AND SOUTHERN ARKANSAS. IN THE DIAGRAM THE LAYERS HAVE BEEN SEPARATED ALONG THE UNCONFORMITIES FOR THE SAKE OF CLARITY. IN THIS SEQUENCE OF LAYERS, AS IN THOSE SHOWN IN FIGURES 1 AND 2, THE UPPER LAYERS DO NOT SUGGEST THE NATURE OF THOSE BELOW THEM AND EACH LAYER MUST THEREFORE BE PROSPECTED DIFFERENTLY.



From F. L. Ransome

FIG. 4. EVOLUTION OF A COPPER DEPOSIT

SUCCESSIVE STAGES SHOWING HOW A DEPOSIT BECAME CONCEALED BENEATH AN UNCONFORMITY.

bring immediate results, and therefore their attention is focused on relatively small areas. Scientific advances are incidental and accidental. Because of the vital importance of the petroleum supply to the nation, the U. S. Geological Survey has begun a nationwide investigation of all the regions that are probably or possibly oil-bearing. This investigation is based on fundamental principles and is planned as a long-range campaign.

Fundamental to such a long-range campaign is the making of a geologic atlas of the whole United States, comprising more than 13,000 individual maps on a scale of one or two miles to the inch, or even larger scale for districts of particularly complex geology. On these maps will be shown all the formations that underlie the subsoil. Such maps, experience has proved, are the indispensable foundation for all further scientific research, as well as for the more material purposes of finding concealed mineral resources. The best brief explanation why such maps are indispensable is that maps on the large scale of the individual units can be combined and recombined into successively smaller scale maps, and from these the observer can obtain, as it were, a bird's-eye view of the geology of a regional unit. Broad relations thereby become visible, and penetrating deductions become possible.

So far only 15 percent of the area of the United States has been geologically mapped on an adequate scale. That leaves 85 percent yet to be surveyed: we can therefore say that our main work is still ahead of us.

JOHN ERICSSON

By OWEN JOHNSON

How estimate the age of a man? By the years to his death? Or by the way those years have been lived? When we cease to produce we begin to die. By 50 the great majority of men are not so much living as gradually dying—pleasantly, quite unconsciously—but still dying. I know a few who have been plodding the outward trail from a much earlier period. True, some tragic figures at 30 (and how many will come out of this war?) have lived through a dozen lifetimes. Michael Angelo, Leonardo da Vinci, Darwin—such men, by their driving power of creation, double or triple their generous span of life.

To say that John Ericsson lived 86 years would be an understatement. By his complete concentration on his work he crowded into his long life the efforts of three or four average workers. He died in full possession of his faculties. For the last 40 years he had worked 14 hours a day every day of the year, completely happy, completely absorbed. He may well be said to have lived out two centuries.

He lived his life as though it were a race against time, jealous of every hour he had to divert from his workroom. He had no time to spare for the adulation showered on him in the final triumphant years. No time for the distinguished visitors who begged in vain for an interview with the inventor of the epoch-making *Monitor* and the revolutionary screw propeller. No time even for his wife, his friend and admirer to the end, who gave up the unequal struggle and returned to her native England.

"She was jealous of a machine," he said laconically. Even to his few intimate friends he grudged a quarter of an hour's interview.

"Your time is up," he was wont to say. "Now you must go."

His drafting room was his castle, from which his vision soared outward across the great uncharted future of science. So little had been done—so much lay ahead and so little time! If he could have 50 years more, 20—10! Even when he needed a short nap he refused to leave his monastic cell, stretching out on his drafting board with a book for his pillow. In all his years in New York he never saw Central Park and only once set foot on Brooklyn Bridge, and that by a trick of a friend. Once Delameter, his closest associate, fearful for his health, tried to inveigle him into a trip to Niagara Falls.

"What's wrong with them?" he asked, not without a certain grim humor. No other reason could justify the loss of a working day.

He died a figure of world dimensions. The country of his adoption paid him the signal honor of conveying his body with full military honors to the country of his birth, which for a generation had clamored for his return as a national hero.

EVERYTHING about Ericsson's life had a touch of the dramatic, even to the month of his birth: July 1803. For in that same month Robert Fulton was conducting his first experiments with his paddle-wheel steamer on the Seine, little dreaming that a genius was born who would nullify all that he was creating.

It was a dramatic century which had just begun, the era of accelerating speed of which the end is not yet in sight. The speed of locomotion had been static since the dawn of civilization. The world was still jogging along on land at a pace of six miles an hour. On sea the winds and their caprices still ruled, as they had since the days of the Greeks and the Phoenicians. Galileo had cut the dis-

tances to the stars but the great oceans still held their barriers against the races of men. When Ericsson died in 1889 the conquest of terrestrial space was in full sway, largely due to his contribution of the revolutionary motive force of the screw propeller. Today this one physical achievement has wrought social and political evolutions which have determined the whole future course of civilization.

Never once through 40 years of persisting adversity was Ericsson's confidence in himself shaken. No failure, no ingratitude of nations, no ridicule from his own profession was able for a moment to sway him from the destinies he knew were in him. And the key to his character is in the drama of his boyhood.

He was blessed with three gifts: poverty, the greatest spur of all; isolation from the conforming influence of the crowd, and the imperative necessity of securing his own education. His parents were of good average stock living a pinched existence. He was born at Filipstad in the Viking land of the northern mines of Sweden, within six degrees of the Arctic Circle. These were primitive surroundings, where men looked inward for their strength and reliance, and where in the deep silences of long nights to dream was instinctive.

Where other children played with toys, he was fascinated by machines. When he was only seven he haunted the drafting room of the great Gotha Canal where his father was a foreman. There was no money to supply him with proper instruments for mechanical drawing, so he constructed them himself. He made a compass out of pine twigs and inserted needles; a drawing pen from tweezers sharpened to a point, controlling the thickness of the line by a thread which could be slipped up and down. To color his drawings he mixed his own pigments. He made two small brushes from hairs discreetly plucked from his mother's sable coat. With these tools the child sat long hours at his drawing board, drawing to scale. At the age of nine he was ready for the first great adventure.

He determined to construct a model of a sawmill. He had never seen one. All he had to guide him were descriptions supplied by his father. Nevertheless the young engineer drew his plans and made his precise calculations. He had only a gimlet, a jacknife, and a file. It was not a boyhood toy he evolved but a complete working model. The wooden frame was constructed to a calculated scale. For a saw, he took a watch spring and filed out the teeth. Everything was complete in miniature; the bed to carry the logs, moved by a cord wound around a drum, the ratchet wheel and lever to turn the drum, the crankshaft, and a lever fashioned out of a broken spoon. When it was completed to the last detail he hurried to a brook to put it to the test. Would it work? Had he missed one necessary detail? In all the after years, even at the height of his fame, probably no moment ever exceeded the thrill that came to the boy of nine as the wheels turned with the current and every detail functioned in perfect harmony.

The next year, at the age of ten, he constructed an even more complicated model, a pumping engine to draw water from a mine, the motive force supplied by a windmill. To the original model he later added a ball-and-socket joint to adapt it to the shifting of the wind. As with the sawmill, all the descriptions had been supplied him by his father. Both models had been so expertly planned and constructed that they needed only to be reproduced on a larger scale to function practically.

Years later, when Ericsson was asked for a list of his more important mechanical achievements, these boyhood models headed his list. He made no mistake in his estimation of their importance to his career. From these early victories, he acquired an unshakable faith in himself.

A legend began to form about him.

Men in high places interested themselves in his future. Before he was 14 he was employed as a leveler in the construction of the Gotha Canal. Six hundred Swedish soldiers worked under the direction of a boy still so small that he needed a stool to reach the eyepiece of his leveling instrument.

Meanwhile he continued his self-education. He never attended school or college, but he absorbed knowledge hungrily from anyone who could help him. Down to the last detail all his inventions were planned and drawn out by himself. Without this ability as a master draftsman he could never have constructed his first locomotive in seven weeks or accomplished the amazing feat of building the *Monitor* in 100 days from the laying of the keel.

During this period, always interested in the theories of motion, he became fascinated by the obliquely applied stroke of the bird's wing and the propelling power that lies in the tail of the fish. From these observations of nature's forces he evolved his conception of motive power applied through a screw propeller.

A second observation affected the conception of the *Monitor*. He watched the floating rafts of logs on the lakes and noticed their unusual stability in storms which overturned or sank other boats. From this memory he evolved the low flat deck of the *Monitor*—which was in fact what it was derisively dubbed, "a cheesebox on a raft."

So ended a brilliant and precocious youth. No one who knew him had any doubt of the great things he would accomplish. Yet for almost 30 years, despite a series of brilliant accomplishments, the full measure of success was denied him.

Ericsson's intention was to establish himself permanently in England; without two dramatic failures he would never have sought the shores of America. He had not succeeded in an early attempt to introduce a flame machine which, though it had worked satisfactorily under wood fueling, was not adapted to the use of coke. His prolific imagination, however, was turning out four or five important mechanical inventions a year. When he was 26, after he had been in London three years, there occurred the first dramatic test of his career.

Three-quarters of a century after Watts' experiments with steam, only a few crude locomotives had been built. They were used to draw heavy freight for short distances at no more than three to four miles an hour. After years of hesitation the Liverpool and Manchester Company succeeded in securing a charter to build a railroad. It immediately announced a prize of £500 for a locomotive which must attain the speed of ten miles an hour, weigh no more than ten tons and draw twenty tons seventy miles. Ericsson learned of the contest when but seven weeks of the allotted five months remained. He had never built a locomotive, and he would not have time to test it. Undaunted, he began work at once.

The contest took place over a two-mile stretch of railroad, and it was necessary to make twenty round trips. There were five entries but the contest narrowed down to George Stephenson's "Rocket" and Ericsson's "Novelty." Ericsson was supremely confident, for he had equipped his engine with a forced draft to produce high speed and had ingeniously suspended it on springs so that it held the track without the slightest swaying motion.

It did develop a speed of well over thirty miles an hour—the fastest that any human being had ever moved! On the first day of the trial the "Novelty" shot by the "Rocket" like a projectile. For one dramatic moment the young engineer felt the thrill of a triumph that would have made him the greatest figure in the mechanical world. But on the third trip the untested boiler proved too

weak to contain the great power of the steam generated. It was a defect that could have been easily remedied.

But there was no going back on the decision of the judges. Stephenson and not Ericsson became the father of the modern locomotive. For his successful rival, Ericsson retained a high esteem. And philosophically he came to regard his own defeat as all for the best. Had he reached world fame at the age of 26, he said, he probably would have created nothing further. As it was, he turned to a new field.

ERICSSON saw clearly the limitations of the paddle-wheel steamer. For naval combat is was obviously vulnerable. For commercial purposes it was cumbersome and limited in speed. It could move directly into the wind, it is true, but even under favorable circumstances it could develop a speed of only four to six knots. Childhood observations on the movement of birds and fish remained in his memory. Gradually he perfected the principle of the screw propeller. Other minds in Austria, France, and the United States had played with the same idea, but Ericsson was the first engineer with the knowledge and experience to bring a theory into practical realization. The screw propeller, as developed by him, revolutionized the ocean commerce of the world. Yet for years, even after the first demonstration of its efficiency, it was fiercely fought as both impractical and visionary. It is difficult today to realize the opposition to new ideas against which that generation of great engineers had to contend. The explanation, however, is simple. A revolutionary idea is not only a creative force, it is essentially a destructive one. For that reason every world-transforming invention has had to contend against the vested economic order—both capital and labor.

Ericsson was now ready a second time to challenge the old order, and a second time he met with defeat. In 1837, at the age of 34, he was ready to demonstrate his screw propeller to an incredulous world. This time he was absolutely confident of success. He had had the time he needed to complete every test. To the last detail he had guarded against the possibility of failure.

He invited the Lords of the Admiralty to the first public demonstration. He had installed below the water line in the stern of the *Francis B. Ogden,* a vessel of 45 feet in length, his twin screw propellers. Already the boatmen of the Thames had been so mystified at the sight of this steamer moving swiftly and mysteriously along the river that they had named it "The Flying Devil." On the appointed day Ericsson repaired to where the Lords of the Admiralty and a distinguished company were waiting. Unfortunately they did not come with open minds; their verdict was already established. They had listened to the engineering corps of the nation, which was arrayed unanimously against this ridiculous invention constructed "on erroneous principles and full of practical defects."

The *Ogden* effortlessly and smoothly, moved by the unseen propellers, towed a barge up the river and back without in the least shaking their convictions. They disembarked with a few perfunctory expressions of thanks for the "interesting" experiment. Not one had the slightest suspicion that he had taken part in the first successful demonstration of a new motive principle destined to revolutionize sea power and relegate to oblivion the ponderous side-wheel steamers they were so obstinately defending. And the reason for the rejection? It was solemnly pronounced that "if the power was applied at the stern it would be absolutely impossible to make a vessel steer!" A second time Ericsson had stood on the brink of fame and seen it denied him.

Ericsson now went through a period of such utter poverty that he was actu-

ally imprisoned in Fleet Street, the debtors' prison. Completely discouraged, he abandoned the country which had twice misunderstood him and sought his career in a land that seemed to welcome new ideas. This was in 1839 and he was now 36. Had the result been different, his luck in England better, Ericsson undoubtedly would have remained there at the top of his profession, and the *Merrimac* without opposition would have swept the sea and ravaged the ports of the North. On such obscure vicissitudes are the destinies of men and nations fashioned.

Ericsson came to America with the highest hopes. Americans had recognized the value of the screw propeller. Americans had insisted on the opportunities that lay before him in the New World. He believed that his troubles were now over. Unfortunately they were just to begin. His mind was so prolific that he was able to establish some financial independence by the invention of a steam fire engine and a hot-air or "caloric" engine.

But in the development of the screw propeller for naval vessels, on which his heart was set, he met only with discouragement and disappointment. It was five years before the Naval Board was willing to make a test. When the *Princeton,* the first man-of-war to be equipped with a screw propeller, was finally contracted for, it was done under such humiliating conditions that Ericsson felt he had been betrayed. His contribution was minimized. Even the payment for his services was disputed by the Government and was never made. And on a trial trip, through no fault of Ericsson's, one of the *Princeton's* big guns exploded and killed the Secretary of State, the Secretary of the Navy, and other high officials gathered to witness its triumph.

For almost twenty years defeat, discouragement, and adversity pursued him. Even the credit for his invention was for a long time denied him. A new venture into his favorite field of the hot-air engine ended in a fantastic disaster. The ship equipped with the new engine was struck by a tornado on its trial trip and sank. Pecuniary difficulties beset him a second time, and he lacked the commercial sense to capitalize on his inventions. Yielding to a momentary discouragement, he now considered abandoning the struggle in America and returning to Europe. Fortunately for the North he reconsidered and remained to play his decisive part in the history of his adopted country.

When the fateful year of 1862 arrived Ericsson was still struggling for recognition of his greatest achievement. He was beset with litigation over his claim to fame as the inventor of a screw propeller. Imitators abroad copied his mechanical devices or openly stole them. The invention in which billions are invested today brought him no change in fortune. A few men recognized him as the engineering genius of the time, but outside these he was still meeting hostility in his own profession, and a large section of the scientific world condemned him as a mountebank and a visionary.

It was known that the Confederates were completing the construction of the ironclad *Merrimac,* and yet nothing had been done in the stagnant naval circles at Washington to meet the threat of destruction of the wooden ships of the Federal Navy. Fortunately the *Monitor* did not have to be created. The plans had been perfected for years. As far back as 1854, Ericsson had offered to Napoleon III a completed model which, with the exception of one unimportant detail, was the exact working model of the *Monitor.* In it he had perfected his conception of an ironclad ship where all vital parts would be below the water line. It had a flat raft-like deck with the firing power concentrated in a revolving tur-

ret, which would permit the guns to be reloaded in security instead of being exposed to enemy fire.

Despite the shabby treatment he had received at the hands of the Government in the matter of compensation for his work on the *Princeton,* Ericsson loved the country of democracy. He wrote to Lincoln offering his services, stating that he sought "no private advantages or emolument of any kind." He maintained this attitude to the end.

He presented before the Naval Board at Washington a replica of the model which had been offered to Napoleon III. Lincoln was impressed and threw his weight in its favor. The opposition remained violent and unconvinced. One member scornfully remarked: "Take the little thing home and worship it, as it will not be idolatry because it is in the image of nothing in heaven above or on earth beneath or in the waters under the earth!" The contract had been actually rejected, when Ericsson appeared before the full board and made such a convincing exposition that the decision was reversed. But the Navy insisted on inserting a clause guaranteeing refund to the Government of all money paid out in case the vessel proved a failure.

The *Monitor* was built in 100 days from the laying of the keel. It was accomplished despite tardy payments from the Government (the battle of the *Monitor* and the *Merrimac* was fought before the final payment was received), despite continuing criticism and interference of naval authorities.

Ericsson, the old Viking, refused to give an inch. He made the drawings himself, answered every criticism personally, fought off all interference and delivered the vessel on time. Inside the ship were more than forty patentable devices which he had turned out in the full flush of his genius. At the first trial a slight error developed in the steering apparatus. The naval authorities, alarmed, advised putting the ship in drydock and fitting a new rudder. Thereupon Ericsson exploded.

"They would waste a month in doing that. I'll make it steer in three days." He did it in less than that and the *Monitor* arrived at Hampton Roads in the nick of time to fulfill her rendezvous with destiny.

Despite timidity in Washington, which insisted that the *Monitor* fight a defensive battle and would not permit it to pursue the wounded *Merrimac;* despite failure to use the wrought-iron shot which Ericsson insisted would have penetrated the armor of the enemy; despite the plan of battle, which was fought at a distance contrary to the directions of Ericsson, there was no question of where the victory lay. The first shots from the *Monitor* had in truth sunk the wooden navies of the world. The commander of the *Merrimac* himself testified before a naval court that the *Monitor* should have sunk his vessel in fifteen minutes. Later he added, "Ericsson is a great genius."

World recognition now came to Ericsson at the age of 59. Without the dramatic appearance of the *Monitor,* the ironclads of the Confederate navy would have mowed down the wooden battleships of the Union, devastated the seaports, lifted the blockade, and quite possibly have secured European recognition of the Confederacy. And all this depended on the vicissitudes in the life of one man, on two outstanding failures, without which he would never have been in America to emerge as the instrument of Providence.

The revolving turret in every battleship today is a tribute to the memory of Ericsson. His principle of screw propulsion still remains as the central principle of motive force, not only on the sea but in the skies themselves. The *Monitor* wrote his name indelibly in American history, but the screw propeller places him in the ranks of the great pathfinders who have affected the history of civilization.

RUBBER, HERITAGE OF THE AMERICAN TROPICS

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SHORTLY after 1510 Pietro Martyre d'Anghiera, chaplain in the court of Ferdinand and Isabella, wrote the preface to a still unfolding chronicle, relating in impressive Latin how the Aztecs played with bouncing balls made "from the juice of a certain herb." There are few products whose history reflects the recent changing facets of developing civilization as vividly as does that of the substance of those Aztec balls. Rubber is a constituent of latex, a milky substance formed in specialized vessels or cells in many different plants of widely separated families. In spite of a good deal of research, its role in the growth and life of the plant which bears it remains a mystery. To man, uses for rubber are multitudinous and, in both peace and war, highly essential. The development and expansion of these uses mirror faithfully the effects of the industrial revolution. The Indian discoverers utilized the substance in several ways; the conquering whites added a few others. Before 1600 the Spaniards in Mexico were dipping capes into latex to waterproof them. But nobody then really got excited about its potentialities. It took the age of machines and motion to give prominence to the stuff.

Rubber's first effective press agents were Charles-Marie de La Condamine, an exploring scholar sent forth by the Paris Academy of Sciences to make a meridian measurement on the Equator in an attempt to settle whether the earth was a flattened or elongated sphere, and his friend, François Fresneau. These two men furnished Europe with descriptions of the uses and preparation of rubber, samples of the material, and details of the botanical characteristics of the trees tapped by the Indians. La Condamine made important early observations on both rubber trees and the cinchona tree, source of quinine. Ever since, the products of the two trees have been strangely parallel in development. Both came to occupy important places in the list of man's needs; the two industries were transported to the East to be greatly expanded and lately conquered by Japan; the war years have seen heroic attempts to re-establish both in the Western Hemisphere and rapid strides in the synthesis of substitute products.

Rubber's advertising campaign really got under way about the middle of the nineteenth century. Priestley had named the stuff *India rubber* in 1770 after discovering that his sample, actually from India, would rub out pencil marks. In 1823, Charles McIntosh found a solvent for rubber in naphtha, thus making manufacturing a real possibility. Previously only fresh latex could be used, and the factories had to be almost under the trees which produced it. Still, however, one's boots and coat cracked in the winter and oozed and stuck in the summer. Then, in 1839, Charles Goodyear mixed rubber, sulphur, and heat and invented the vulcanizing process. The cornerstone was laid. New uses were developed and demands rose. More plants were sampled; great areas were explored.

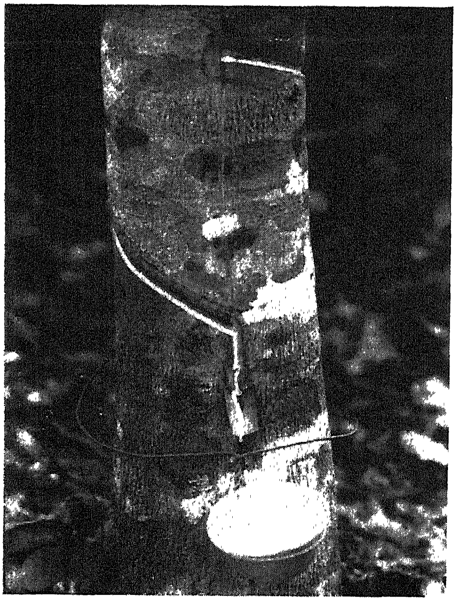
Latex production is a characteristic of hundreds of plant species. The number whose latex contains rubber of acceptable quality and in significant amounts is much smaller. Discovery of this fact

soon focused the main efforts of the wild-rubber collecting industry on the Central American rubber tree, *Castilla elastica,* and other species of *Castilla* from Mexico to Bolivia; the Pará rubber tree, *Hevea brasiliensis,* and other species, particularly *H. benthamiana* and *H. guianensis,* in the Amazon region; the Ceará rubber tree, *Manihot glaziovii,* and species of *Sapium* in Brazil; Kickxia, *Funtumia elastica,* and various species of *Landolphia, Clitandra,* and *Carpodinus* in Africa; and *Ficus elastica* in India, Burma, and the Malay Archipelago. The most rubber and that of the highest quality came from the Pará rubber tree, and the Amazon region gained first place among rubber-producing areas. In 1827 rubber was collected only around Pará, enough to make an export total of 31 tons. In 1856 Brazil exported 2,607 tons. Credit Goodyear's vulcanizing process. The increase was steady until 1912, when a peak Amazonian production of 45,067 tons was reached. The saga of exploitation of trees and men involved in the expansion is a fantastic one depicting remarkable strides against jungle country, great fortunes built in a year or two, ravaging tropical diseases, and decimation of whole populations. In wealth, glory, and blood it is matched only by the Congo rubber operations, which supported a Belgian empire. The decreases in yields caused by overcutting of Hevea during tapping and the actual destruction of Castilla by the felling method of tapping rapidly lowered the output of certain areas and aroused grave concern over future supplies of this increasingly important commodity.

In an attempt to find ways of alleviating the predicted shortage, President McKinley, who had been a Congressman from the Ohio district including Akron, suggested to Congress in 1899 that rubber planting be undertaken in the Philippines, Hawaii, and Puerto Rico. The President was giving expression to the oft-recurring hope that we might bring at least a considerable part of the Nation's rubber supply under our own control. Inadvertently he gave incentive to a great gambling game in which many of the bets were fraudulent and most of the rest were on the wrong tree. In the decade following delivery of the President's advice, New York school teachers, Chicago policemen and letter carriers, and a host of other Americans, hopeful or just gullible, invested $75,000,000 in real or imaginary rubber plantations. The real ones covered large areas in Mexico and Central America and actually contained more than 30,000,000 trees, nearly all, as it turned out, the wrong species. Except for a few small plots, the plantings were Castilla, native to the area. As a rubber producer Castilla is second rate. Its rubber, as commonly collected and prepared, is inferior in quality to that of Hevea, and the trees can be tapped only two or three times a year; thus, while the yield per tapping is considerably above that of Hevea, the annual yield per tree is but a pound or two as against 5 to 10 or even 25 pounds for selected Hevea. Nevertheless, a highly significant proportion of the world's rubber came from Castilla during the early years of this century when the plantation rubber industry of the Far East was in its infancy. Some of these plantings still survive, and a few have yielded a little rubber now and then, but their total production has been inconsequential.

Meanwhile, the British had stolen the march. Spurred by Thomas Hancock and other rubber manufacturers and by the desirability of developing their vast tropical holdings, there rose an intense interest in attempting to establish plantations of Hevea, whose high yield of excellent rubber was bringing wealth to Brazil, in tropical areas under Her Britannic Majesty's flag. The prime

figures in bringing action in the hemisphere exchange were Sir Clements Markham, a geographer and India office official who had engineered the transfer of cinchona from Peru to India and later was to be rewarded with a knighthood; James Collins whom Markham commissioned to study the rubber situation; Robert Cross, one of Markham's quinine explorers; Henry Wickham, an enterprising Amazon planter; and Sir Joseph Hooker, foresighted director of Kew Botanical Gardens. The results of Collins' survey of the situation, containing pertinent observations of Richard Spruce, another quinine explorer engaged in botanical collecting for Kew, were published in 1872. The next year Collins bought 2,000 rubber seeds for $27 and shipped them to Kew. Spoilage of the large, oily seed was great; out of the lot only a dozen germinated. Half were kept as specimens at Kew; the rest were dispatched to the Royal Botanic Gardens in Calcutta, where despite the staff's best efforts they promptly died. In 1875 Wickham made two unsuccessful shipments of seed to Kew. The following year this practical-minded gentleman resorted to more extreme measures and, as Bekkedahl pointed out in his article on *Brazil's Research for Increased Rubber Production,* which appeared in the September 1945 issue of THE SCIENTIFIC MONTHLY, succeeded. It was the season of the Hevea seed fall, and the liner *Amazonas* of the newly inaugurated Liverpool-Amazon service lay in the upper river without a return cargo. That was too good an opportunity to pass up, and Wickham chartered the boat and set off up the Tapajós to collect all the seed he could. Thoroughly familiar by now with the perishable nature of the seed, he treated them with tender care, finally packing them in openwork baskets between layers of dried banana leaves. The baskets were carried in canoes to the *Amazonas* waiting at the junction of the Tapajós and the Amazon. Once on board Wickham began to worry about the possibility of delays in obtaining port clearance at Pará. Brazil's neighbors had been greatly incensed at the cinchona snatch, and Brazilian officialdom was reputedly much concerned over the attempts to repeat the deal with Hevea, its near-monopoly crop. Though Brazil had no law prohibiting the export of seed, Wickham supposed that he might encounter delay with what he now knew to be a spoilable cargo. To circumvent the possible difficulty he listed his shipment not as Hevea seed but as delicate botanical specimens consigned to the Royal Botanic Gardens. No delay was encountered, and he was off on a speedy voyage. At Kew the seed germinated well enough to give about 2,700 plants. Markham and Hooker decided to attempt to establish the seedlings around Tenasserim in southern Burma, but economic considerations led them to change their plans and dispatch most of them to the Botanic Garden at Ceylon. Meanwhile, Cross had started to Brazil to collect more seed, which was likewise sent to Kew for germination. These seedlings then also went to the East. Out of these early collections of Wickham and Cross, seedlings were sent to Ceylon, Java, Singapore, and Perak. Later, more seedling shipments and distribution of seed from the first importations spread trees through Malaya, Borneo, Burma, and parts of India and even the West Indies. The introduction was followed by three decades of observation and experimentation and relatively little further planting. During this period *Ficus elastica* was planted instead of Hevea, and opinion still generally favored use of this species. However, important advances were made in the development of cultural practices and especially tapping methods. The newly developed tapping, based on the so-called "wound response"



EXPERIMENTAL TAPPING
RUBBER-YIELDING CAPACITY OF HEVEA TREES IS DETERMINED BY TEST-TAPPING YOUNG TREES.

of the trees, brought Hevea's superiority to light. Important, too, was introduction of the use of acetic acid in coagulating rubber in the latex, a great advance over the primitive smoking method of the Western jungle natives. Restricted probing of various areas revealed many possessing the conditions essential for Hevea cultivation, deep soil, friable and fairly rich, and a warm climate, rather consistently moist with a rainfall in excess of 70 inches a year and without destructive high winds. By 1900 the stage-set was pretty well worked out. The curtain rise came shortly. Brazil began economic maneuverings designed to increase output and price. By 1905 the price hit $1.50 a pound. The British and Dutch responded by pouring capital, cautiously at first and then in wildcat fashion, into plantation development in their Eastern holdings. Thousands of failing coffee plantations in Ceylon and Java were quickly converted to rubber. When the price should have come down it continued to rise, for the automobile, the most important factor in the whole rubber picture, was on its way. By 1910 the American output of automobiles had reached 180,000 a year. Rubber production had risen to 70,500 long tons, 8,200 from the new plantations. Buyers paid an average of $2 a pound and once had to go to $2.88. Fortunes were made. More plantations were established. By 1914 plantation production exceeded wild collections, and at about the same time production came nearer in line with demand. The price began to decline. With falling prices the plantation system and its controlled organization had all the advantages when it came to cutting costs. The result was that the plantation growers captured and held the lion's share of the market while Brazil's native rubber industry nearly vanished. World War I and its aftereffects temporarily revived it, but by the early 1930's it had sunk to less than 10,000 tons a year, out of a world total of around 1,000,000. In 1941, 97 percent of the production of 1,527,820 tons came from the Eastern plantations, while wild rubber from South America, Africa, and all other sources, including Mexico's guayule, made up only about 45,000 tons.

The plantations then totaled about 8.5 million acres, about three-eighths of them in Malaya, a slightly smaller acreage in the Dutch East Indies, the remainder distributed in order of acreages in Ceylon, French Indo-China, Siam, Sarawak, British North Borneo, India, and Burma, the core of the empire coveted by the Japanese.

The concentration of all but a minor fraction of the world's supply in the Far East has always had serious implications for the United States, by far the rubber planter's best customer. Despite rapidly increasing demand for the product, the potential supply began, shortly after World War I, to exceed the demand. The economic consequences assumed

grave proportions. In an attempt to correct the situation, the British in 1922 put into effect a restriction scheme known as the "Stevenson Plan." Abandoned in 1928 because it did not include the Dutch and was not flexible enough to cope with rapid changes in a complex market, the plan nonetheless accomplished some of its purposes. One was a sharp increase in the price of rubber. An immediate result was curtailment of expansion of uses in this country and the extension of the use of reclaimed rubber. Congress investigated the situation and representations were made to the British, without avail. An unintended effect of the operation of the plan was to stimulate production in the Netherlands Indies and other non-British areas. Eventually the picture of the restrictionless Dutch waxing prosperous by virtue of self-inflicted British control and several other factors led to termination of the plan. From 1928 to 1934 production was without regulation. During this period the depression sent the price into its worst tumble (2.7 cents a pound in New York in 1932), and the British and Dutch jointly put into effect the International Rubber Regulation Agreement which controlled export and production of rubber from June 1934 until its expiration in April 1944. This scheme was more flexible than its predecessor and did cover all the major producing areas. How much it hindered normal improvement of the industry would be hard to estimate. It did keep high the price which American manufacturers had to pay, and it did restrict, through inflexibility, the amount of rubber which the United States could obtain in the critical days of 1939 and 1940.

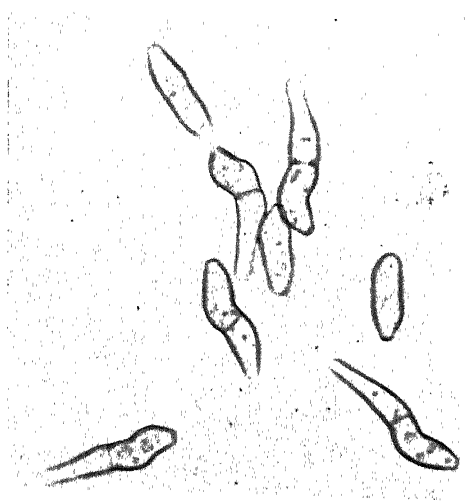
The economic disadvantages to the American public of the tight British-Dutch control, as it has been exercised, have been great. The military loss of the far-away plantation areas to the enemy came near to being tragic. During the unprecedented demand period of World War II only the Ceylon and India plantings, comprising about one-twelfth of the Eastern total, remained as sources for the Allies.

Catastrophe was averted by the tremendous development of synthetic rubber production and the tapping of every available source of natural rubber. Attention has been redirected to the resources of the American Tropics. Large sums have been spent in stimulating the collection of wild rubber from the jungle trees, and Brazil's once great industry has, for the moment at least, been to some extent revived. The revival is a war measure, though, and little more can be expected from it. After all, the annual output of Amazonia in its heyday was only about 45,000 tons, hardly significant any more. Far more important in the ultimate is an attempt to stimulate rubber plantings in the Western Hemisphere, a wartime measure with long postwar sights. In the earlier attempts to establish plantations in this hemisphere there were a few, in Trinidad and the British and Dutch Guianas, in which Hevea was used extensively. Most of these trees failed to grow well or to yield any quantity of latex, because of heavy infections of South American leaf blight, a disease caused by a fungus, *Dothidella ulei*. The relative political instability of the governments of the period was another major factor in dampening interest in the development of Hevea plantings in several of the Central American countries where small areas were more or less experimentally planted to the tree.

In 1928 Henry Ford, following the example of Harvey Firestone in attempting to build up his own source of rubber in Liberia, began operations for the development of part of a 2,500,000-acre concession up the Rio Tapajós in Brazil

EFFECT OF SOUTH AMERICAN LEAF BLIGHT ON YOUNG TREES
Left, LEAVES OF THE RESISTANT STRAIN. *Right,* LEAVES OF A HIGHLY SUSCEPTIBLE STRAIN.

as a Hevea rubber plantation. The early difficulties of the Ford plantations were described by Bekkedahl in the article referred to above. The worst was leaf blight, the inroads of which were terrific. Whole areas were wiped out. In 1935 the Goodyear Tire & Rubber Co. started plantations in Panama and the next year extended them into Costa Rica. Goodyear had initiated a campaign a few years before to bring some rubber cultivation to the Western Hemisphere, establishing plantings in



CONIDIA OF DOTHIDELLA ULEI
SOUTH AMERICAN LEAF BLIGHT, SPREAD MAINLY BY CONIDIA, IS MOST HARMFUL IN WET SEASONS.

the Philippines as the first of the homeward steps. In Goodyear's tropical American plantations, as in Ford's, leaf blight was destructively rampant. These discouraging early ventures not only emphasized the magnitude and severity of the disease problem but also pointed a way to circumventing it by showing up certain disease-resistant stocks and demonstrating that, except for the leaf blight, Hevea lends itself to cultivation as well in the West as in the East.

When the progress of the war in Europe began to highlight the distance between Akron and Singapore, one of the foci of attention landed upon the plantation possibilities of Latin America. Consultations between the United States Departments of State and Agriculture and the governments of the tropical republics to the south revealed anxious interest on the part of many of the potential rubber-growing countries but a lack of technical knowledge as to how or where to begin.

In 1940 Congress paid heed to the shadows of the rapidly approaching crisis and the interest of our tropical neighbors by appropriating funds for a cooperative program directed toward encouragement of a rubber-producing industry in the American republics. The first step was a series of surveys of

the supposedly suitable areas. Exploring parties were constituted of technicians from the United States Department of Agriculture, most of them with experience in the rubber areas of the East or on other tropical crops, and Latin American plant and soils scientists familiar with local conditions. Where within the area could plantations be developed and how far beyond the natural range of Hevea could they be taken were questions for the surveys to decide. Beyond the fundamental necessity of soil, moisture, and the other qualities of environment suited to vigorous growth of the plant were the two major considerations of the widespread South American leaf blight and the economic aspect involving, in turn, the prospects of a market at an adequate price, availability of labor, and a host of other factors.

The limiting disease, South American leaf blight, is spread throughout Amazonia and well up into Central America. In the wild its effects are relatively moderate, largely because native Hevea trees generally occur sparsely over wide areas, and between them are barriers of other nonsusceptible species. In the much denser stands of plantations and nurseries, the disease defoliates trees with amazing swiftness and disastrous effects. Thanks mostly to plant pathologists of the United States Department of Agriculture and the vitally interested Goodyear Rubber Plantations Co., new spraying methods and adaptations of older standard methods now make it possible to control the disease adequately in nurseries, mostly by the use of copper compounds. In plantations of mature trees effective spraying would be mechanically difficult and prohibitive in cost. As with most diseases the greatest hope is immunity or relatively high resistance to infection. Freedom of certain trees

RESISTANT SEEDLINGS

THESE PLANTS WERE NURSERY-GROWN FROM SEED COLLECTED IN THE ACRE TERRITORY OF BRAZIL.

from infection was observed in the early Guiana plantations. The Ford and Goodyear experiments have segregated several highly resistant strains. Most of those tested, however, are low yielding, and it is a basic premise that if new plantings of rubber are to succeed they must at least equal in yield the best Eastern ones. Superior trees are being produced by crossing Eastern strains, carefully selected over a period of many years for high yield, with disease-resistant Amazon strains. Outstanding jungle trees in Brazil, Peru, and Colombia are being studied further as possible commercial material. Planting material from such selection and breeding is amassed slowly, and successful establishment of a Latin American rubber industry depends upon immediate commercial planting to take advantage of the expanded market. To hasten production, resort is being had to an expedient used by the Ford Plantations, i.e., double budding, a procedure of which Bekkedahl has given a detailed description. Rubber planters, like many other tree growers, have made extensive use of bud-grafting superior strains onto run-of-the-wild rootstocks. The result is a uniform stand of superior trees. In 1926, a man named Cramer, in Java, interested in the possibilities of increasing yields and controlling certain local diseases, suggested improving upon nature by double budding to make a tree of three strains, root, trunk, and top, each selected for superiority under a given set of conditions. Various factors of compatibility, size relations, and developmental pattern are concerned, but several successful combinations have

DISEASE CONTROL IN A COSTA RICAN HEVEA NURSERY

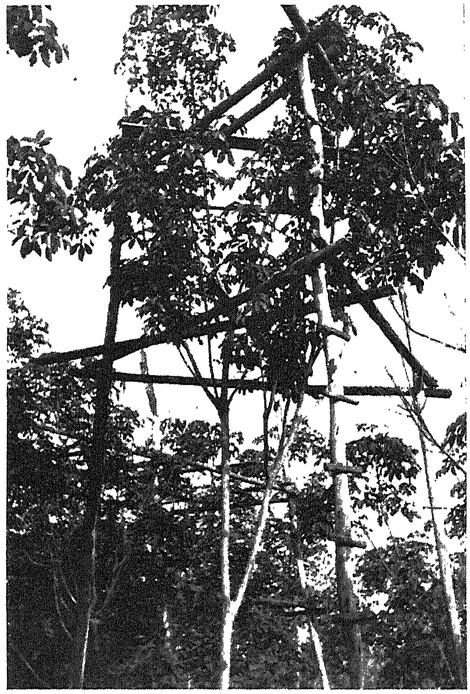
EASTERN-STRAIN SEEDLINGS ARE BEING PROTECTED AGAINST THE LEAF BLIGHT BY SPRAYING.

been made and such three-part trees are being planted in the disease areas. In practice high-yielding Eastern clones are budded on native seedling rootstocks just above the ground level. The trees are then protected by spraying until they have grown enough to permit top budding at 6 to 8 feet from the ground with a disease-resistant strain or species. Use is also being made of top budding to speed up the breeding program, for it has been discovered that buds implanted in old trees will grow, mature, and flower rapidly and profusely.

It can now be said with assurance that the disease to which the earlier tropical American Pará rubber plantations succumbed is well understood and is readily brought under control.

To fit the pattern of Latin American agriculture and take greatest advantage of the economic factors, emphasis is being placed on plantings small enough to be handled adequately by a single farmer or single family. More than half the Eastern rubber is supplied by such enterprises. Such plantings obviate the necessity for large movements and organizations of labor, and require relatively little capital. Further, they provide urgently needed diversification, and most of the areas where Hevea can be grown have been cursed by all the hazards of one-crop farming, mainly too much coffee or bananas.

The wartime implications of this encouragement of Hevea planting in the West were many. It represented assurance of expanding the supply of natural rubber if the period necessary to retake the Eastern areas had been prolonged beyond the time the first plantings mature. It has given the governments and peoples of tropical America a sense of direct participation in both the war effort and the basic planning for postwar economy, along with an appreciation of earnest effort on our part to assist them to an active place in the world.



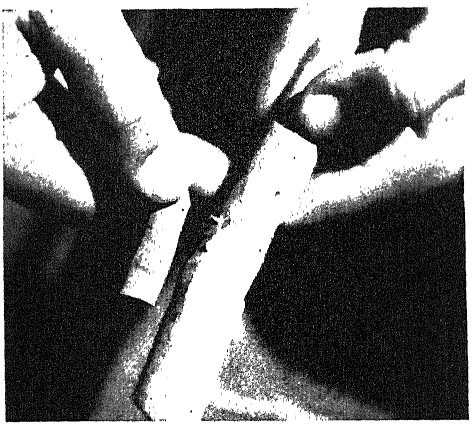
HYBRIDIZATION
BLIGHT-RESISTANT TREE BEING USED IN CROSSING TO COMBINE RESISTANCE WITH HIGH YIELD.

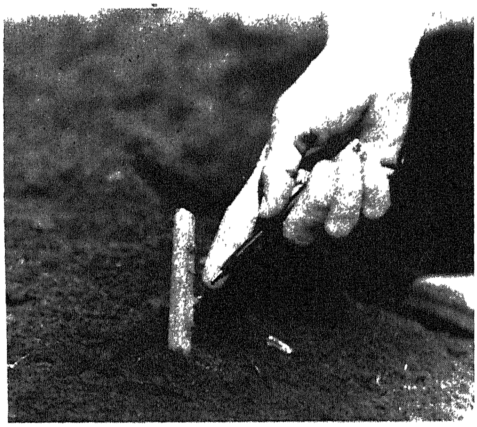
The war did much to speed up industrialization of Latin America. The postwar years undoubtedly will see more highway building, partly as a result of continued industrial and agricultural expansion in attempts to raise living standards and partly as a bid for American tourists. The total effect of this and other developments will be a sharp rise in domestic rubber requirements which, in many countries, were already becoming higher before the war. Yet any increase would take only a small proportion of the potential producing capacity. In Mexico, for instance, 25,000 acres of selected Hevea would furnish four times the 1941 imports. A rubber supply within the borders of the smaller nations is insurance of obtainability of this increasingly vital material.

For various reasons rural population shifts are important in the current

development of many of the tropical American countries. In Mexico the *ejidal* system of communal farming is part of an agrarian reform which is a pillar of the present government. In Guatemala overproduction of coffee and consequent decline of the market have stranded a large agricultural population. In Honduras, Nicaragua, and Costa Rica the inroads of the Panama and Sigatoka diseases have cut banana production very sharply and left groups of small farmers without a cash crop. Colombia is undertaking an immense colonization scheme in its rich tropical northwest territory. Brazil moved about 26,000 rubber workers and their families into the Amazon Valley to bring out wild rubber during the war, and several other countries have population problems in areas admirably suited to Hevea. Rubber represents one of the most hopeful solutions for the problems of these stranded populations and colonizing operations. Planted over small acreages with subsistence food crops, it will provide cash income to the individual farmers or government-settled colonists. Rubber produced in this manner is less subject to labor cost factors, since its selling price tends to be measured in terms of the urgency of the cash requirements of the grower rather than against national labor wage rates.

The question of production costs is, of course, a controlling one. Experience to date indicates that Latin American rubber produced in either small or large plantings can compete successfully in the market at or even considerably below the prewar level of 16 to 22 cents a pound. Any Latin American disadvantage in labor costs may be compensated for by increased yields and reduced shipping charges. Replacement of the already established Eastern plantings with higher yielding selected clones is a task that will take decades to complete. As it progresses, however, high-cost marginal producers will be eliminated.





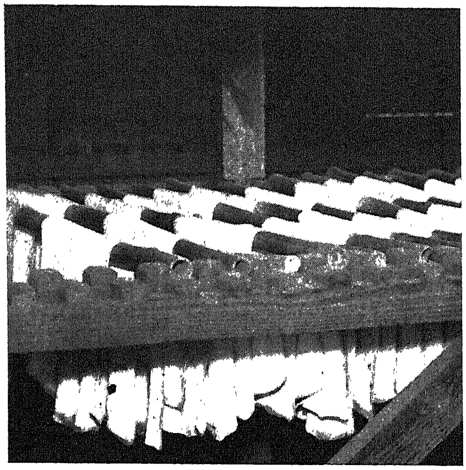
BUDDING INSURES SUPERIOR TREES

Left, REMOVING A BUDDING-PATCH FROM STICK OF HIGH-YIELDING HEVEA BUDWOOD. *Right,* SELECTED BUD STARTS TO GROW AFTER INSERTION. SEVERAL KINDS OF BUDDING ARE DONE.

So far, synthetic rubber has not been produced at prices which would permit it to compete, unaided by some subsidization, with natural rubber. That the almost miraculous development of the synthetic industry saved the war effort is obvious, as is also the fact that a large synthetic industry is here to stay—this last because in many uses synthetics are superior to natural rubber and are a military precaution and a safeguard

against unfavorable price manipulations by the Eastern rubber interests.

Synthetic alone, however, is not enough. Wartime experience has indicated the manifest superiority of natural rubber over any of the synthetic substitutes as far as the manufacture of certain products is concerned. Further, the development of natural rubbers with particular outstanding qualities by selection or treatment is a field which as yet has hardly been touched. The rubber market will definitely depend in the future upon both natural and synthetic. It is estimated that for some years after the war consumption can absorb at least 1,500,000 tons of rubber, natural and synthetic combined. No estimate can be made at the present of the balance which will be struck eventually between natural and synthetic. But no one doubts that, regardless of what the balance is, large stocks of natural rubber will have to be available at all times. Dependable supplies of Latin American rubber have two extremely important advantages for the United States. As a noncompetitive crop sold to us rubber will build up dollar exchange, and Latin American purchases are an important part of our foreign trade. Further, tropical American plantings will provide a strategically located and readily accessible supply available in an emergency without the



LATIN AMERICAN RUBBER
SHEETS HANGING IN THE SMOKEHOUSE OF THE GOODYEAR SPEEDWAY ESTATE IN COSTA RICA.

heavy cost attendant to storing a minimum supply.

As an example of international collaboration, this attempt to bring some of the Hevea rubber industry home is a hopeful example. Initiated by mutual interest and developed by the Latin American countries themselves at their own expense, with scientific and technical assistance and advice from the United States, toward a mutually advantageous goal, the program is assisting materially in cementing good neighborliness and building toward hemispheric solidarity.

RESEARCH ON PHENOTHIAZINE AS AN ANTHELMINTIC

By PAUL D. HARWOOD

SOME insist that practical or economic considerations are so much smoke in the eyes of the scientist, vastly limiting his chances of increasing human knowledge, while others claim that any research having no practical end in view is evidence of so much wasted energy. The history of the research on phenothiazine as an anthelmintic, or worm killer, is perhaps illustrative since our studies have been confined largely by an economic smoke-screen. I wish to call attention to some of the characteristics of this smoke-screen, to mention some of the problems successfully solved behind its somewhat restrictive protection, and to indicate other problems where exploration was barred because the search led into the hazy obscurity of theory.

There is little doubt that, thus far, dollars and cents have limited the study of phenothiazine as an anthelmintic. The estimate of the United States Department of Agriculture that the drug has saved annually more than ten million dollars in meats, hides, wool, and surgical catgut suggests appreciable progress within these limits. Furthermore, estimates place the return to the sheep industry of Kentucky alone at three million dollars in a single year. These estimates, which by their impressiveness tend to cover the gaps in our knowledge, are obviously discrepant, since phenothiazine is useful in treating horses, cattle, goats, swine, and poultry, as well as sheep. The Department of Agriculture may assert privately that ten millions represents only one-half the "guesstimated" annual savings; the other half is charged to Federal caution against possible overenthusiasm. Perhaps the total savings are nearer to the more sumptuous figure that may be inferred from the Kentucky estimate since it is supported by observations made on Kentucky farms by Prof. R. C. Miller, sheep specialist. Possibly even the higher figure is meiotic.

We can be more certain of the effect of this anthelmintic research on the chemical industry. Phenothiazine was only a laboratory compound when the Federal zoologists started to kill worms with it in 1938. By 1943, three million pounds were manufactured and sold to farmers for an average price of one dollar per pound. Farmers as a group are pretty fair businessmen. They did not spend three million dollars on phenothiazine and many hours administering the drug to their livestock without objective evidence of dollars returned. How many dollars? I haven't the slightest idea.

A dollars-and-cents estimate of values may be misleading in a time of war and of the OPA. Just when military surgeons began sending out frantic SOS's because of the impending shortages of surgical catgut, phenothiazine became available. Surgical catgut is made from the guts of sheep. The knotty lesions produced by nodular worms regularly made the intestines of many American sheep unsuitable for the manufacture of catgut (Fig. 1). In a single season phenothiazine conquered the nodular-worm disease of sheep over wide areas. A money value is placed on the surgical sutures thus saved, but in wartime and in face of threatened shortage does this represent the true value?

These practical achievements were not obtained without error and dispute, although they appear inevitable to one

looking backward. The experimental therapy of verminous infections demands considerable knowledge in chemistry, physics, and statistics, as well as in zoology and veterinary medicine. Few of us possessed all the training desirable, and occasionally we were impatient with one another's shortcomings. Therefore, we made our share of errors; we sometimes debated trivial issues acrimoniously; but all of us, I think, were sometimes a little wistful because a particularly inviting field could not be explored. Looking backward, we can be more tolerant and, perhaps, profit a little by our errors and by calling to mind again a few of those accessory problems to which little or no study has been given, thus far, because they were not immediately productive of practical results.

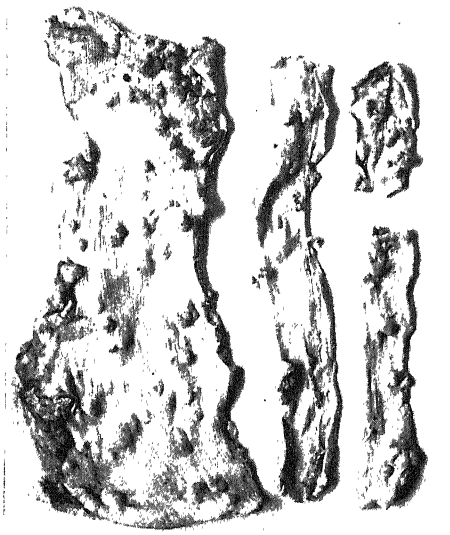


FIG. 1. FRAGMENTS OF SHEEPGUT SHOWING SEVERE LESIONS CAUSED BY NODULAR WORMS PICKED UP BY THE ANIMALS ON PASTURE.

PHENOTHIAZINE was used first as an anthelmintic in the Zoological Division of the U. S. Bureau of Animal Industry. This Division had already accumulated an impressive series of significant first discoveries. Here Theobald Smith discovered the import of the transmission of infectious diseases by arthropods, and here C. W. Stiles slighted his work with domestic animals to establish the presence and character of hookworm disease in the United States. Here a young zoologist, Maurice C. Hall, started to evaluate all known vermicides only a few years after Ehrlich discovered arsphenamine. This project, which has been pursued continuously to the present time, resulted in the discovery of the hookworm remedies, carbon tetrachloride and tetrachlorethylene, that are used in human medicine today. These discoveries attracted widest interest because they applied either directly or indirectly to human welfare. Beside them may be placed an enormously greater number of investigations affecting the parasites of both domestic and wild animals.

The treatment project was deserted in 1936 by Hall and those of his staff who were experienced in chemotherapeutic research because their achievements won for them greater opportunity for service in the National Institute of Health. Although relatively inexperienced, I was, of necessity, placed in the key position on the project. At first I hoped to find in the record of past experiments some guiding principles such as chemists use to estimate the characteristics of unknown compounds. Caius and Mhaskar (inevitably nicknamed Chaos and Massacre), Fischel and Schlossberger, as well as the records of the Zoological Division were studied without finding any trustworthy guide. Yet two publications, only a month apart in time but half a world apart in space, were fascinating, for Hall as well as Caius and Mhaskar had pointed out that chloroform was effective against hookworms but dangerously toxic. Simultaneously they reasoned that carbon tetrachloride might be more effective and less toxic because each molecule of the latter drug

was saturated with chlorine. The hypothesis seemed valid when Hall proved carbon tetrachloride superior to all known drugs for removing hookworms from dogs, but it was destroyed completely when he and his colleagues made further studies on the halogenated hydrocarbons.

Other false hypotheses which, nevertheless, led directly to very practical results are recorded in the literature. The diamidines, which are now used against certain types of parasitic diseases, developed from such a false start. Why not, therefore, select compounds largely at random and test them critically after Hall's established method? It was an exciting question, for who could tell what common chemical might prove to be the solution to existing problems such as controlling the nodular-worm disease of sheep (Figs. 2, 3, 4), which older and more experienced men had pronounced unsolvable? Perhaps the traditional attempts to associate chemotherapeutic activity and chemical structure were foredoomed to failure—even as the search for the philosopher's stone. But as that earlier search had produced modern chemistry, so the confident, though false, theorizing of the biologists had led to valuable results.

Each annual report of the Chief of the Bureau of Entomology and Plant Quarantine exhibited in the late 1930's definite enthusiasm over a new insecticide called phenothiazine. I did not locate a supply of this new insecticide because I was not familiar with the synonym, thiodiphenylamine, under which it was listed in the catalogues. However, in January of 1938 E. F. Knipling's manuscript describing the action of phenothiazine against horn-fly larvae in cattle feces was referred to the Zoological Division. Immediately Knipling's superior, Dr. F. C. Bishopp, was asked for the source of phenothiazine, and in a few days the Zoological Division received from him a sample of the chemical. It was tried first against two important parasites, the broad-headed tapeworm in poultry and the protozoan of cecal coccidiosis in chickens. In both tests, phenothiazine failed to show any promise of curative action. Even today it has no proved action in any protozoan or cestode infection.

In May 1938 we received a gift of some worm-stunted pigs. At this time starvation of the host was considered essential to success in anthelmintic medication. Perversely, we gave phenothiazine to one of the pigs in a mixture of feed. Twenty-four hours later this pig, as well as another animal treated

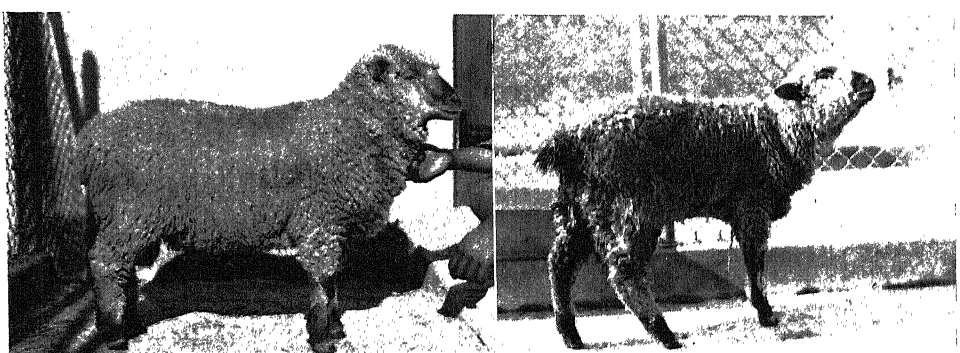


FIG. 2. EXTERNAL EFFECTS OF NODULAR-WORM DISEASE ON SHEEP
GRADE RAM AT LEFT WAS RAISED NEARLY WORM-FREE. GRADE RAM AT RIGHT WAS INFECTED EXPERIMENTALLY WITH NODULAR WORMS. (PHOTO BY SARLES, U.S.D.A. TECH. BULL. 875.)

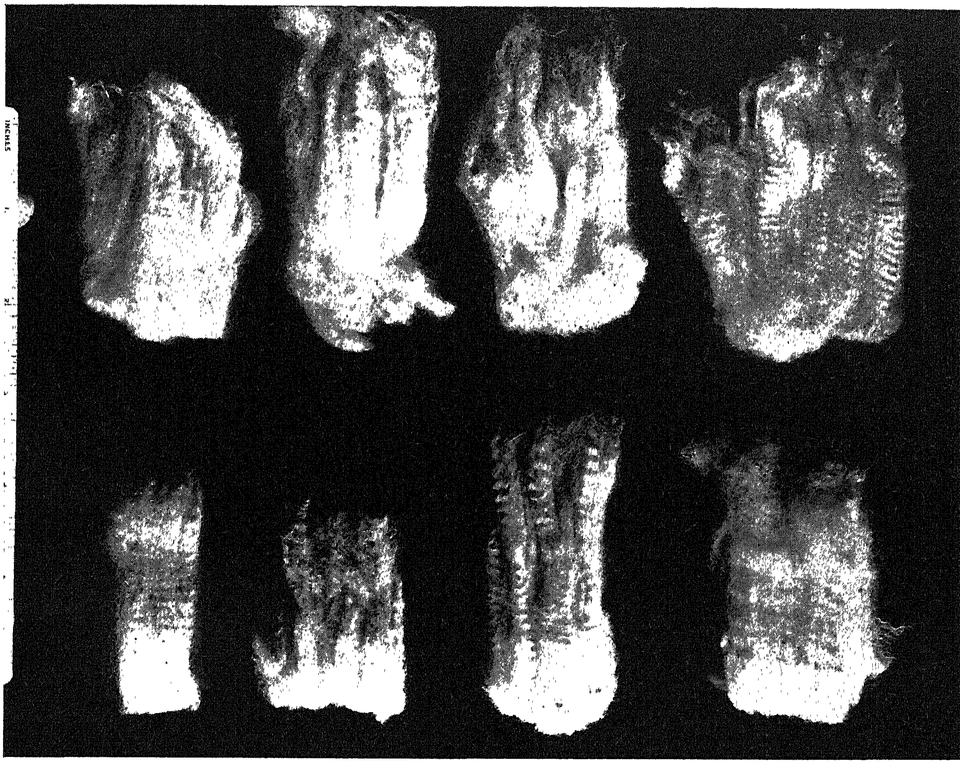


FIG. 3. EFFECT OF NODULAR-WORM DISEASE ON WOOL
Upper: FROM 4 UNINFECTED SHEEP. *Lower:* FROM 4 HEAVILY INFECTED ANIMALS. (SARLES.)

after the traditional manner, began to eliminate the untouchable nodular worms in large numbers. This was only the first tradition of anthelmintic medication to be reversed by the new drug. In September of that year we undertook the real problem, the application of the new drug to the control of the nodular worm of sheep. The greater part of the routine work fell upon a veterinarian who was brand-new to research with worms and vermicides. Although lesions of previous infection were numerous and extensive, the first animal, to his profound distrust, harbored only one nodular worm, which the drug removed. He was assured that the circumstance was ordinary, but he was not convinced. He feared the trivial results were due somehow to his failure through inexperience.

A young animal in poor condition was selected next as a likely subject. The veterinarian was advised to wait until Monday before treating the animal, but he dosed it immediately, although it was late Thursday afternoon. Friday was uneventful, but by Saturday morning there were hundreds of worms to be collected, hunted out, and counted. In the Federal laboratories at Beltsville, Md., no one was expected to work outside of official hours. Consequently, the veterinarian, who was proceeding cautiously, found himself locked away from his work just as it became interesting and exciting. It was a beautiful Indian summer afternoon when he called me from the garden and asked for assistance in getting back to his work. Apparently almost everyone in the division was on

some outing. After much telephoning we located a key and succeeded in completing the test. In a single dose phenothiazine had removed 296 nodular worms and 125 hookworms from a sheep, as well as uncounted tens of thousands of tiny bankrupt worms. The technique of Hall's critical test which was used in these experiments is so deceptively simple that the novice almost always expects to accomplish too much in too short a time. Hall delighted to recount his own

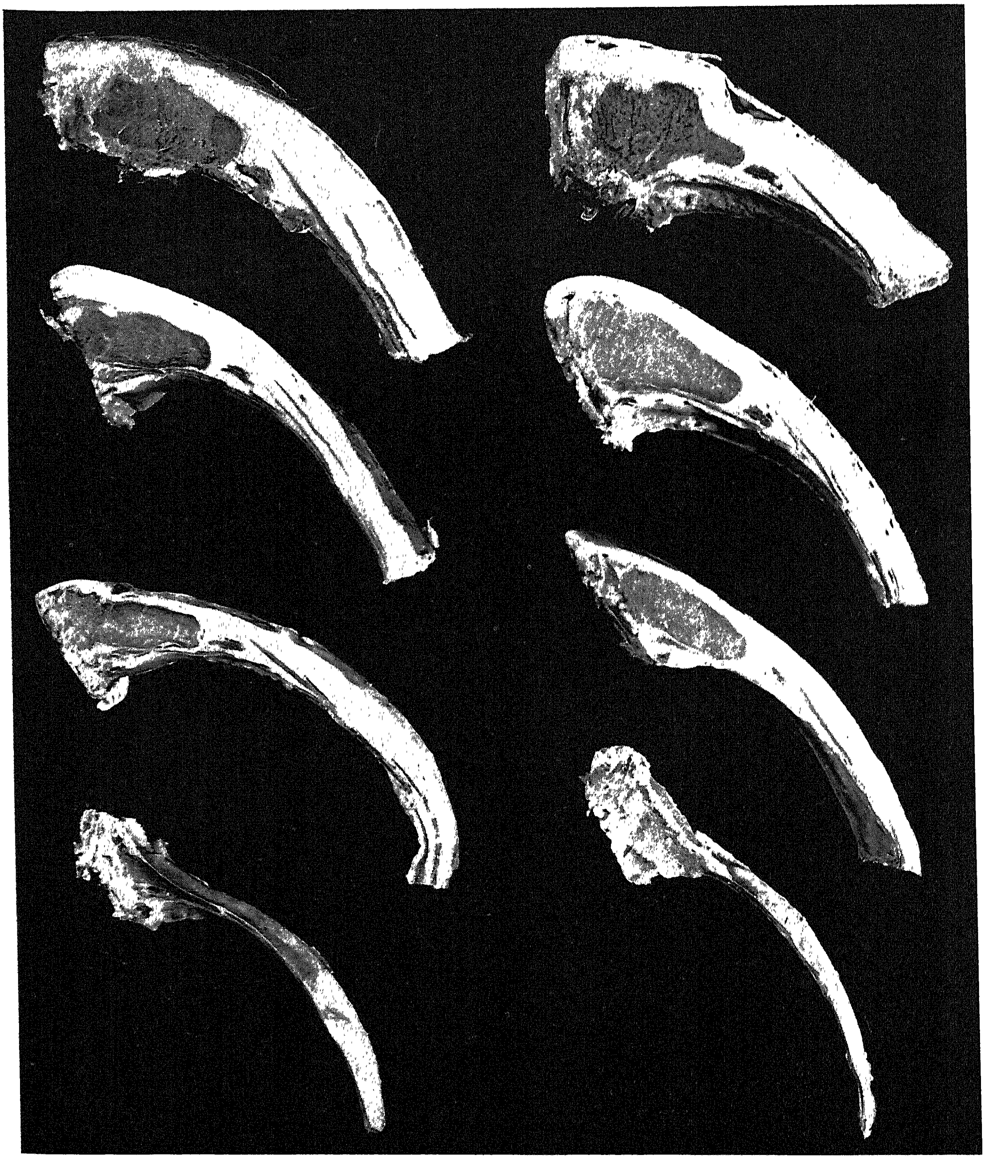


FIG. 4. EFFECT OF NODULAR-WORM INFECTION ON MEAT PRODUCTION
TWELFTH-RIB CHOPS FROM 8 LAMBS—CHOPS FROM EWES AT LEFT, FROM RAMS AT RIGHT. THE INDIVIDUAL LAMBS OF EACH PAIR (FROM TOP TO BOTTOM) WERE GIVEN EXPERIMENTALLY 0, 280, 2,800, AND 28,000 LARVAE OF THE NODULAR WORM, RESPECTIVELY. (FROM SARLES.)

early experience with the test to novices in the Zoological Divison.

"I dosed six horses for bots at one time," he would remark, then smiling cryptically he would puff at his pipe.

Invariably the novice would ask, "Was that too many?"

"Too damn many," Hall swore casually and without emphasis, "within two days I was completely hidden behind garbage cans of manure, and still it kept coming. One horse would have been too many."

Warned by Hall's story, I was conservative when it came time to test phenothiazine in horses. To Dr. Schwartz's request for an estimate of the number of animals needed, I replied, "One at a time will be sufficient." Dr. Schwartz smiled tolerantly but as usual continued his careful planning.

The results of our first tests were released quickly in a technical journal. Consequently, interested investigators were able to determine in a relatively short time that phenothiazine, which is valueless against certain parasitic worms, was, nevertheless, more effective against a greater variety of verminous vermin than any other anthelmintic known heretofore. Its effectiveness in parasitic conditions of various ruminants, of horses, of swine, and of poultry was proved in repeated tests from all parts of the world. Fortunately, we had access to De Eds's invaluable and extensive studies, which were undertaken origially at the instigation of the U. S. Bureau of Entomology and Plant Quarantine, on the toxicity of the compound for laboratory animals. This background furnished justification for proceeding to practical application with rather limited toxicity tests on each species of animal. For the most part it was adequate, but unfortunately a few horses proved extremely sensitive to the drug. Recent research in California and in the Beltsville Research Center hold promise of overcoming this handicap to the use of phenothiazine for the control of equine parasites.

Although Hall's synthesis of chemistry and chemotherapy failed, he bequeathed his critical method of proving vermicidal materials to those who followed. In this method all worms removed by a treatment are collected and counted. The treated animal is killed as soon as elimination of helminths ceases, and any worms which survive the treatment are likewise collected and counted. Thus precise information is obtained. The phenothiazine data collected after this method in the laboratories of the Zoological Division, at the Beltsville Research Center, were quickly corroborated at other institutions where other techniques were often used. Yet conclusions based on these experiments were severely criticized because statistical analysis cannot be applied to data of this type. In due time an extensive coordinated trial, involving the services of over a dozen scientists, convinced the statisticians and proved by further corroboration, if such were necessary, that conclusions based upon Hall's critical method, which has been used effectively for three decades, are still reliable.

Nevertheless, statistical methods have invaded the field of anthelmintic research quite successfully from another direction. Animal husbandmen do not always appreciate the necessity of determining precisely those helminths that are affected by a particular drug, nor are they able to estimate the value of the data collected by means of the critical test, for to most of them a species of worm is only an awkward name. The only important question, as these scientists conceive the problem, is the effect of treatment on the health of the animals, particularly on the rates of gain in young animals. Furthermore, some nutritionists felt that parasitic disease was merely an expression of malnutri-

tion, a state supposedly unknown to parasitologists. However, the British investigators Stewart and Crofton, with the aid of Prof. R. A. Fisher, demonstrated that parasitism and malnutrition are distinct. In Great Britain many sheep were unthrifty—the condition being known as pining. The cause of pining was variously described as parasitism or as a nutritional disturbance due to a shortage of certain minerals in the diet. Stewart and Crofton divided a flock of sheep on a pasture where pining was suspected into four groups: a control group, a group receiving phenothiazine, a group receiving a drench containing several minerals that were supposedly present in insufficient amounts on the pasture in question, and a group receiving both drug and minerals. All sheep were weighed at intervals. The differences in uncorrected mean gains, which favored the treatments, were as follows:

Phenothiazine versus controls	4.7 pounds
Minerals versus controls	4.2 pounds
Phenothiazine and minerals versus controls	8.9 pounds

The raw data, therefore, suggest that the sheep suffered both from a mineral deficiency and from parasitism, but the range of the gains made by individual sheep was, as usual, much greater than the mean differences. Therefore, interpretation was difficult or impossible without statistical analysis, which was supplied by Prof. Fisher. After appropriate analysis he was able to state, "It will be seen that there is practically an additive effect of phenothiazine and of minerals and no appreciable interaction. These two main effects are unquestionably significant."

Heretofore parasitologists have been quite willing to concede that some interaction occurred between malnutrition and parasitism to the detriment of the host. Now Stewart and Crofton's results suggest that the minority, i.e., the parasitologists, should take a more independent view of their science. For ammunition with which to implement their war of independence these scientists must acknowledge the usefulness of statistical methods which, undoubtedly, have won a permanent place in helminthic research, but these methods must remain secondary to the facts-of-life pertaining to parasites.

The mechanism by which phenothiazine destroys the helminth remains unknown. In this connection, the extreme insolubility of the drug attracted attention at once. Hall and his colleagues had discovered that superior anthelmintic efficacy among the halogenated hydrocarbons was associated with the degree of water solubility rather than with the amount or type of halogen present in the molecule. Most other types of vermicides had similar properties as regards water solubility. At saturation from 1 to 10,000 parts of water of room temperature are necessary to dissolve 1 part of the previously known, effective anthelmintics. However, 800,000 parts of water are required to dissolve 1 part of phenothiazine. Although phenothiazine is extremely insoluble, it is known to form within the animal body certain compounds by oxidation.

Usually the leuco-forms are excreted in the urine of the dosed animals. Therefore, I reasoned in an early paper that phenothiazine as such may not be the actual anthelmintic but that some more soluble compound derived from the drug may be the vermicidal agent. The investigation of the hypothesis was beyond my resources at the time, but competent investigators soon tried available oxidized derivatives, including phenothiazone and thionol, only to prove them worthless. Nevertheless, J. M. Zukel found that phenothiazone as well as phenothiazine was an active insecticide against the American cockroach. He reported that the lethal compound

for cockroaches was conjugated leucothionol, which was produced in the body of the insect from either phenothiazine or phenothiazone but not from thionol. Experimental study of this problem might yield interesting data for the anthelminticist.

Possibly most investigators have dreamed of leisure to learn other techniques, to apply them to new experi-

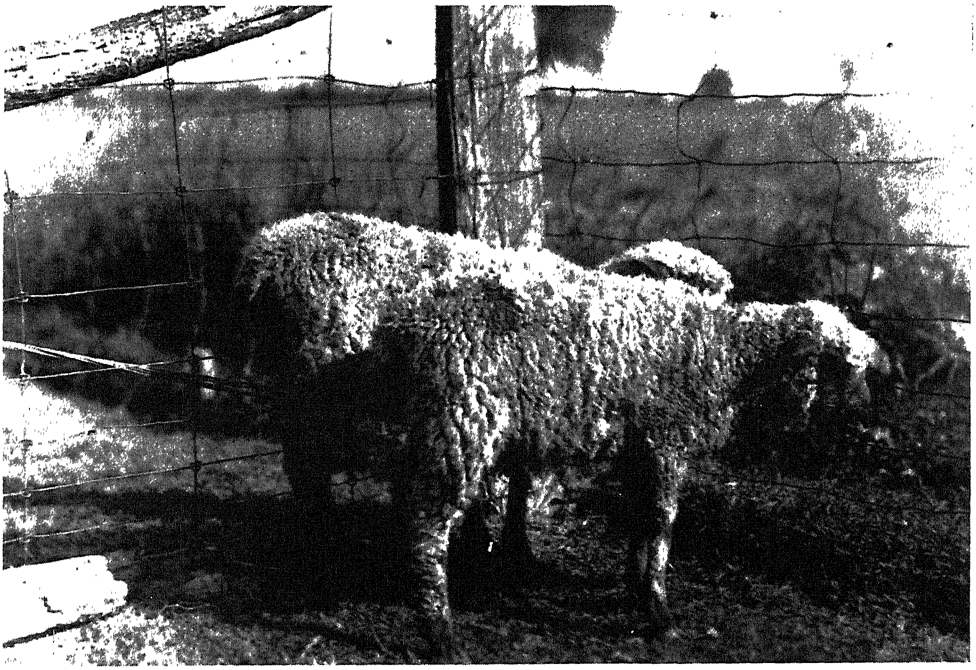


Photo by Bell, Ohio Agr. Exp. Sta.

FIG. 5. LAMBS SUFFERING FROM WORMS

THESE SHEEP ACQUIRED A MIXED INFECTION ON OHIO PASTURES. LOSS FROM WORM DISEASES EVERY YEAR IN A SINGLE OHIO COUNTY FORMERLY WAS EQUIVALENT TO A TRAINLOAD OF LAMBS.

mental designs, and actually to conduct the experiments which might explain this puzzle. Since the practical value of the results remains questionable, most of us, whose livelihood depends upon the results of our research, have of necessity directed our attention toward practical problems which are far from completely explored. However, a few youthful investigators, confident of their new research ability, have been scornful of our yielding to the dollar-pressure. They have promised publicly to raise phenothiazine medication above the "merely empirical" and to elucidate the mechanism of phenothiazine activity, although analogous mechanisms remain unknown for all other anthelmintics except the escharotic phenols. Nothing has come from these lofty promises. Meanwhile, the Canadian H. B. Collier is calmly and quietly reporting his explorations which closely border and may some day enter this interesting field.

To helminthologists phenothiazine has broken precedents and principles within its field as consistently as did the late Mr. Roosevelt within the political arena. Fortunately, the scientists have accepted the former innovations with better grace than some of us Republicans were able to accept the latter. Since its earliest days, the Department of Agriculture had discouraged self-medication

of animals with drugs mixed with salt. Sulfur, tobacco, copper, and various mineral mixtures were not only useless when given in salt, the substances sometimes proved decidedly toxic. On the other hand, livestockmen preferred the easy way; namely, self-medication. Since Knipling was able to poison hornfly larvae in cattle manure by feeding to the animals very small amounts of phenothiazine, the possibility of controlling worms by poisoning the free-living stages which develop in the manure was apparent to all informed helminthologists. We received letters on the problem from as far away as Australia, we discussed it frequently at Beltsville under the leadership of our chief, Dr. Benjamin Schwartz, and we worried over the possible undesirable effects, such as wool-stains, that might result from continuous administration. Obviously, the problem required cautious preliminary study such as it received from Shorb and Habermann. Once these investigators had determined the phenothiazine-salt concentrations acceptable to sheep and deadly to worms, numerous cooperators appeared. Within two years the Department gladly reversed a time-honored policy and recommended self-medication of sheep as a means of parasite control.

For the first time an anthelmintic has been used in a manner analogous to the use of insecticides in *Anopheles* control where the main objective is the conquest of human malaria. The cecal worm of domestic poultry carries in its egg the protozoan that causes blackhead disease in gallinaceous birds, particularly turkeys. Scientists from the Washington State Experiment Station demonstrated that phenothiazine is very effective for the removal of cecal worms from chickens. Overleaping the next logical step, namely, determining the efficacy of the drug against the cecal worm in turkeys, we applied our new knowledge directly to the control of blackhead by treating large numbers of these birds with phenothiazine. Apparently we have thereby discovered a means of controlling a serious disease by destroying its carrier; but had we chosen pheasants instead of turkeys we might have been disappointed, for limited tests carried out in Ashland, Ohio, suggest that the drug may be of relatively little value for removing cecal worms from pheasants. Usually in anthelmintic medication the size of the dose is regulated by the size of the animal, the larger animal receiving the bigger dose. Nevertheless, records of several hundred tests which are being prepared for publication suggest that an effective dose for large fowls may be less effective with small fowls. Possibly the failure of phenothiazine in pheasants is associated with the small size of the host, but the problem requires further study which we cannot give to it in Ashland at present.

Parasitologists have long known that the most serious losses from parasitism frequently are unapparent. When malaria is controlled within an endemic area, other chronic conditions may be greatly alleviated. Also they know that cattle are extensively infected with gastrointestinal parasites, but they are able to prove only comparatively light losses in these animals. They hesitate to claim for these hosts the existence of the unapparent but serious losses known to follow extensive infection with many types of parasites in other hosts. Phenothiazine quickly proved valuable against the obvious cases of severe gastroenteritis in bovines because of its specific effect on a variety of nematodes associated with this condition. Lately evidence of extensive, but unapparent, losses in the cattle industry is beginning to accumulate because of investigations of phenothiazine. The Survey Committee of the National Veterinary Medical Association (of Great Britain) reported in

February 1945: "Some veterinary practitioners maintain that sub-clinical parasitism in adult cattle is widespread. They report a rise in the milk yield and a noticeable improvement in the condition of adult animals in apparently normal health following the administration of phenothiazine."

Moreover, we have recently published the results of experiments conducted in Ashland County on steers that harbored relatively few parasites. Animals from a single herd were divided and placed on similar lots. A mixture of phenothiazine and salt was given to one group, while salt alone was given to the other. Although there was never any evidence of clinical parasitism in either group, the steers receiving medicated salt outgained the steers on salt alone in two experiments conducted in successive years. The differences were statistically significant. Such observations suggest that the drug may find wide application in the cattle industry, but many more studies are needed.

There is little need to mention other practical consequences of phenothiazine research. The problems it has solved are already cut and dried, while others are being carved. The highway to these goals has offered for exploration many a tempting byway, but pushed as we were by dollar-demands we never had a chance to turn aside; to study, for example, the method by which phenothiazine kills worms. We have neglected fundamental problems, some have said, but it is impossible to define fundamental as it applies to science. To some "fundamental" means the study of smaller and ever smaller particles such as molecules, atoms, and electrons. Yet is a proton essentially more fundamental than a living being? Indeed, "fundamental research" is no more susceptible to an accurate definition than "practical research." Not many years ago, I witnessed the discomfiture of a Government bureau which had been charged with the duty of controlling an important insect pest of livestock. Dutifully the bureau spent large sums annually "controlling" the pest. One year they discovered there were two distinct but closely related species of insects involved—one essentially harmless, the other very troublesome. Unfortunately, many of the control measures recommended had been aimed directly at the relatively harmless form. A little preliminary work by an "impractical" taxonomist might have saved many millions of dollars and much valuable research time. In truth, the unknown remains unknown both in fact and in significance until some bit of it is objectively studied. Any attempt to classify it as practical or ideal, as fundamental or superficial, as god or devil will prove, in specific cases, erroneous, for the classification of the unknown must be based largely upon reasoning by analogy from established premises.

Yet we may not censure a bureau that has consistently requested larger appropriations for taxonomic research because too small a portion of its funds were allocated by Congress to the investigation of insect identification. Only the initiated can foretell with reasonable accuracy the points which are likely to yield fundamental or significant results. Until scientists have broader control of their projects, we may expect very frequent repetitions of this embarrassment. Even planning by the initiated can only reduce, not wholly obviate, such errors. Opportunity for several investigators to attack a problem independently, according to their separate concepts of what is fundamental, seems most likely to keep research close to the optimum of productivity. Even a relatively minor subject, such as phenothiazine as an anthelmintic, has required thus far the services of more than a hundred scientists to reach its present, imperfect state of development.

Others will contribute before our knowledge is complete. No one man, or group, could have accomplished all this by working in seclusion. Through cooperation we may hope for a practical investment of the problems remaining in this field.

Many of the practical investigators of phenothiazine were trained in such academic pursuits as systematic zoology—"butterfly chasing" it is called by some deprecators. Therefore, some helminthologists are not unfamiliar with abstruse science. Indeed, in the Zoological Division, we sometimes discussed in the midst of blue tobacco smoke such fundamental problems as the role of helminths in the transmission of diseases. Possibly, we reasoned, some human disease for which no clear etiology is known (for example, poliomyelitis) is carried by a helminth. The Division was not able to undertake the problem because of obvious limitations of equipment and authority. However, it never permitted its limitations to grow into frustrations. Similarly, the students of phenothiazine have not scorned to consider either the practical or the recondite. Slowly, they hope, the more esoteric, rather than fundamental, considerations which have been slighted may be filled in. For the present, however, the practical research which also leads to worth-while results is the only type that can be justified to such employers as the U. S. Congress.

COMMUNION AT MIDNIGHT

The hour is late; I put my books aside.
The empty sheet before me mocks the hope
I had when I began. Oh how we grope
From clue to clue before the one is tried
That brings success. I sigh, and quit the room,
Meaning to leave my work behind, but no . . .
And burdened with unbidden thoughts I go,
Head bent, and gloomy, to the outer gloom.

The campus clock strikes with a somber tone.
I pause, and turn, and raising up my head
I see the darkness marked by squares of light
Where other men still labor in the night;
Dejection passes, and there comes instead
The warming thought that I am not alone.

CLARENCE R. WYLIE, JR.

EARLY MAN IN OREGON

STRATIGRAPHIC EVIDENCE*

By L. S. CRESSMAN
DEPARTMENT OF ANTHROPOLOGY, UNIVERSITY OF OREGON

WHEN did man, that is the Paleo-Indian, first come into Oregon? This, of course, is but one aspect of the larger question: When did man enter the New World? Obviously his arrival in Oregon must have been somewhat subsequent to his setting foot on the Western Hemisphere. His cultural remains have been found in the High Plains east of the Rocky Mountains from Saskatchewan to Mexico, with some scattered evidence of uncertain significance in the lower Yukon of Alaska. He has been reported from Florida, and in recent years impressive series of archeological finds have been made in the Great Basin, lying between the Rocky and Cascade Mountains. In many of these instances human remains have been associated with those of an extinct fauna characteristic of the Pleistocene—*Equus*, camel, *Bison taylori*, sloth, dire wolf, mammoth, and others. What does this association with an extinct fauna mean? It obviously indicates that we can correlate the time of man's occupation of this vast area with that of this Pleistocene fauna and that therefore we are dealing with Pleistocene man. Or the association may simply be evidence that the Pleistocene fauna actually did not become extinct until Post-glacial times. The significance of the association depends upon the time of the extinction of Pleistocene fauna, an event varying probably both in time and space.

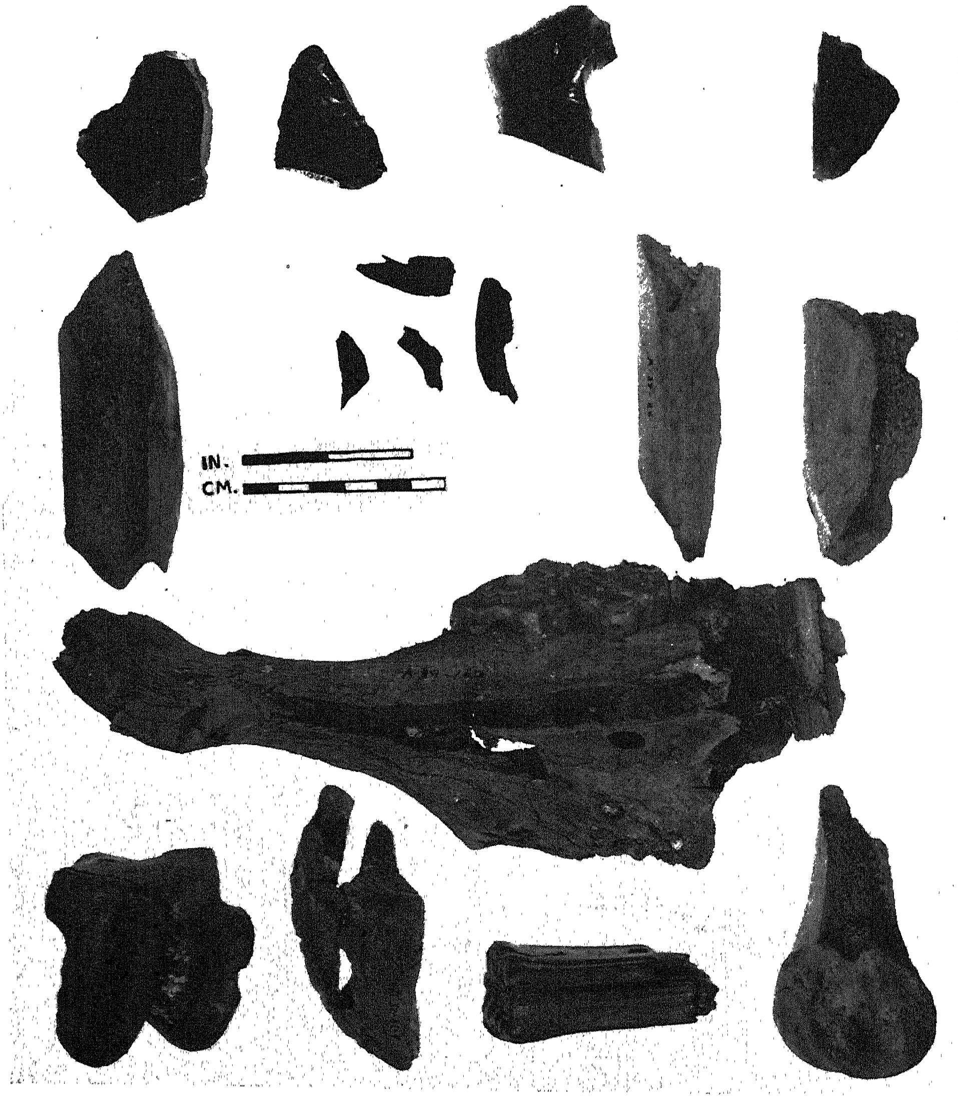
Remains of Early Man have been found, too, in situations indicating that the climatic conditions prevailing in his time were different from those of the present. We find him associated with beds of old lakes, such as those in eastern Oregon, Lake Mohave in California, and

* This article and the two following ones by Drs. Hansen and Allison were based on papers presented at a symposium on Early Man in Oregon at the annual meeting of the Oregon Academy of Science, Portland Ore., January 13, 1945. They are here published together as an example of an effective integration of archeological, botanical, and geological knowledge in a common research problem.—EDITOR.

TEST PIT AT LOWER KLAMATH LAKE, 1940. MOUNT SHASTA IN BACKGROUND

others, all of which are now dry and possess only limited power to support life under primitive conditions with its great dependence upon the natural environment. We also find him associated with such geological phenomena as volcanism. This is best illustrated in Oregon by sites in Summer Lake and Fort Rock Valley, where we find the archeological remains separated by beds of pumice—in Summer Lake from the explosion which formed the crater in which Crater Lake now lies, and in Fort Rock Valley from the Newberry Crater some 20 miles to the north.

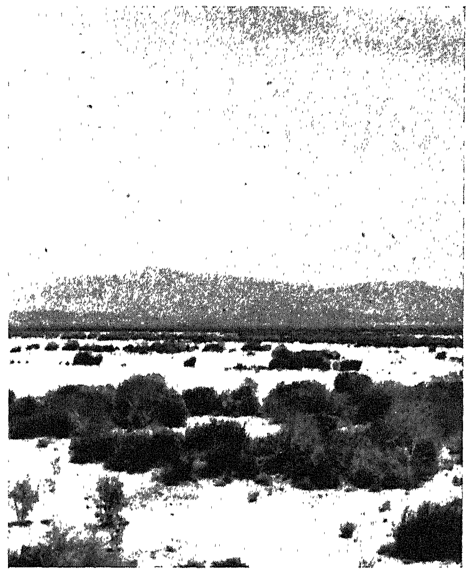
At this point we should define our



ARTIFACTS AND FOSSIL REMAINS

Equus, CAMEL, BISON, AND OTHER REMAINS, WITH CHARCOAL, OBSIDIAN, AND SPLIT BONES ASSOCIATED IN A DEPOSIT IN PAISLEY FIVE MILE CAVE NO. 3. DEPOSITS FOUND AT 7 FEET.

terms for a common basis of understanding. By Early Man we mean the representative of *Homo sapiens* sometimes called the Paleo-Indian. He is not, as some have wished to think, a specimen suggestive of the pre-*sapiens* type like Neanderthal man, but a true representative of *Homo sapiens*. We shall use the words Pluvial and Postpluvial throughout this paper. Pluvial period will refer to the period contemporaneous with, and slightly subsequent to, the last glaciation and characterized by a greater precipitation and somewhat lower conditions of temperature than at present. The area affected by the Pluvial period extends from the northern Great Basin of south-central Oregon roughly to the Mexican border and trans-Pecos in Texas. This area was once characterized, as O. E. Meinzer has shown, by a vast system of lakes without outlet dependent for their water supply primarily upon precipitation and its loss through evaporation. It is now characterized for the most part by dry lake beds, but in some places there are small bodies of water that last throughout the year, while in others there are only playas limited to a few weeks, more or less, at the time of the spring runoff. By Postpluvial we refer



NEAR THE NARROWS
LOOKING SOUTHWEST OVER LOWER KLAMATH LAKE.

to the period following the retreat of the ice and the corresponding northward movement of the storm tracks, with reduced precipitation and rising temperature. Postpluvial corresponds in the main to the expression Postglacial or Postpleistocene. Since this term is well established in the literature, and the chronology has been worked out in terms of the divisions of the Postpluvial, we shall use it in our discussion.

KNIVES FROM WIKIUP DAM SITE NO. 1
FOUND WELL UNDER BED OF MT. MAZAMA PUMICE.

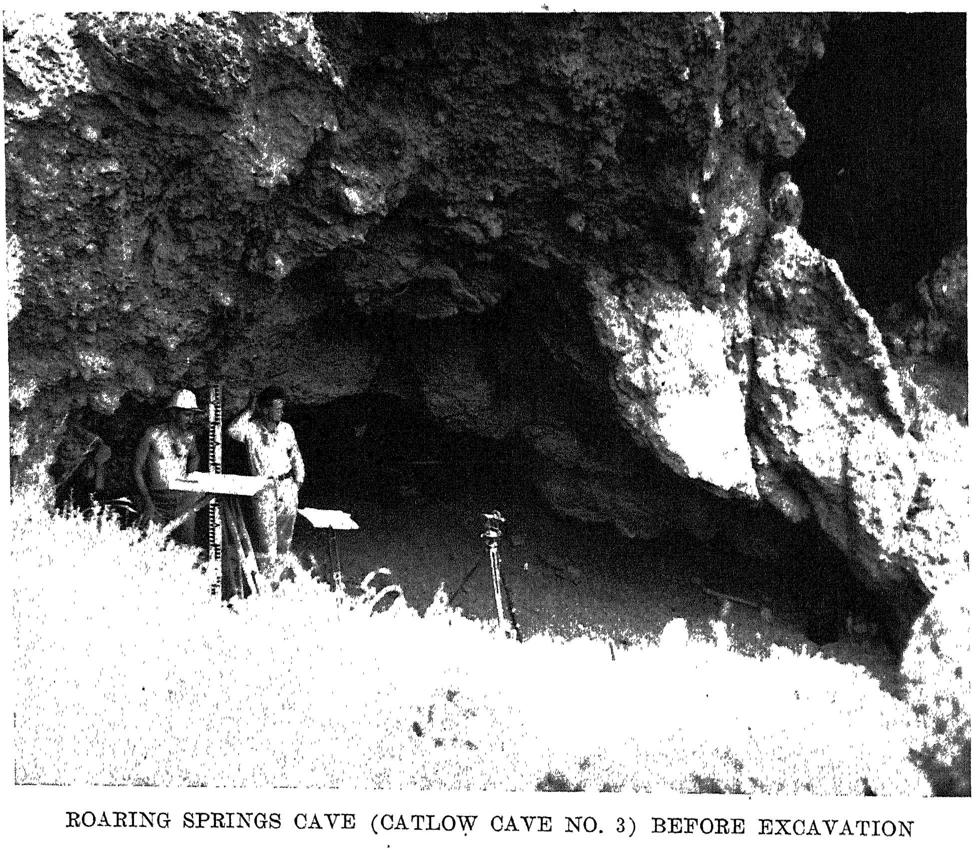
Archeology has for its purpose the reconstruction of human history previous to the time of written records or the memory of man. Its first function is to orient its findings in space and time. The space problem is not difficult, but the time problem often proves to be a troublesome hurdle. This time problem is of two kinds. There is the simple matter of establishing relative times for the specimens dug up, that is, which is earlier than another, and this is the familiar problem of stratification with which the geologist works. It has some of the same difficulties of confusion of beds or layers that the geologist faces.

The second problem is that of establishing an exact or reasonably exact chronology. This means that some reference point, the age of which is known, must be established, and the archeologist then may orient his finds in time with reference to this datum.

The archeologist, in order to work out his chronological record, must combine his knowledge with that of the cooperating geologist, paleontologist, botanist, climatologist, and sometimes other specialists such as the bacteriologist and the pathologist. He is not always able to find a primary reference point in any one of the fields dealing with earth history, and then he is forced to rely upon the picture provided by an integration of the results from these cooperative fields. Sometimes he has the good fortune, as we have had in Oregon, to find a stratified site that can be related to the geologic occurrences which deposited the sediments interrupting the course of human occupation. Then, if the date of this geologic occurrence can be fixed, it will serve as a reference point in time from which the archeologist can work.

Geologic History

The geologic history of the Great Basin has been studied fairly extensively since the early 1920's, with earlier investigations generally in the form of surveys of specific areas. G. K. Gilbert (in 1890) and Israel C. Russell (in 1885–86) provided the outstanding early accounts of the geologic history of Lakes Lahontan and Bonneville. In Oregon, G. A. Waring (1908–09) and Russell (1884) sup-



ROARING SPRINGS CAVE (CATLOW CAVE NO. 3) BEFORE EXCAVATION

plied important early information on the northern Great Basin. In recent years, Ernst Antevs has formulated a statement of the Postpluvial history of the Great Basin, and studies of glaciation have been carried out by F. E. Matthes, Eliot Blackwelder, and others. Henry P. Hansen, of Oregon State College, has incorporated analyses of the pollen profiles of northern Great Basin localities in his extensive study of Postglacial forest succession in the Pacific Northwest. Volcanism and its relation to the geological history of the Great Basin have been studied in greatest detail by Howel Williams. Various studies in sedimentation have been made by J. C. Jones, Ernst Antevs, and others. The most recent and most significant study in stratification for the whole Postpluvial picture is now under way by Ira S. Allison, of Oregon State College.

In Oregon there are seven basin lakes distributed from Klamath Lake in the west to the valley which held a shallow arm of Nevada's old Lake Lahontan, extending some 15 miles north of McDermitt in southeastern Oregon. Of these, all are typical basin lakes except Klamath Lake.

The post-Pleistocene climatic history is now thought of in terms of a series of fluctuations of precipitation and aridity. There are two theories underlying the studies of the chronology of the Postpluvial basin lakes. The first holds that each lake had its own history and therefore that a sound study could be made of the history of one lake without reference to that of any other. The only exponent of this theory with whose works I am familiar is the late J. C. Jones. Although Jones does not expressly state this theory, nevertheless it is implicit in his studies of Lake Lahontan in which he treats the history of that body of water as a phenomenon isolated from the general history of the Great Basin. The second theory, and the one to which all

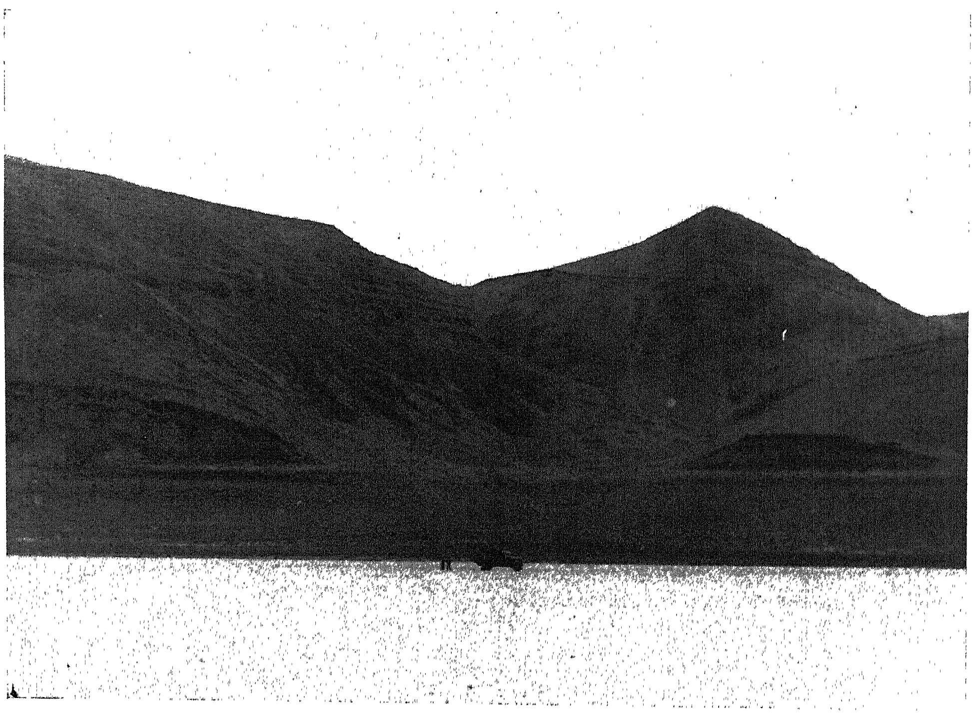
PAISLEY FIVE MILE POINT CAVE NO. 1
STRATIGRAPHIC BENCH RETAINED IN ORDER TO FACILITATE STUDY OF PUMICE STRATIFICATION.

other students of the problem adhere, is that the climatic changes covering long periods of time are cosmic in nature. This theory holds that the Great Basin represents a single province and that changes will be reflected throughout the whole province with minor fluctuations due to local situations affecting temperature, winds, and other factors. It also follows that climatic conditions in a great area such as this are local reflections of world-wide phenomena of climatic change. Therefore, we find Blackwelder attempting to correlate the glaciation of the Great Basin with that of the northeastern part of the continent. Matthes not only sees the history of the Basin as a unit but also correlates it with world-wide phenomena of glaciation and climatic change as reflected in records from Europe and other parts of the world. Antevs accepts this cosmic relationship of the climatic history of the Great Basin as the fundamental postulate for study of this subject. Among others who contributed to the subject, accepting this fundamental point of view, are Allison, Hansen, Kirk Bryan, John T. Hack, and Williams.

Antevs has formulated a Postpluvial climatic sequence and chronology. He divides the Postpluvial into the Early, Middle, and Late periods. The Early

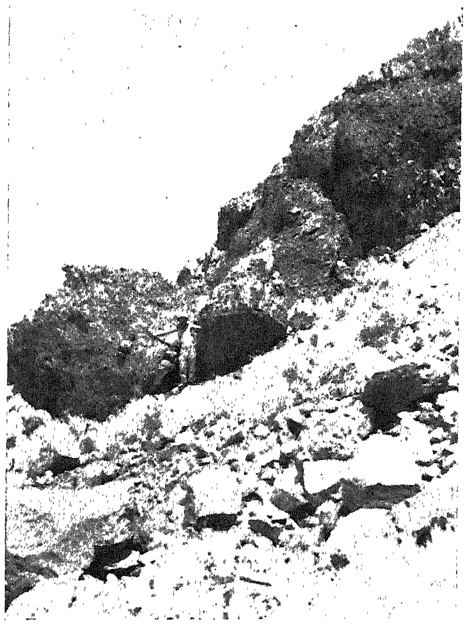
period terminates about 7,500 years ago after a period of progressive desiccation and rising temperature. The Middle Postpluvial extends from about 7,500 years to 4,000 years ago and is marked by a period of greater aridity than any other time in the Postpluvial. During this period most of the lakes in the Great Basin dried up. By 4,000 years ago the Late Postpluvial begins, with an increase in precipitation and a drop in mean annual temperature which resulted in revitalizing the lakes, but not sufficiently to cause them to reach the high levels of the Pluvial period. The Late Postpluvial continued cold and moist until about the beginning of the Christian Era, when the climate became less moist and warmer, approximately what it has been since that time and what it is now. The more moist early half is sometimes referred to as the Little Pluvial.

Hoyt S. Gale, in his study of Owens Lake, estimated on the basis of the salinity of its water that the present lake dates from about 4,000 years ago. Walton Van Winkle studied Pluvial Lake Chewaucan in Oregon, consisting of what are now Summer and Abert Lakes, and concluded on the basis of the salinity of the water that they date from about 4,000 years ago. Matthes in his study of the glaciers of the Sierras formulated the thesis that most of the Pleistocene glaciers disappeared and then were reformed about 4,000 years ago in the western mountain systems. This conclusion is based on the small, very fresh moraines a short distance beyond the cirques. Hansen in his study of pollen profiles has found corroborative evidence



ESCARPMENT ON EAST SIDE OF CATLOW VALLEY, OREGON
SHOWING HIGHEST TERRACE AT BASE OF THE CLIFFS, ABOUT 200 FEET ABOVE THE PLAYA.

of the type of climatic sequences set forth by Antevs and on the basis of estimated rates of deposit of pollen-bearing sediments generally agrees with the chronology established by Antevs. Williams in his study of the history of Crater Lake has found that the utilization of the chronology formulated by Antevs, and agreed to by these other students, provides a satisfactory explanation for the time of the final incidents in the history of Mount Mazama. Probably the most important of all the studies establishing a chronology is that by Allison previously referred to, in which a continuous stratigraphic sequence from Summer Lake is analyzed. We have then a generally accepted chronology and sequential developments of the Postpluvial in the Great Basin accepted by a large number of distinguished experts to which the archeologist can appeal for the necessary assistance in establishing a chronology for Early Man in Oregon and the Great Basin.



ROCKS ON TALUS SLOPE
BELOW PAISLEY FIVE MILE POINT CAVE NO. 2. THESE FELL AFTER LEVEL OF LAKE SUBSIDED.

Archeological Evidence

Let us turn now to the archeological sites where definite evidence occurs by which the history of Early Man in Oregon may be fitted into the general pattern of Postpluvial history. The first is known as the Wikiup Dam Site No. 1, located on the Deschutes River about 30 miles southwest of Bend. Here two knives were found well under pumice from the final explosions which formed the crater in which Crater Lake now lies. Hereafter this source of pumice will be referred to as Mount Mazama pumice. Under the bed of pumice was one of sand mixed with pebbles, many of them highly stained with limonite. This had all the appearance of being glacial outwash. Under this bed was one of partly solidified sand about 4 inches thick, suggestive of hardpan in an early stage of formation. It was this bed from which the knives came. The level character of the bedding in this area is shown by the profiles from test pits made by the Bureau of Reclamation and indicates that the river had been dammed and that probably these sediments were laid down in a shallow lake. At any rate, the knives were deposited well before the Mount Mazama pumice was laid down. Later another knife, in general similar to those just described, was found in a test pit on the opposite side and about three-fourths of a mile farther up the river. This was also under Mount Mazama pumice and was in wash material including good-sized rocks. Consequently, it did not offer the same possibilities of dating as the first two.

A series of caves known as the Paisley Five Mile Point Caves, named from the local designation of the escarpment, where they were found 5 miles from Paisley in Summer Lake Valley, has provided the best stratigraphic evidence of Early Man in Oregon with reference

to the geological and paleontological framework. Two of these caves provide clear-cut evidence of interruption of their occupation by the deposition of pumice from the final Mount Mazama eruption. One of them, known as Five Mile Point Cave No. 3, contained deposits to a depth of about 7 feet. In the bottom of this cave were the remains of an old beach, sand, and water-smoothed pebbles. Partly embedded in the sand and extending into the debris above we found the remains of a campfire, worked obsidian implements, bones broken for their marrow, and the remains of *Equus,* camel, and several other mammals, including an undetermined species of bison. Over this bed was a series of deposits made up of fine dust, bat guano, and then coarser materials, perhaps weathered out of or shaken down from the roof and walls of the cave. On this in turn rested a bed of Mount Mazama pumice. Overlying the pumice was a bed spotted with evidence of occupation. Cave No. 1 was better for permanent occupation and showed continuous, although somewhat spotty, human occupation for a long time antecedent to the deposition of Mount Mazama pumice. Overlying the pumice was a bed of human occupation. We thus had in these caves a point of reference, namely, the eruption which deposited the pumice, from which dating could be attempted.

In the Fort Rock Valley, about a mile west of the remains of the volcano which gives its name to the valley, we excavated a cave in which the levels of the occupation were separated by a bed of pumice ejected at the formation of the Newberry Crater—another point of reference.

Lower Klamath Lake provided three horizons of human occupation, the earliest of which was associated with extinct fauna—*Equus,* camel, elephant, and possibly others. The remains of these animals, as well as the long bone projectile points used by the Indians, and a portion of a human mandible, were all fossilized and were all grouped around a relatively small area in what was apparently the last part of the lake to contain water before it dried up completely. The second horizon is associated with the refilling of the lake and is found at the south end at approximately 8 feet below the historic water level. This occupation was apparently terminated by the rising lake level. The third occupation represents the historic period of the Modoc Indians and is found around the lake shore and on the remains of the "islands" throughout the lake on which the Indians camped and hunted waterfowl. The method used in establishing the chronological sequences in this area has been to formulate the geological history of the lake and relate it to the Postpluvial history of the Great Basin. In this work the writer had the assistance of Antevs, W. D. Smith, and Allison. Hansen made an analysis of the pollen profile at the south end of the lake and at the Narrows, and at the south end the profile extended from well above to below the artifact horizon. As a result of his very careful study, we were able to corroborate the soundness of the earlier geological and climatological history of the lake as we had formulated it.

Correlating the Data

Paleontological evidence shows that man was in south-central Oregon living in caves around the lake shores along with a fauna now extinct, characteristic of the Pleistocene or Early Postpluvial. This fits into the geological and climatological picture and provides irrefutable evidence of human occupation of this area either in the Pluvial or Early Postpluvial period. The difficulty is, as I have mentioned above, in establishing the point of time at which this fauna became extinct. Extinction did not occur uniformly in time, but some of these animals must have lived on in more fav-

orable environments longer than their fellows in less favorable situations. Thus the evidence would seem to indicate that this fauna became extinct in the Lower Klamath Lake region sometime very near the end of the Early Postpluvial, or about 7,500 years ago.

Williams, in his study of the history of Crater Lake, fixed the time for the final eruption of the Mount Mazama pumice at a minimum of 5,000 years ago and a maximum of 10,000. It is not necessary here to go into the evidence he used to establish this chronology. However, a gap of 5,000 years in human history is of real importance to the archeologist, whereas it may be quite negligible to the geologist. At any rate, the evidence from the Paisley Caves and the Wikiup Dam Site locality shows conclusively that man was here considerably before the deposition of the Mount Mazama pumice.

Allison, in his study of the Summer Lake profile, fixes the date of the deposition of the Mount Mazama pumice at not less than 10,000 and perhaps as much as 14,000 years ago. This would therefore push back the date of the occupation of these Summer Lake caves by a considerable period. The pumice in the caves shows that it was deposited either directly from the air from the clouds of pumice thrown out at the explosion or else was blown in from heavy drifts at the mouth of the caves in the bottom of the escarpment. If the latter were the case, it must have fallen on snow-covered ground and been blown in before it could have had dust or other debris mixed with it (Dr. Allison has advanced this suggestion). Hansen's pollen profiles in general support the time estimates for the general chronological picture and the time of the deposition of the Mount Mazama pumice. If the date of the deposition of the Mount Mazama pumice is eventually fixed at 10,000 or 14,000 years ago, then we undoubtedly have evidence of man in the northern Basin dating from probably 15,000 years ago. Thus archeology in collaboration with these other sciences has established an integrated and coherent picture of the Paleo-Indian in Oregon in the Early Postpluvial and continuing to the present.

L. S. CRESSMAN

L. S. CRESSMAN, Ph.D., is Head of the Department of Anthropology and Director of the Museum of Natural History at the University of Oregon. He was born near Pottstown, Pa., October 24, 1897. His B.A. degree was taken in the classics at Pennsylvania State College in 1918. Brief service in the armed forces followed with a commission in the Field Artillery Reserve Corps. He received his Ph.D. at Columbia University in 1925. The next year was spent traveling and studying in Europe. After teaching at the College of the City of New York, he went west in 1928, spending a year teaching at the Ellensburg Normal School in the state of Washington. The following year he went to the University of Oregon. From 1931 through 1940, with the exception of 1936, he has carried on archaeological field work in Oregon. In 1940–41 he was a Guggenheim Fellow studying and preparing for publication a monograph, *Archaeological Researches in the Northern Great Basin*, published in 1942 by the Carnegie Institution as Publication 538. Since the outbreak of the present war he added civilian defense activities to his regular university duties, both in aircraft warning service and on the conservation of cultural resources of Oregon.

EARLY MAN IN OREGON

POLLEN ANALYSIS AND POSTGLACIAL CLIMATE AND CHRONOLOGY*

By HENRY P. HANSEN
DEPARTMENT OF BOTANY, OREGON STATE COLLEGE

SCIENTISTS are constantly delving into the past, seeking to reconstruct the life, climate, and events of prehistoric time, seizing upon any evidence, however minute, to find and fit in a piece of this gigantic paleontologic jigsaw puzzle. This picture of past life, conditions, and events is being projected farther and farther into the past as more evidence comes to light. The farther back one goes, however, the more fragmentary becomes the record, and so the picture of prehistoric life must be portrayed in terms of millions of years. There are three chief problems involved. The first is to find the evidence or record, the second is to interpret the record, and the third is to fit the fragments into a systematic and logical chronology. To do this the chronologist must analyze the record and the interpretations of many scientists in diverse fields. Some events readily fall into their chronological niche, while others are fitted into a sequence with more painstaking and laborious effort. The more conspicuous and widespread time markers provide a means of dividing prehistoric time into a series of large segments for vast regions. More obscure and local time markers help to fill the gaps for limited areas. The shorter periods of time for smaller areas must then be correlated with those from other localities, and an integrated timetable constructed. The units of this chronologic column are largely relative, and it does not seem probable that absolute chronology will ever be attained except for local areas. However, the more segments into which this column can be accurately divided, the more nearly absolute the chronology becomes. In the Pacific Northwest, the cooperative efforts of geologists, archeologists, and botanists have resulted in at least a partial picture of life, climate, and events, and the development of a tentative, relative chronology for postglacial time.

POLLEN ANALYSIS

The most important source of evidence for prehistoric life is the fossil record. It has been said that the paleontologist can reconstruct a prehistoric monster on the basis of a bone of its little toe. Whether this be true or not, the paleobotanist reconstructs prehistoric vegetation with more minute evidence than that. This does not mean a lesser amount of evidence, but rather that the fossils involved are much smaller. The pollen grains of plants are one of the most valuable of these microfossils. One of the routine processes in the life of a flowering plant is the production of almost countless numbers of pollen grains each year from the time of its maturity until it dies. If the plant is an annual or biennial, it may produce pollen only once, whereas if it is a tree it may produce pollen for several centuries. Some of this pollen is the same that causes hay fever in man, and many persons are acquainted with pollen grains only through this unpleasant experience. The pollen of some plants is disseminated through the air in order to reach its objective, the pistil of a flower

* From a paper presented at a symposium on Early Man in Oregon at the annual meeting of the Oregon Academy of Science, Portland, Ore., January 13, 1945.

of the same species. The pollen grains of other plants are large and sticky, cohere in masses, and must be carried by insects if cross-pollination is essential. The air-borne pollen grains are well protected by a thick outer coat and a thinner inner coat, so as to retain their viability for a long time. Pollen grains have been noted thousands of feet high in the atmosphere and hundreds of miles out at sea.

The pollen grains of various groups of plants differ considerably in shape, size, and external configuration. It is possible to separate the principal families, genera, and even species by their pollen grains. After some study one can distinguish pine from fir, spruce from hemlock, oak from alder, etc. In the Pacific Northwest it is possible to distinguish to a reasonable degree of accuracy the pollen of some species of pine and fir, while it is a simple matter to differentiate between pollen of western and mountain hemlock, Sitka and Engelmann spruce, Douglas fir and larch, and many others. Certain groups like the grasses are almost impossible to separate specifically, and so one must be content to interpret the record of fossil grass pollen on the basis of a group.

Down through the ages plants have shed their pollen annually, and in certain favorable sites much of this pollen has been preserved in the accumulating sediments. Analysis of these sediments for their fossil pollen record provides the paleobotanist with a basis for reconstructing the past vegetation. It not only furnishes qualitative evidence but also a quantitative record, something that most fossil records do not. Their preservation in postglacial organic sediments provides, perhaps, a more intensive, detailed, short-period record of the changes and adjustments in prehistoric vegetation than any other type of fossil.

Pleistocene glaciation has been most important in furnishing sites for the preservation of the pollen record. Not only has this well-defined geologic event been instrumental in making possible the preservation of the fossil evidence, but it also provides a significant time marker for the beginning of the record. As the last glaciers retreated from the United States, many lakes and ponds were left in their wake. In fact, most of our lakes owe their origin directly or indirectly to glaciation, whether they lie within or beyond the boundaries of glaciation. The inevitable fate of any lake, large or small, is to become filled with either organic or inorganic material, or both. In the North Temperate Zone, the smaller lakes have become entirely filled with peat, and in many cases their sites are forested so as to obliterate any surface evidence of their former existence. Others are in various stages of swamp or bog formation or other processes of filling. Peat consists of undecayed vegetable matter, composed largely of plants and other organisms that lived first in the open lake and then in the bog that followed. Various groups of plants encroach upon a lake in definite order. First invaders are the submerged species, which are followed by floating species. As the plants die their remains are contributed to the accumulating bottom sediments which are also being added to by incoming inorganic materials. As the water is shoaled by these sediments, rooted aquatics replace them, and this marks the beginning of the end of the lake. An immature bog may have open water in the center, surrounded by concentric zones of vegetation. When the center of the lake is reached by these encroaching plants, there is no place to migrate and they are gradually eliminated by the succeeding zone, and so on. Eventually the lake is covered, and bog or swamp vegetation consisting of sphagnum moss, heaths, reeds, rushes, and

sedges covers the entire site. In the meantime, perhaps many feet of peat have been built up. The coldness and acidity of the water and substratum provide an aseptic medium for the almost perfect preservation of the cellular structure of the plant tissues. Among the plant cells preserved are numberless pollen grains that drifted into the lake and later the bog. They are usually so well preserved that the trained observer can distinguish many of them even as to the species they represent. Year after year, decade after decade, and millennium after millennium a rain of pollen from the adjacent forests has settled on the lake or bog and has become incorporated into the accumulating sediments. The pollen is disseminated to great distances, so that the forests that have existed within a radius of 50 miles may be well represented. Time of pollen shedding, direction of the wind, receptivity of the lake or bog, degree of preservation, relative amounts of pollen produced from the various species, as well as many other factors enter into the accuracy of the record.

In order to analyze and interpret the record, a sedimentary column is obtained, beginning with the silts and sands of the earliest lake bed and extending to and including the surface peat. This is done with a peat sampler, which is so constructed that an uncontaminated sample of a few cubic inches can be obtained at any interval desired. The magnitude of the sampling interval is determined by the depth of the bog; the greater the depth the greater the interval used. A 10-foot section of pollen-bearing sediments may be chronologically equivalent to a 50-foot column from another bog. A small portion of the sediments from each level, a cubic centimeter or so, is chemically treated in order to deflocculate the pollen grains from the rest of the organic matrix, stained, and mounted in glycerin jelly on a microslide. From 100 to 200 pollen grains of indicator species are identified from each level. Fortunately most

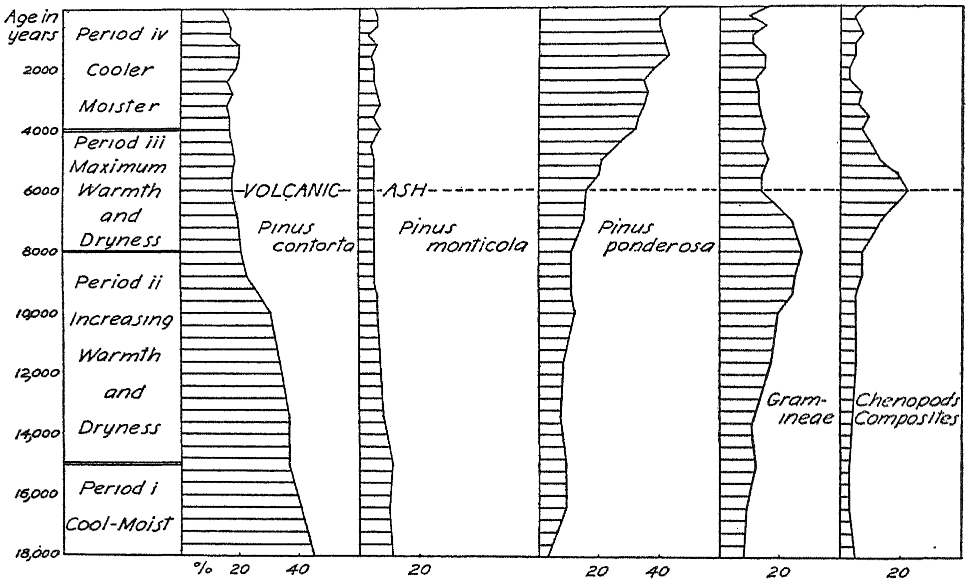


FIG. 1. POLLEN PROFILES, EASTERN WASHINGTON REGION
FOR LODGEPOLE, WESTERN WHITE AND YELLOW PINES, GRASSES, CHENOPODS, AND COMPOSITES.

of our trees have air-borne pollen, and they have left an excellent record in the peat columns. The trees of a forested area are the best indicators of climate and other factors of the environment. In unforested areas, grasses, composites, and chenopods are valuable indicators in the Pacific Northwest, and pollen from these groups is abundant in sedimentary columns from the timberless areas of eastern Washington and Oregon. Many pollen grains of less important species are present, but these have little or no index value in reconstructing the vegetation of the past.

The percentage of pollen for each indicator species for each level is determined. From these data a curve is constructed for each species, revealing its fluctuations during the time represented. Some trees are more abundantly and consistently represented and provide the most significant data for interpretation of the past forests. A given species may be predominantly represented in the lower levels of the column, and then its pollen become less and less upward in the profile until it is almost or entirely absent near the surface (Figs. 1, 2). Other species may not be recorded in the bottom levels but become predominantly represented before the surface is reached. These fluctuations in the proportions of pollen grains of the several species upward in the column reflect the changes in forest composition during the postglacial time as the environmental conditions changed. In a few cases the forest succession thus recorded merely indicates a natural development toward the climax or ultimate type of vegetation that can be supported by the area, involving little climatic change. In other words, vegetation itself modifies the environment and paves the way for continued plant succession, such as mentioned above in bog succession. Slight changes in the pollen proportions of a species from level to level do not indicate changes in the forest composition unless these trends are sustained for several horizons. Minor

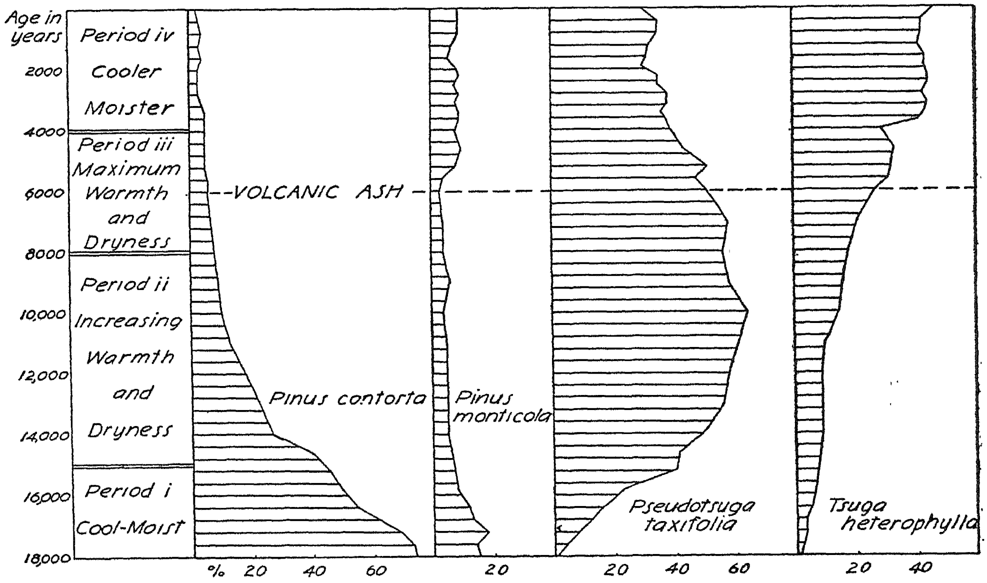


FIG. 2. POLLEN PROFILES, PUGET SOUND REGION
FOR LODGEPOLE AND WESTERN WHITE PINES, COMMON DOUGLAS FIR, AND WESTERN HEMLOCK.

fluctuations, back and forth, are peculiar to the method of pollen analysis. It is the greater and long-range trends that indicate change in vegetation complex, which in turn suggests environmental fluctuation. Sometimes sharp fluctuations from one level to the next denote an abrupt change in the environment, such as those caused by fire, volcanic eruptions, or other catastrophic events that destroy the vegetation and suddenly change the trend of succession.

The principle of fossil pollen analysis is simple in itself; the interred record is obvious and easily accessible, but it did not receive the systematic and concentrated attention of scientists until recently. Although the earliest known work in fossil pollen analysis was done by a Swiss scientist in 1865, it was not until 1916 that Swedish scientists applied the method in working out the postglacial vegetation of northern Europe. In North America this method was first used as early as 1927, but it was not until the thirties that sufficient investigations were being conducted to give any significant results. The future of this paleobotanical method is bright, and more and more applications of its results will be made. Its most recent application is to American archeology, and it may serve to shed much light on the prehistory of man in North America.

Chronological Criteria

The most recent, systematic, and widespread major time marker is Pleistocene glaciation, which occurred during the past million years in the Northern Hemisphere. The time since the last of these great ice sheets reached its greatest advance and began its final retreat is estimated by geologists to be about 25,000 years. In the Pacific Northwest, the Puget Lowland of western Washington and the northern part of that state east of the Cascade Range were covered with glaciers that are considered to be of the same age as those of north-central and eastern United States. Mountain glaciation also occurred in the Cascade Range of Oregon and Washington; the major movements of these were probably generally contemporaneous with those of the continental glaciers. All pollen-bearing sediments that rest upon glacial drift or its chronological equivalent are necessarily postglacial in age. It is doubtful, however, that the vegetation history recorded in the peat columns goes back to immediate deglaciation of a given site, because of delay in sedimentation and/or the absence of forests within range of pollen dispersal. Occupation of the basins by dead ice for some time after the main body had wasted, as well as other conditions brought about by the unstable physiographic situations existent in deglaciated areas, is responsible for an incomplete record. Another factor causing a possible time differential for the beginning of sedimentation in different areas is the rate of ice retreat. It may have taken several thousands of years for the ice to waste back from the point of greatest advance to its center of accumulation. Because of these and still other factors, all immeasurable as to time, the average age of the pollen-bearing sediments in the Pacific Northwest, resting upon glacial drift or its chronological equivalent is estimated at about 18,000 years. Some may be younger and others older, depending upon their locations with respect to the position of the glacial termini and subsequent geomorphic cycles. Many postglacial sedimentary columns lying beyond the limits of glaciation are probably of the same age, because they rest on materials that owe their emplacement indirectly to glaciation, as by inundation of their sites by glacial waters, impounding water in tributaries by aggrading of main stream valleys, and eustatic changes in sea level caused by nurturing and wasting of glaciers, and

POSTGLACIAL CLIMATE AND CHRONOLOGY

YEARS AGO	SWEDEN	EASTERN NORTH AMERICA			GREAT BASIN	PACIFIC NORTHWEST	ASH and PUMICE
	SUB-	COOLER-MOISTER					
1,000	ATLANTIC COOLER	OAK-CHESTNUT				PERIOD IV	
2,000	MOISTER	SPRUCE (oak-beech)		LATE	COOLER-MOISTER		
3,000	SUB-BOREAL	WARM-DRY TEMPERATURE				COOLER-MOISTER	
4,000	WARM-DRY	MAXIMUM OAK-HICKORY					Devils Hill pumice
5,000	ATLANTIC	WARM-MOIST	POSTGLACIAL		MAXIMUM WARMTH and DRYNESS	PERIOD III	Willamette Valley pumice
6,000	WARM-MOIST	OAK-HEMLOCK		MIDDLE		MAXIMUM WARMTH and DRYNESS	Washington
7,000		(oak-beech)					Volcanic ash
8,000	BOREAL	WARM-DRY PINE					NEWBERRY CRATER
9,000	WARM-DRY	PRE-BOREAL		EARLY	INCREASING WARMTH and DRYNESS	PERIOD II	Eruption
10,000	SUB-ARCTIC	COOL-MOIST SPRUCE-FIR					
11,000	COOL	HUDSONIAN					
12,000					RISING TEMPERATURE	INCREASING WARMTH and DRYNESS	MOUNT MAZAMA
13,000	ARCTIC COLD			YOUNGER			Eruption
14,000			LATEGLACIAL		DECREASING MOISTURE		
15,000					SUBSIDING LAKES		
16,000		PRE-HUDSONIAN		MIDDLE			
17,000						PERIOD I COOL-MOIST	
18–20,000							

the modified marine cycles and shoreline processes resulting therefrom.

Many alpine and montane bogs in the Pacific Northwest are underlain with drift from mountain deglaciation. Because of a probable lag in deglaciation at these higher elevations, these sediments are slightly younger than those that lie on continental glacial drift. An average age for these is estimated to be about 15,000 years.

A second but more local series of time markers is postglacial volcanic activity as revealed by one or more layers of ash or pumice in Pacific Northwest sedimentary columns. Perhaps the most spectacular and evident is the eruption of Mount Mazama, which formed the caldera holding Crater Lake in the southern Cascades of Oregon. This great eruption dispersed pumice over some 5,000 square miles to the north and east of Crater Lake, which provides a common time marker for the pollen-bearing sediments that rest on the pumice, and for those at greater distance that contain an interbedded stratum. Dr. Howel Williams, of the University of California, has dated this eruption at 4,000 to 7,000 years ago, a figure which finds support in the limited amount of organic sedimentation which has occurred since. Dr. Ira Allison, of Oregon State College, by correlating lake beds in south-central Oregon containing Crater Lake pumice with postglacial fluctuations of Great Basin lakes, concludes that it occurred between 12,000 and 14,000 years ago. Upon the basis of the depth of pollen-bearing sediments and the recorded forest succession, the writer believes that the eruption of Mount Mazama did not occur more than 12,500 years ago, and probably somewhat later (see table). This preceded the eruption of Newberry Crater located about 70 miles to the northeast of Crater Lake, as shown by the position of its pumice layer above those from Mount Mazama in the lake beds of south-central Oregon (table). Pumice from this source has not been found in bogs in the Cascades, as its dispersal was both local and to the east of the main Cascade Range.

More recent and lesser eruptions of volcanoes in the Three Sisters area of the central Cascades of Oregon have deposited local pumice mantles. A few bogs lie on pumice from an eruption of Devils Hill, which the writer has set at about 4,000 years based upon the thickness of the overlying pollen-bearing sediments and the forest succession recorded therein (table). That this eruption followed that of Mount Mazama by thousands of years is shown by the occurrence of its pumice at 2 meters and the presence of a stratum of Mount Mazama pumice at 4.5 meters in a 7-meter profile 13 miles west of Bend, Ore.

In the Willamette Valley of northwestern Oregon, a layer of pumice is present in the upper third of most peat sections. Examination reveals that it did not come from Mount Mazama. Its position immediately above an oak maximum recorded in the profiles, signifying a warm, dry climate, suggests that the eruption providing the pumice occurred much later than that of Mount Mazama. This pumice may have come from Mount St. Helens in southwestern Washington, about 100 miles to the north. The writer has estimated this volcanic activity at about 5,000 years (table).

In most bogs in the state of Washington, with the exception of those along the coast and in the southwestern part, there occurs a single layer of fine, volcanic ash which may have come from Glacier Peak in the north-central part of the state. This is the most valuable time marker resulting from volcanic activity in the Pacific Northwest because of its widespread occurrence and presence in so many postglacial sedimentary columns. The position of the ash stratum in the sedimentary columns is usually at a level

from two-thirds to three-quarters down from the top. The average thickness of 30 sections resting upon glacial drift or its chronological equivalent is about 7.2 meters, and the average depth at which the ash occurs is 4.4 meters. The stratigraphic position of the ash near the culmination of a warm, dry period as interpreted from the recorded forest succession, suggests that the volcanic activity occurred about 6,000 years ago (table; Figs. 1, 2).

A third factor that has indirectly provided chronological criteria for postglacial vegetation history in the Pacific Northwest is the fluctuations in the levels of the Great Basin lakes. During the last glacial (pluvial in the Great Basin) stage, the levels of Lake Bonneville, antecedent of modern Great Salt Lake, and of Lake Lahontan, predecessor of modern Pyramid Lake in western Nevada, were higher than the modern lakes. They perhaps reached their last high levels during the Tioga glacial stage of the Sierra Nevada of California. This is probably equivalent to the last Wisconsin glacier, which in turn is considered to have been contemporaneous with the final glacial stage in the Puget Lowland and eastern Washington. As mentioned above, the culmination of this stage was reached about 25,000 years ago, the maximum age of Pacific Northwest postglacial sedimentary columns. As the glaciers receded and the climate became drier, both Lake Bonneville and Lake Lahontan apparently dried up during the peak of the drought. The modern lakes, Great Salt Lake and Pyramid Lake, are thought to have been formed later when there was a general increase in precipitation over the Northern Hemisphere. Dr. Antevs believes that this period of extreme desiccation occurred between 8,000 and 4,000 years ago, and he has named it the warm, dry Middle Postglacial (table). The lakes of south-central Oregon, in the northern part of the Great Basin, seem to have had a somewhat similar history as shown by Dr. Allison in the following article in this issue. It is the presence of pumice from Mount Mazama and Newberry Crater in the dry bed of Lake Chewaucan, Pleistocene antecedent of modern Summer Lake, correlated with this evidence for fluctuating lake levels, that furnishes a connecting link between the Great Basin and the Pacific Northwest with respect to postglacial climatic trends. Further evidence for this warm, dry period is offered by the salinity of Owens Lake in California and of Abert and Summer Lakes in south-central Oregon. The present salinity of these lakes need not have required more than 4,000 years to have been attained. As these lakes lie in closed basins, there is no chance for the incoming salts to be lost, so that as time goes on their water develops a greater concentration of dissolved salts. These lakes apparently dried up during the warm, dry Middle Postglacial, and the sediments were buried or removed by deflation. The lakes were reborn with the advent of more moisture about 4,000 years ago, and since have become more saline.

A fourth line of evidence lending support to the termination of a dry period about 4,000 years ago is found in the history of western mountain glaciers. With the exception of the main trunk glaciers, which have persisted since the Pleistocene, many glaciers in the Sierra Nevada were born about 4,000 years ago, according to glacial geologists. This suggests that most of the Pleistocene glaciers disappeared during the warm, dry Middle Postglacial, and their present successors were initiated with an increase in moisture about 2,000 years B.C.

Postglacial Climate

Much evidence has accumulated for the occurrence of at least one warm, dry period in the North Temperate Zone

during postglacial time. In Scandinavia typological succession in peat bogs correlated with varved clay chronology has been interpreted to mean that there were three warm stages of alternating dryness and moisture during the past 15,000 years. Application of the absolute chronology of varved clays has dated these as occurring between 9,000 and 2,600 years ago. Varved clays are annually banded sediments that were deposited in glacial lakes during glacial retreat. The rate of ice retreat has been determined by counting the varves and cross-dating them with varves from lakes formed successively in the wake of the retreating ice. In eastern North American pollen profiles, trans-Atlantic correlations have been applied, and interpreted as reflecting the occurrence of these three warm periods about the same time. Von Post, a Swedish scientist, upon the basis of pollen analytical data from Scandinavia, has divided the Postglacial into three climatic periods. The first was one of increasing warmth, the second was one of maximum warmth, and the third was one of decreasing warmth. He believes that after glacial retreat the temperature gradually rose and attained its maximum between 7,000 and 6,000 years ago. He includes the time represented by the three warm periods mentioned above in an "Age of Warmth" but does not interpret alternating dryness and wetness. The von Post sequence has been applied to pollen profiles in England and also to eastern North American pollen profiles by some American workers. A somewhat modified scheme seems to fit the pollen profiles of the Pacific Northwest.

Pollen analytical data from 65 postglacial sedimentary columns, correlated with the time markers and climatic evidence mentioned above, reveal that there were four periods of climatic trends in the Pacific Northwest. Period I, persisting from deglaciation until about 15,000 years ago, was cool and moist, owing to the influence of recent glaciation. Period II was one of increasing warmth and dryness, lasting until about 8,000 years ago. Period III marks the stage of maximum warmth and dryness which endured until about 4,000 years ago. This stage marks a period of maximum warmth and dryness which was perhaps general over the entire Northern Hemisphere. It was during this time that the Great Basin lakes dried up or reached their lowest levels. This stage may be designated as the warm, dry Middle Postglacial. Period IV saw a return to cooler and moister conditions which with minor fluctuations have persisted to the present (table).

These climatic stages are perhaps most sharply defined by the vegetation history recorded in the sedimentary columns in eastern Washington. In this region, the marine climate of the Coastal Strip and Puget-Willamette Lowland is considerably modified by the Cascade Range and the continental influence to the east. The rainfall is at a critical minimum for forest growth, permitting contraction or expansion of forested areas with only slight changes in precipitation. In pollen profiles from this region, the maxima of grasses, chenopods, and composites for a period beginning before, and continuing for some time after, the recorded volcanic activity marks an expansion of the timberless area due to the warming and drying during the Middle Postglacial (Fig. 1). Western yellow pine, the present climax dominant, rapidly expanded above the volcanic-ash stratum, indicating increased moisture and expansion of the forested zone. The attainment of the yellow-pine climax was apparently held in abeyance by the continued warming and drying until about 5,000 years ago.

In the Puget Sound region the four climatic periods are not so well portrayed by the forest history because of

the greater amount of precipitation and the wide range of environmental conditions tolerated by one of the principal species, Douglas fir. This species is the principal timber tree of the Pacific Northwest. The chief dominant species in the Puget Sound region is western hemlock, which requires more moisture and better soil conditions than Douglas fir. The latter is a subclimax tree that has been able to persist as one of the most abundant species because of recurring fire down through the centuries. If forest succession is permitted to continue without interruption, hemlock gradually becomes the most abundant arboreal species, and Douglas fir is crowded out entirely. The development of western-hemlock postglacial predominance was held in check until sometime after the volcanic activity, first by the unfavorable soil conditions and then by the warming and drying of the Middle Postglacial. The maximum and predominance of Douglas fir occur below the level of the volcanic-ash stratum in most Puget Sound region pollen profiles, while the maximum and predominance of hemlock do not occur until above the ash layer (Fig. 2). The retardation of the development of the hemlock supremacy may have been partly influenced by fires, which tended to favor the persistence of Douglas fir as the most abundant arboreal species.

In the central Oregon Cascades the climatic record in the pollen profiles is somewhat obscured by the influence of a thick pumice mantle upon forest succession. In areas where the pumice is several to many feet thick, the vegetation was destroyed, and the subsequent forest succession has apparently been controlled by the sterile, pumiceous soil. In areas where the pumice fall was lighter, the vegetation was not entirely or immediately destroyed, but a well-developed trend toward a yellow-pine climatic climax was interrupted, and lodgepole pine superseded it and has since remained predominant. Lodgepole pine is the most common tree in Yellowstone National Park. This is well shown in pollen profiles from a peat bog at Tumalo Lake, 13 miles west of Bend, Ore., where two layers of pumice, one from Mount Mazama and the other from Devils Hill, are interbedded in the peat column. In areas southwest of Crater Lake, where the pumice is confined largely to the valley floors, western yellow pine has been predominant since the eruption of Mount Mazama. It attained its maximum in the lower third of the profiles marking the culmination of the warm, dry stage. As these columns rest directly upon Mount Mazama pumice, the warm, dry maximum must have followed the eruption of Mount Mazama by several thousands of years. Two bogs lying upon glacial drift north of the pumice mantle in the Oregon Cascades, reveal that lodgepole pine was the pioneer postglacial invader, the same as in the Puget Sound region and in eastern Washington. The yellow-pine maximum, marking the warm, dry stage, was attained in the middle third of the profiles. A return to moister conditions is reflected in the marked increase in western hemlock and a correlative decline in yellow pine in the upper third of both profiles. As these sections probably represent most of postglacial time, or about 15,000 years, the eruption of Mount Mazama could hardly have taken place more than 15,000 nor less than 8,000 years ago.

In the Willamette Valley of northwestern Oregon the warm, dry Middle Postglacial is indicated by a high maximum of Oregon white oak, which was able to supersede Douglas fir for a brief interval. As oak is the most xerophytic arboreal species in the Willamette Valley, its predominance at these levels supports the other evidence for the warm, dry interval. Its later decline and an increase in Douglas fir denote the return

of moister conditions in more recent time. In the northern Great Basin of south-central Oregon, the evidence of Early Man provides further chronological data which are readily correlated with the history of postglacial vegetation. In four peat columns from Lower Klamath Lake, which probably represent most of post-Mount Mazama time, the pollen profiles reveal evidence for a warm, dry stage, which upon the basis of stratigraphy may be inferred to have been contemporaneous with the warm, dry Middle Postglacial. Also, the occurrence of an artifact horizon in one of the sedimentary columns, at or near the level denoting maximum warmth and dryness, suggests that the lake dried up so that Early Man camped along a slough or stream that flowed through the exposed lake bed. Hence man was in the Klamath Basin 4,000 or 5,000 years ago. Upon the advent of moister conditions, the lake bed was re-inundated and 6 to 8 feet of peat was deposited over the artifacts.

That Early Man was in south-central Oregon as early as 12,000 years ago, or prior to the eruption of Mount Mazama, is revealed by the occurrence of artifacts under the pumice in caves, and the presence of obsidian knives beneath the pumice at the Wikiup Dam Site on the Deschutes River about 30 miles south of Bend, Ore. The importance of this evidence is well shown by Dr. Cressman, of the University of Oregon, in his companion paper in this issue.

The work of Williams, Antevs, Allison, and Cressman has been most valuable in reconstructing the chronology of climate and of the postglacial vegetation in the Pacific Northwest. It shows how the results of several phases of scientific research help to fill in some of the pieces of the paleontologic jigsaw puzzle, which is entombed in the archives of the earth.

INTO THE ROCK

Another footprint shall we be
In rock that held for history
The tusk of giant mastodon
That sweating jungles bore upon . . .
Where lizard slid from slime to stone,
Where fighting dinosaur left bone
And carrion gave to bat-like bird
Mid sounds no human ear has heard . . .
There lava poured its scorching death
Before the glacier's icy breath
Laid waste the land, swept to the sea,
Till nothing lived of fern and tree,
Save only, in terrain, the core
That fought through chaos gone before
When planets to their orbits swung
And from the dark thin light was wrung . . .
God will not let man's footprint fail,
Though wars may blaze till stars grow pale,
Who walked this way and stood erect,
Tasting the world with intellect!

BARBARA WHITNEY

EARY MAN IN OREGON

PLUVIAL LAKES AND PUMICE*

By IRA S. ALLISON
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As evidences of Early Man in Oregon were discovered by Dr. L. S. Cressman, of the University of Oregon, to underlie air-laid pumice in caves excavated by waves on the shores of extinct lakes that once occupied the Fort Rock, Summer Lake, and other fault basins in south-central Oregon, it became important to trace the pumice to its sources and to determine as completely as possible the history of those ancient lakes. Dr. Howel Williams, of the University of California, who was studying the volcanic history of the Crater Lake region, identified the pumice in the caves near Paisley, Ore., as the product of former Mount Mazama (on the present site of Crater Lake) and the pumice in the cave near Fort Rock, Ore., as the product of Newberry Volcano. Dr. Ernst Antevs, research associate of the Carnegie Institution of Washington, following a brief reconnaissance assigned the caves and other shore features to the Pluvial or Glacial Age, as had Russell, Meinzer, and others many years ago. At the request of Dr. John C. Merriam, late president emeritus of the Carnegie Institution of Washington, the writer in 1939 undertook a field investigation so as to provide a more complete geologic framework into which the archeological data could be fitted. Subsequently these studies have continued through parts of several field seasons.

Pluvial History of the Great Basin

The prototypes of these various pluvial lakes are described in the classic works of G. K. Gilbert on Lake Bonneville and of I. C. Russell on Lake Lahontan. Lake Bonneville is an extinct lake that at its maximum stage was more than 1,000 feet deep and covered more than 19,000 square miles in the eastern part of the Great Basin in western Utah and eastern Nevada. Its shore features are clearly shown around its margin, especially against the western foothills of the Wasatch Range in Utah, as at Salt Lake City, Provo, Logan, and elsewhere.

Lake Bonneville had two distinct high-water stages, about 1,000 and 625 feet deep, respectively, which were named by Gilbert the Bonneville and Provo stages. They were separated by a stage of comparatively low water, presumably caused by a change toward a drier climate. Recent interpretations correlate these maximum stages with the Tahoe and Tioga glacial stages of the Sierra Nevada of California, which in turn seem to be equivalent to early Wisconsin (Iowan) and late Wisconsin (Mankato) glacial stages of the north-central United States. Their culminating stages, therefore, were reached about 65,000 and 23,000 years ago.

Former Lake Lahontan in western Nevada, which once covered nearly 9,000 square miles and stood about 530 feet higher than modern Pyramid Lake, had a similar history.

Both Lake Bonneville and Lake Lahontan are thought to have disappeared by desiccation several thousand years ago as a result of extreme drought, and the modern lakes such as Great Salt Lake and Pyramid Lake are thought to have been formed later, because of a change to somewhat moister, though still semiarid, conditions.

* From a paper presented at a symposium on Early Man in Oregon at the annual meeting of the Oregon Academy of Science, Portland, Ore., January 13, 1945.

Thus from the climatic standpoint these former lakes indicate two stages of moist or pluvial climate, each followed by a stage of reduced precipitation (or increased evaporation, or both), and the modern lakes indicate a slight return to pluvial conditions in recent millennia.

Evidence from Fort Rock Basin

The Fort Rock basin is a fault-block depression in the northern part of the Great Basin in south-central Oregon. It too has a record of an early pluvial lake about 230 feet deep and of a later one about 140 feet deep. Waves on the lake of the 140-foot, or Provo, stage, which the writer calls pluvial Fort Rock Lake, eroded the cave near Fort Rock in which Cressman found the artifacts of Early Man beneath a blanket of pebble pumice derived from Newberry Volcano.

Studies of the sediments on the former bottom of the lake by the writer disclosed that the Newberry pumice fall occurred when Fort Rock Lake, once 140 feet deep, had been reduced by evaporation to a depth of about 30 feet and that the lake entirely disappeared shortly afterward. If one assumes that the 140-foot stage was reached about 23,000 years ago and that the lake in its waning stage persisted until about 10,000 years ago (a short time prior to the dry climatic stage of 8,000 to 4,000 years ago) the age of the Newberry pumice and hence of the human occupation of the area is carried back to about 11,000 or 12,000 years ago.

During the later dry stage the wind excavated basins in the lake plain which range in depth from a few feet to 40 feet or more and in area from a fraction of an acre to several hundred acres. Several of these deflation basins later became the sites of small lakes or ponds. Subsequently, such lakes have dried up and deflation on a moderate scale has been resumed. Thus the basins give evidence of post-Pluvial climatic history as follows: (1) Decreasing moisture, increasing aridity, and increasing wind work, that reached a maximum probably during the dry stage of 8,000 to 4,000 years ago, (2) increased precipitation, cessation of deflation, and formation of small lakes in the basins previously excavated, some 2,000 years ago, and (3) later return to somewhat dry conditions and partial renewal of wind erosion.

Clues from Summer Lake Basin

Summer Lake basin in south-central Oregon is another fault-block depression with an area of about 200 square miles, of which modern Summer Lake, shallow and alkaline, covers about 50 to 70 square miles. In this basin pluvial Lake Chewaucan of the Bonneville, or Tahoe, stage stood about 350 feet higher than Summer Lake is now, and pluvial Winter Lake of the Provo, or Tioga, stage was about 210 feet deeper.

The artifact- and fossil-bearing caves near Paisley, Ore., which contain an air-laid stratum of sandy Mount Mazama (Crater Lake) pumice above the artifacts and fossils, belong to the early high-water level.

The sediments of the pluvial lakes, partly exposed in the short valley of Ana River at the north end of the basin, include a series of lake-laid pumice or ash falls. Certain of these are correlated by petrographic criteria with eruptions of Mount Mazama and a later one with the eruption of Newberry Volcano. At the time of the climactic outburst of Mount Mazama pluvial Winter Lake, formerly 210 feet deeper than Summer Lake, was still about 85 feet deeper. Hence the distribution of the pumice took place before the end of Pluvial time. If the last pluvial lake attained its maximum level about 23,000 years ago and disappeared about 10,000 years ago, the age of the main Mount Mazama pumice fall, as indicated by the

80 to 90 foot depth of the lake then existing, must be about 12,000 to 14,000 years. The artifacts underlying the pumice in the caves nearby then must be older yet.

As the pumice in the Paisley caves is in places 18 to 24 inches thick, whereas the corresponding water-laid material on the lake bed is only about 8 inches thick, the possibility that the pumice was drifted into the caves by the wind, perhaps long after the original fall, has to be considered. Evidence against such a secondary eolian origin includes: (1) The purity of the pumice, (2) the difficulty of transporting the pumice particles up hill over or through a coarse boulder field to an elevation about 100 feet higher than the edge of the lake plain, (3) the presence of large numbers of crystals of feldspar and hypersthene that should have been winnowed out from the light pumiceous glass while in transit, and (4) the failure of the wind in modern times, active as it is, so to deliver pumice to the caves.

Instead, the exceptional thickness of the pumice in the caves probably is to be explained as primarily a gravity and not an eolian effect. Very likely as the pumice sand fell on the steep hillside above the caves, it tumbled, rolled, or slid down the slope like hail from a roof and accumulated on a shelf at the cave mouths, whence tramping or minor wind action easily could spread it within the wide but shallow caves. Thus a secondary origin of the pumice layer is unnecessary, if not impossible, and an estimate of 12,000 to 14,000 years for the age of the underlying artifacts appears to be justified by the record of the pumice fall in pluvial Winter Lake.

Conclusion

In both the Fort Rock and the Summer Lake basins the general history of the pluvial lakes, parallel to that of Lake Bonneville and Lake Lahontan, and the sedimentary record in such lakes of a series of pumice falls afford a means of dating the volcanic eruptions. Inasmuch as evidences of Early Man in both areas occur beneath layers of air-laid pumice in caves eroded by waves on the shores of large, deep lakes that formerly occupied these basins in Pluvial time, man must have been present in the region before the explosive eruptions both of Mount Mazama (just prior to the formation of the caldera of Crater Lake) and of Newberry Volcano, which supplied the pumice. Judged by the depths of the shrinking pluvial lakes into which the pumice fell, the main Mount Mazama pumice is about 12,000 to 14,000 years old, and that of Newberry Crater about 11,000 or 12,000 years old. These figures in turn represent minimum estimates of man's antiquity in Oregon.

THE NATION'S MILITARY SECURITY

By Captain MYRON W. CURZON, GSC

PRESIDENT TRUMAN'S message to the Joint Session of Congress on October 23, 1945, on universal military training clarified a major issue upon which the country must now make a decision. It will be a vital decision. The President's introductory words dealt with "a long range program of national military security." We must inaugurate military policies which will secure our present advantage which prompted General Marshall to report to the Secretary of War that "for the first time [in] . . . six years . . . the security of the United States of America is entirely in our own hands." We cannot gamble with our national existence; the stakes are too big.

Articles which have previously appeared in THE SCIENTIFIC MONTHLY on universal military training have dealt at length with collateral benefits and detriments which have in the past characterized military service. They have debated the merits of a year of military training—from the standpoint of mental processes, health benefits, and experiences in democracy. These factors are not to be ignored, but the justification for universal military training, conducted by the Army and Navy in military installations and under military regulations, can only be military necessity.

Hopeful though we all are that the United Nations Organization will bring to the world an enduring peace, it is recognized that our obligations under the United Nations Charter requires us to keep ourselves militarily powerful. Other nations will judge the sincerity of our pledges by the manner in which we show ourselves as ready and willing to contribute our share to the enforcement of the Organization's authority. As one key member of the organization we share responsibility for assuming the initiative; other member nations will work with us and gradually the framework of the United Nations Organization, drafted at San Francisco, will take on substance and strength.

Indeed, in our present world where so many nations have learned no other language except that of military power, the voice we raise for peace and international accord must be accented by our own proved ability to support our words with deeds, if necessary, or it will go unheeded at the time it should be heard. Only after the passing of many years and the application of tests not yet devised will we be able to entrust our national security to the stewardship of other powers, no matter in what combination. Today we are faced with the reality that the surest guarantee that no nation will dare again to attack us is to remain strong in the only kind of strength an aggressor understands—military power.

Military Lessons from World Wars I and II. The mobilization of American forces proved to be the decisive factor in winning victory for the Allied Powers in World War I. Hitler and his General Staff absorbed that lesson well; they sought to avoid direct conflict with the United States as they went about their conquests on the European Continent. The available evidence indicates that the Japanese attack at Pearl Harbor was not in accordance with a unified strategic Axis plan. Once the United States was involved, however, forces were set in motion which eventually led to the defeat of these criminal nations—although at a cost even now only partially realized.

Thus, two efforts to achieve world

domination through the conquest of democratic nations failed—one because the aggressor underestimated the effectiveness of American power, the other because the aggressors failed to isolate and immobilize that power. A future aggressor embarking upon world conquest will not commit these mistakes again. The United States, the Arsenal of Democracy in two wars, will be the first target of attack. Not again will we have years in which to prepare to assume the offensive while valiant allies and our own advance guards hold the line. The initial attack, if it comes, will be upon the very heart of the United States. The robot bomb, the rocket, aircraft carriers, and modern air-borne armies have weakened our geographical security.

It is clear that the United States, now possessed of a fighting strength greater than at any time in its history, and greater than that of any other nation in the world, must take direct, positive action to preserve its position.

Our Military Requirements. Our military requirements in the future will be for a force which can be committed to combat in sufficient strength to withstand a surprise attack and undertake a counteroffensive which would overwhelm the enemy. The development of electronic and rocket weapons and the advent of nuclear physics in warfare have not decreased the importance of trained manpower. On the contrary, the day has long since passed when we can call untrained minutemen to the colors in an emergency to oppose with familiar muskets the threat of invasion. We need men versed in the complexities of radar and rockets, airplanes and atomic bombs, amphibious landings and air-borne invasions. It is not the purpose of universal military training to prepare men for wars of the past but to educate our reserves in the intricacies of the type of warfare in which inevitably our total manpower would be engaged if war should come again.

One means of achieving the desired military strength would be to raise and support a large standing Army. We are well aware, however, that the experiences of other nations with large standing armies have been anything but desirable. Exclusive professional soldier castes have developed. Desirable strengths have had to be maintained through resort to conscription. So great a proportion of the national income has been channeled to the military that the nations approached national bankruptcy, from which they were "saved" only by acts of aggression which eventually culminated in war.

The President's proposal that the postwar military organization be composed of—

(1) a comparatively small Regular Army, Navy, and Marine Corps;

(2) a greatly strengthened National Guard and Organized Reserve for the Army, Navy, and Marine Corps; and

(3) a General Reserve composed of all the male citizens of the United States who have received universal military training

successfully avoids the pitfalls of militarism and is a far-reaching step toward the goal of national security. It contemplates a force of regulars, no larger than necessary to perform its normal peacetime duties and to meet sudden minor emergencies, to be reinforced when necessary by reserve forces organized from the trained young men of the Nation.

If these young men are to reinforce our small regular forces promptly for active service in any part of the world, they must be reservists in fact. Otherwise they will require more training upon mobilization and delay the reinforcement of the regular forces, thereby entailing, to provide the same degree of security, a larger and more expensive regular peace establishment than would be necessary if immediately effective reservists were available.

A military organization adapted to the international situation which confronts

us, erected upon our traditional military policy whereby the military is always subordinated to the civilian authority, will be practicable if supported by a system of universal military training, but not otherwise. With it, even with a relatively small establishment, the United States will always have enough organized units in the Regular Army and Navy, the National Guard, and the Organized Reserves to supply a substantial quota in any force required by the United Nations to suppress aggression. To meet a great emergency additional trained reservists will be drawn into the service under such emergency legislation as Congress may enact.

Universal military training is only one of the essential parts of the program of national security for the United States. Equally important are the development of an efficient and practicable scheme for industrial mobilization and an adequate program of scientific research and development. The development of expeditious methods for the mass production of war matériel is an important phase of scientific research which has been neglected in the past.

The adoption of a sound plan for universal military training now will give impetus to the whole program of national security for the United States. The training program will pick up where the military service of the veterans of this war leaves off. Training installations, matériel, and the guidance of combat veterans, with their war lore, uninterrupted and fresh in mind, will be available and at hand. Once we disband and scatter this setup, it will be more difficult and more expensive to re-establish the necessary facilities.

It has been suggested in some quarters that inauguration of the training program should be postponed until the shape of the peace is better known and until our commitments under the United Nations Organization are determined. It is difficult at any time to know precisely what our responsibilities will require in the way of military power. This much we know—if we are to have available a force when needed, the time to begin preparing is now. If, at some later time, conditions change, then the program can be re-examined and revalued.

It is no valid argument against adopting universal military training at this time that there are now millions of trained veterans of this war available for service in any emergency. We cannot rely indefinitely upon those veterans; they have earned the right to return to civil life. In any emergency the burden will rest upon the youth of the nation to constitute the new reserve military strength. If that be their responsibility, then ours is to give them the training to fit them to their tasks.

The Training Program. Both the Army and the Navy have prepared programs for the training of our vast citizen forces. The objectives of the programs are similar—to produce units capable of engaging in combat upon the shortest possible notice. It is no accident that each service recommends that the overall length of the training program be one continuous year. The recommendations are based upon experiences gained under the stress of war, when every nerve and fiber were strained to expedite the flow of men from induction stations to the theater of combat.

A youth will commence his training at the age of eighteen, or upon his graduation from high school—whichever is later; but in any event before his twentieth birthday. Those who complete high school in the seventeenth year may enter upon the training immediately, if they have parental consent. In general, the time at which a youth graduates from high school is optimum from the point of view of his being physically able to undergo the training, mentally capable

of assimilating its principles, and economically able to withstand its interruption to his career. At that time, too, the economic loss in the production of goods and services for the Nation as a whole will be at its least.

Perhaps it is inaccurate to speak of an "interruption" in the young man's life. For the small percentage who might be continuing their education (less than 17 percent of the whole) the year would, as Dr. Karl T. Compton has said, bring the young men to college "with greater maturity, more realistic social adjustment, and greater determination to make the best use of their future educational opportunities." For the rest they would at the conclusion of their training enter the ranks of business, agriculture, or industry with a useful preparation in technical skills and the inculcation of valuable habits of discipline, teamwork, and industry. The year would be a part of, and not apart from, the total educational experience of our youth.

Adequate basic or "boot" training, further technical training applied according to each youth's aptitude and preference insofar as the needs of the services allow, and then unit training culminating in combined maneuvers in which all arms and services participate under simulated battle conditions will provide a well-rounded fifty-two weeks of stimulating activity. The emphasis in the training will not be on mere drilling. It will be on the use of all the instruments and weapons of modern warfare. Every qualified youth will be offered a chance to perfect himself in some military specialty.

Provision will be made within the armed service to help trainees improve their educational status. Ample opportunity will be presented for self-improvement. Some part of the training could be used to develop skills which would be useful in future civilian life. Moral and spiritual guidance has always been considered of highest importance by the armed forces; it will continue so in the training program. Ample medical care will be available.

These programs differ greatly in concept from the three- and four-month replacement training programs of World War II. In the early days of the war well over a year was devoted to the preparation of, first, the individual; then the small unit; and finally the larger unit. After all the units required by the troop basis were trained and transported to the various theaters of operation, the wartime training programs concentrated principally upon providing adequately trained individuals for replacements. The replacement must be trained well as an individual, but since he joins a veteran organization in which he is surrounded with skilled, tried men who willingly aid him, his training need not be as extensive as that of the original members of the group.

Once the youths have completed the prescribed course of training, they would be enrolled in the General Reserve for a period of six years. The General Reserve would be available for rapid mobilization in time of emergency, but no obligation to serve in the military forces would be imposed upon its members, either in this country or abroad, unless and until called by an act of Congress.

Trainees need not stop with the prescribed program. Commissions would be granted to qualified men who complete the course of training and who then take additional instruction in Officer Candidate Schools, in the Reserve Officers Training Corps, and in the Naval Reserve Officers Training Corps. Outstanding trainees could be selected after an adequate period of training and sent to college with Government financial aid, on condition that they return, after graduation and with ROTC training, as junior officers for a year or more of additional training and service.

Freed of the necessity of providing basic training for enrollees, the ROTC program could be established at a high level, comparable to the academic levels of college education in which the young men of the ROTC are engaged. They would be prepared for training as officers, prospective leaders of men. The product of such an ROTC would provide the National Guard and the Organized Reserve with an officer corps of exceptional character.

One continuous year of training will effect the maximum economy in the cost, in time and money, of a universal military-training program. A year of training will not, of course, furnish fully trained men for all the specialties and ratings which the armed forces require. Many specialists require considerably more than a year of training. The numbers of men of this type required for a well-rounded reserve will be met with trainees who volunteer for additional training.

The Role of the Armed Forces. The system of military organization herein described would provide a democratic and efficient military force. It would be a constant bulwark in support of American ideals of government. It would constitute the backbone of defense against any possible future act of aggression.

It has been suggested that universal military training violates traditional American concepts of liberty and democracy and that it will create a militaristic nation. On the contrary, the objective of the program is to train citizens so that, if and when Congress declares it necessary for them to become soldiers, they could do so more quickly and more efficiently. A large trained reserve of peace-loving citizens would never go to war, if it could be avoided. But if war came to them, repetitions of many of the bloody sacrifices heretofore experienced could be rendered unnecessary.

Nothing can alter our Nation's unshakable opposition to war and traditional determination to preserve the peace. We have tried throughout our history to support our pacific purposes with a display of weakness. The inevitable result has been to encourage the vaulting ambitions of every sawdust Caesar who dreamed of world domination. Against their rising power we could oppose only the pitiful weapons of pacts and appeasement. The end of this road was war and war for which we were forever unprepared. The lesson of this our past history is plain. We must enforce our will for peace with military power.

In the words of the former Chief of Staff, General Marshall: "If this nation is to remain great it must bear in mind now and in the future that war is not the choice of those who wish passionately for peace. It is the choice of those who are willing to resort to violence for political advantage. We can fortify ourselves against disaster, I am convinced, by the measures I have here outlined. In these protections we can face the future with a reasonable hope for the best and with quiet assurance that even though the worst may come, we are prepared."

WHAT IS NATIONAL DEFENSE?*

By Lieutenant (jg) KENNETH B. PLATT, USNR

WERE the best course to pursue for our national defense clearly definable, doubtless it would have been so defined long ago. Since the course is uncertain and the problem urgent, thorough exploration of all views is in order.

In his article *The Fallacy of the Lost Year* in the August, 1945, SCIENTIFIC MONTHLY, sounding the virtues of compulsory military training both for its own sake and to improve our educational program, Professor Keller has raised issues too vital to our national future to be left unchallenged. Emphatically, I wish to second his statement: "The proper order of education, especially at the outset of schooling . . . is: obedience first, reasoning second. That has been the order which the race has had to follow if it was to survive. It is the order of learning anything." Unquestionably, our prevailing educational practices would benefit from the introduction of more directed study and the deletion of much folderol. But that a year thus gained would be beneficially spent in compulsory military training as outlined by Professor Keller I feel is a fallacy even greater than the one he decries.

The chief concern of this article is with the problem of national defense. On the educational issue, suffice it to say that the vast amount of new material on the natural sciences brought forward in the past two decades, plus the monumental problems in social science affecting interracial and international relations now crying for the major effort of generations, might well absorb all the educational time salvageable from school activities of questionable value.

What is national-defense preparedness? Before attempting positive suggestions, let us review, through the medium of Professor Keller's article, some of the things which preparedness is *not* and explore the reasons therefor.

1. The basic assumption of Professor Keller, that a large body of trained reserves *is* national-defense preparedness, is the very trap from which the world has just dragged its mangled form. In World War II our backlog of ROTC-trained men accumulated since World War I was entirely ignored except where this training had led to reserve commissions. The swift advance of war methods and equipment had left those men who were trained only basically no better fitted for either leadership or combat than those with no training at all. This experience was paralleled in every other country which had pursued a similar preparedness program. It would doubtless be repeated in the event of a third world war.

World War II demonstrated nothing more clearly than the pitiful inadequacy of orthodox military defense against a determined aggressor. Recent testimony by General Halder, one-time German army chief of staff, showed that Czechoslovakia had 45 divisions of men better trained and equipped than the 15 divisions Hitler had at the time of the Munich *putsch,* plus a more formidable defense cordon than the French Maginot line. Add to this the fact that the Czechoslovak Nation was created by the Allies after World War I specifically to block possible future German ambitions in that direction and was, accordingly, backed by the Allies as one of their prime

* The opinions or assertions contained herein are the private ones of the writer and are not to be construed as official or as reflecting the views of the Navy Department or the naval service at large.

defense agents. Yet by one colossal bluff of aggression Hitler negated this whole defense structure, and in the time gained thereby went on to prepare the actual aggressions which demolished Poland and France, each with larger "trained" forces than Germany's, as well as the many smaller countries with armies aggregating far more than the armies of Germany.

In short, this war may be said to have demonstrated incontrovertibly that no nation is prepared to defend itself until it is prepared, psychologically as well as with men and mechanisms, *to wage aggressive offensive war.* This is no startling new idea, but merely a reverse statement of the historic fact that no war ever has been won by defensive means.

Presumably Professor Keller's article already was in print before the atomic bomb burst upon the world, finally and irretrievably shattering the dream of adequate national defense through large standing armies and trained reserves. Actually, this dream has been only a dream, at least ever since science got into full stride. For while it is true that every war has been won by men fighting and dying in battle, it is also true, and decisive, that the means of victory increasingly have come to be the products of superior science. This fact was borne out in a recent tabulation of "lessons learned" in this war, which included as one lesson: "The American soldier learned at Cassarine Pass that he is no better, man for man, than any other soldier with equal equipment and training." The significance of this point as it affects preparedness is that the equipment used at Cassarine Pass was the product of decades of research and development in all branches of science, whereas the G.I. training required to use that equipment to a victorious conclusion was a matter of weeks. Whatever doubt of the futility of numbers of men against superior science may have lingered until Hiroshima and Nagasaki surely has vanished since those doomsdays, as witness the statements of General MacArthur and other eminent militarists that the day of large standing armies is past.

Despite these lessons of current experience, if the United States should now launch into a permanent program of compulsory military training for all physically fit males as they come to military age, there is a very great danger that we should in time come to regard this program as the essence of preparedness.

2. In an early paragraph of his article Professor Keller declares that the youth will benefit from the proposed training in that "his physical defects are going to be detected and rectified, . . . his intoxicants limited, . . . his mental and moral hygiene . . . improved." The experience of the medical profession has been that the majority of physical deficiencies which render a man unfit for military service cannot be satisfactorily cured after the individual reaches military age. It is generally agreed that preventive medicine and hygiene through childhood give the only satisfactory solution to this problem. The fact that most of these deficiencies are regarded as preventable is prima-facie evidence that they should be prevented—by such changes or improvements in our national food, social and recreational habits, sports, hygiene, medical programs, etc., as may be necessary—rather than "cured" after serious harm has been done.

3. Although limiting of intoxicants is a good thing while it lasts, the fallacy of this argument in connection with military training is that the associations and conditions of military life are such as to expand the drinking habit. Coming into this life at an impressionable age, many young men who never before have used intoxicants soon acquire the habit because it is one of the things that "every-

body" does. It is part of the military tradition. Others increase their use of intoxicants out of sheer boredom because bars and saloons often are the only places they can go to mingle with other people during the brief, infrequent, and predominantly night-houred liberty periods customarily granted from military duty. Few will argue that the drinking habit, once acquired, is easily curtailed under self-discipline after imposed prohibitions are removed.

4. The idea of improving the average young American's mental and moral hygiene under conditions of military life simply does not square with the facts. Ask any experienced chaplain. Every circumstance of this unnatural environment is against such improvement. The individual is away from home, family, friends, church, community—away from every outside influence which formerly guided him in a normal balanced life. He is cast into a mass of strange humanity under conditions which, compared with home life, are harsh, confusing, and discouraging. The result is moral corrosion. Although myself raised and hardened in rough company, I found as a Naval enlisted man in "boot" training that masses of men confined in barracks soon dropped to depths of immorality never approached in my previous experience. To quote Kipling (as does Professor Keller, differently) from his immortal character Tommy Atkins:

> An' if some times our conduck isn't all your
> fancy paints,
> Why, single men in barricks don't grow into
> plaster saints.

I may add from personal observation that married men react the same as single men in this respect.

5. In a later paragraph Professor Keller surmises we "can be confident that two vitally essential desirables will be inculcated, directly or indirectly, in all curricula: Discipline and 'Democracy'." The fundamental incompatibility of military discipline and democracy was clearly expounded by John Paul Jones[1] in a letter of September 14, 1775, to Joseph Hewes, member of the Marine Committee of the Continental Congress, embodying suggestions on the organization of a naval force, quoted by Buell in his book *Paul Jones,* in part as follows:

> A navy is essentially and necessarily aristocratic. True as may be the political principles for which we are now contending, they can never be practically applied or even admitted on board ship, out of port or off soundings. This may seem a hardship, but it is nevertheless the simplest of truths. Whilst the ships sent forth by the Congress may and must fight for the principles of human rights and republican freedom, the ships themselves must be ruled and commanded at sea under a system of absolute despotism.

The same relationship between commanders and commanded might have been as well bespoken for army organization. The years have altered this relationship little, if any. Certainly they have not altered the professional militarists' view on the subject. Despite much talk of a "democratic" army in this war, it just did not work out. It could not. We will do ourselves a service if we recognize at the outset that military discipline is part and parcel of military method, and that it is applied from above, inevitably. At the same time every effort should be made to encourage self-discipline in everyday life as one of the priceless fundamentals of true leadership ability which gave us a decisive edge over the discipline-numbed German soldier of World War I, who was helpless when his officers were put out of action. Where we *must* have superimposed discipline, so be it; but let us not fool ourselves that it is good for its own sake.

[1] The authenticity of this letter has been denied by DeKoven (*Life and Letters of John Paul Jones*), but its wide acceptance and teaching as the "moral and intellectual charter of Annapolis" make that point irrelevant to the present argument.

6. As for the prospects of discipline in the proposed compulsory military training program, I do not share Professor Keller's confidence that "it is not at all likely" that the military establishment will impose "a narrow type of it" as in the past, nor that "if it should run in a lot of duds or dubs, in courses or teachers, they will be speedily shown up." Unfortunately in this regard, military discipline requires unquestioning obedience and universal conformity. Unfortunately, too, for the outlook of the prospective trainee, the lower one goes in the military hierarchy the more does force of rank depend upon autocratic authority rather than upon leadership stature. In the final analysis the private in the ranks is left with no recourse except unprotesting subservience to the lowest bracket of officers. Assuming that the trainee will be under permanent organization personnel for the most part, I cannot but feel dubious of his benefits from these discipline contacts, for the obvious reason that men of strong leadership ability do not commonly fall into life careers as noncommissioned military officers. And should the misfit courses and teachers be "shown up" as Professor Keller assumes, by no means does it follow that they would be removed. Indeed, if one may judge by the degrees of incompetence tolerated by the inertia of the military machine in other parts of its mechanism even under war urgency, the probabilities of speedy adjustment of training schedules and personnel under a permanent peacetime training program seem remote.

"Intelligent discipline" is specified in Professor Keller's argument. Yet one may validly question whether there is, or can be, such a thing as intelligent discipline superimposed upon any large mass of adult or near-adult persons with heterogeneous backgrounds and abilities. Visit any large municipal court—an everyday laboratory of applied mass discipline—and you will find that the intelligence factor in the law is injected through *variations* in its application to fit the circumstances and the individual, rather than through rigidity. That the casting of all minds in one mold is not intelligent education long has been recognized. True, the swing toward unguided learning has gone too far in many cases, and educators now are returning to a greater degree of standardization in education as necessary to fit oncoming generations to the realities of our social structure. But it will be agreed, I think, that this standardization should be held to the minimum conformable with social necessity. Likewise, I feel it will be agreed that superimposed discipline as a principle in any part of our lives should be used only as necessity demands.

Recognizing the whole institution of militarism as a social aberration in a world not yet matured to international self-discipline, let us then recognize military discipline for what it is—a necessary ingredient of the military method, whose application we should seek to reduce as rapidly as conditions warrant.

Some Suggestions For Our National Defense. The impossiblity of stabilized military security in a war-minded world is easily demonstrable to any logical mind, since one nation's security automatically spells insecurity for all other nations against which that security is calculated. This article assumes that our first and greatest effort should be toward prevention of international friction as the only solid ground for international peace. But well-intentioned peace efforts have failed in the past and may fail again. So great is this risk that we are compelled to consider every means by which we may resist aggression.

The secret of America's strength in battle has had as many explanations as Sampson's: Our great manpower; our individual superiority, man for man; our

vast natural resources; our unrivaled industrial advancement; our immunity from attack; our unmatched inventiveness; our unity of purpose; and so on. Yet in each of these elements we have been equaled or surpassed by other nations. The real secret of our strength, the catalytic agent that has activated all the separate elements into an invincible combination, is the unfettered American mind.

In World War I we beat the German military machine, the perfection of generations of military planning, training, and preparation, by virtue of our freedom from the rigidity imposed by such a preparatory program. Because we were not bound to a preconceived pattern, we could create a more effective pattern. In World War II we beat the Axis military machine because the unfettered American mind outstripped the driven Axis mind in producing new and more effective engines of war. Our advantage in each case was merely a different facet of the same gem—greater freedom to think and act on individual initiative. And so long as the minds of men vary in capacity and resource, such freedom will remain an advantage over any conceivable regimented program.

It follows that our best defense against possible future aggressions lies in a course which will enhance this inherent advantage of democracy. Education for life will continue to be more productive of advancement than is education for death. Let us therefore hold fast to our traditions of universal liberal education. But let us broaden this education with a modernized view of social and economic relationships, from backyard to world scope. The emphasis on material production which has characterized our society in recent generations needs now to be shifted to a study of more equitable distribution of the fruits of the machine age. In a society which devotes over 90 percent of its time to its own operation and less than 10 percent of its time to producing the necessities of life, yet continues in devastating internal and international strife over allocation of these necessities, the need for more effort toward perfecting the social organization is evident. And when we recognize the industrial strikes now paralyzing America and Britain as symptoms of the same disease which produces wars, we see that there is plenty to be done here at home in strengthening ourselves against possible outside aggression. Be it not forgotten that it was internal strife rather than lack of military defense which made France so vulnerable to the Nazi blitz.

Upon the foregoing broad background of liberal tradition, universal education, and internal solidarity the following specific suggestions for our national defense are offered:

1. First essential for our future national defense, of course, is a unified defense command other than that now lodged in the President as Commander in Chief of both Army and Navy. Our Presidents are not elected for their military prowess, and even if the public at large could and would choose them on that basis, the other duties of the office would preclude adequate fulfillment of the military function. So widely has this point been argued in recent months that space will not be taken to discuss it here.

2. The pace of science in devising new instruments and techniques of war makes it utterly impractical to keep a large segment of the population adequately up-to-date in their uses. Equally impractical because of its inadequacy and rapid obsolescence is such partial training as might be given in a year of compulsory basic military training. On the other hand, an effective and not unduly costly program for providing the essential corps of thoroughly trained military personnel for defense emergencies would be to expand our West Point and Annapolis programs to several times their present size, at the

same time injecting into these programs such additional materials as might be necessary to turn out graduates fitted for various civilian pursuits closely related to military effort—engineers, chemists, transportation experts, disaster relief experts, public-health doctors, and nurses—to name a few. Let the graduates of this program in excess of the needs of our standing forces be given reserve commissions and required to keep them current with refresher training at suitable periods. Precedent for such a course was established by default during the two decades preceding World War II, when even the limited numbers graduated from our military and naval academies exceeded the needs of our standing forces and many graduates could not be given active commissions. A planned program along this line would seem better preparedness than the default situation, which may be expected to arise again when our armed forces are pared to peacetime needs.

The realities of total war—and any major conflict of the future must be expected to be total war—are such as strongly to commend such an arrangement. In total war total populations and total resources must be mobilized to the cause. This is not readily done by strictly military methods in a society essentially dedicated to peaceful pursuits and traditionally steeped in liberal democratic government. In war the need arises for large numbers of persons familiar with both civilian and military needs and capable of integrating the two with greatest over-all efficiency, for the military machine necessarily is powered by civilian effort. The more complex our society grows the more essential becomes the military-civilian liaison function. For this reason it may well be argued that a deliberate policy of injecting militarily trained personnel into key civilian pursuits in peacetime is essential to any realistic preparedness program. In fact, a strong case could be made for requiring even Army and Navy career men to spend one year in five in a civilian occupation, to keep them acquainted with the realities of the social organization from which their strength must come in case of war.

3. Any preparedness program which fails to include women in large numbers ignores the facts of total war so recently demonstrated to us. Behind-the-lines activities of armed forces more and more can be taken over by women as the use of machines and instruments increases. At no time in this war did our women fill all, or nearly all, the military positions they might have filled equally as well as men. Especially critical was the shortage of nurses. Since nurses cannot be trained quickly, and since future aggressions may be expected to strike without warning and on a very broad scale, the presence throughout the nation of large numbers of trained nurses commends itself as an essential of national-defense preparedness.

4. An adequate peacetime public-health program embodying currently known principles of preventive medicine and personal hygiene could make profitable use of the women so trained. Such a program, fully justifiable for its own sake, must be included among the essentials of national-defense preparedness. Selective Service reports show that for every three men inducted into our armed forces one was rejected for physical, mental, educational, or moral reasons. A very large share of these rejections could have been avoided by a program of preventive medicine, child care, accident prevention, and truly population-wide education applied through the years preceding military age. And another war, more devastating and draining than the last, may not find us with a 25 percent margin of manpower not needed in actual combat.

Much of the needed public-health pro-

gram could and should be accomplished in the schools. The competitive sports which comprise almost the total physical development program of most of our schools are good in themselves when not overdone. But too few students participate in these sports, and too many participants sustain disabling injuries. College sport participation is coming to be the almost exclusive realm of professional athletes, themselves often permanently disabled by too strenuous action. From primary grades through college our physical education program is in need of drastic overhauling. A candid view of our probable future defense requirements calls for thorough hygienic education and bodily development of *all* persons of both sexes not incapacitated by insurmountable deficiencies. The methods for doing this are well known; all that lacks is their proper application.

5. Universal health and strength are not enough. An understanding of world forces is needed both to forestall possible wars and to defend ourselves if attacked. Science has so far outstripped the humanities that we now find ourselves with no dependable approach to the problems in class and international conflict brought on by development of unlimited capacity for production of goods and services. The great need now and for the future is to close this gap between scientific advancement and advancement in human relationships. To this end it it here suggested that our whole educational curriculum be redrafted to direct emphasis upon world problems. There need be no concern for the future of science under such a program, for science may be expected to continue or accelerate its current rate of discovery and invention for a long period to come, without other encouragement than a free hand in research. The real issue is that the products of science are pointless if kept in the laboratory; whereas, once out of the laboratory, they may do far more harm than good if exploited by unscrupulous or unenlightened leaders placed in power by peoples unaware of ultimate results to be expected from current measures and policies. Was not Nazi Germany an example of this outcome? And is not our present industrial paralysis a symptom of the same organic weakness?

Ten years ago Alexis Carrel, in his *Man the Unknown,* suggested that soon significant advancement in our knowledge of man as an organism might require as much as 30 years of intensive preparation by the most able scholars in many related fields of science. Is it too much to suggest that in the field of human relationships, already far behind scientific developments, we could well afford to encourage and direct similar intensive preparation of some of our best minds for devotion to national and world improvement in government, economics, sociology, and religion? Certainly we should take more positive measures than we do now toward intelligent preparation of intelligent men and women for places in public life, especially of those who are to represent us abroad. If internal strength, international prestige, and international goodwill be elements of national security, then added emphasis upon study of the humanities is one of our "musts" of future national defense.

Specifically, the indigestible mixture of statistics, irrelevant chronologies, and romanticized military campaigns now taught as history should be replaced with a realistic summary of man's circuitous "progress" through the various forms of government and social structure, philosophy, religion, material culture, etc., with a deadly earnest effort to interpret the cause-and-effect relationship behind their respective ascendencies, conflicts, and collapses. Geography should be raised from travelogue scope to its rightful stature as a study of natural resources, climates, peoples, and cultures each reacting upon the other to affect

the destiny of nations and the course of history, past and future. The languages of at least half a dozen leading nations besides our own should be widely taught throughout our school structure, and mastery of at least one of them required of every college graduate. World-wide international student exchanges should be encouraged—subsidized if necessary—to the point of assuring greatly improved mutual understanding with all nations.

If such a program requires more years of education, so be it. We can well afford them. Further, it is here proposed that the time has fully come when we should expand our compulsory education requirements to include high-school level. Our forefathers pioneered universal education as one of the fundamentals of sound democracy. Today we are in danger of losing that democracy through the votes of those who leave school without any real understanding of our present society, now vastly more complex than in the days when the three R's were the fundamentals of our culture. Regardless of whether we increase our compulsory education requirements, we should make wide use of newly perfected aptitude screening tests to develop a channeling program for students above grade-school level to assure as far as possible that each will spend his time in a field promising to be profitable both to him and to society.

7. For investigations costly beyond the scope of private and state research institutions, such as the controlled use of atomic energy, where probable national benefit promises to justify the expenditure, federal subsidies should be provided. Much research in aeronautics, electronics, and other war-vital fields also probably will require federal support for rapid progress. That such support should be given where needed is so obvious as to be given only passing mention here. Government controls over the research so supported, however, should be scrupulously limited to reasonable accounting of funds. Direction of the research itself must be left to suitable committees of leading scientific workers.

8. In event of future wars, greater care than heretofore should be given to retaining in their respective fields the largest possible number of scientific workers and students, medical students, skilled technicians, etc. The traditional dictum of our democracy that in time of war all are subject to the battle call, regardless of station, loses its validity when a literal application threatens the ability of the nation to win that battle, or to maintain its ascendency after the battle is won. Careful peacetime studies of the probable war necessity of every branch of science should be made by men qualified to judge. Likewise, ratings may be placed upon individual scientific workers on the basis of intelligence tests, college records, and subsequent work, with a view to determining degrees of dispensability in a war emergency. Similar records might be compiled to cover workers in other essential technical or professional fields requiring long periods of preparation. Consultation of these records in wartime placement of personnel would avoid unnecessary training programs and many costly delays in manning war activities.

SCIENCE ON THE MARCH

NOTES ON THE SOLAR ECLIPSE OF JULY 9, 1945

REPORTS are still drifting in on the results of the solar eclipse of July 9, 1945. The path of the moon's shadow stretched from Boise, Idaho, through central Canada across Hudson Bay into Greenland, and across the Scandinavian Peninsula into Asia. In spite of war conditions, professional and amateur astronomers stood watch in this country and in Canada along the central line. The eclipse watchers met with various degrees of luck in observing weather. A New York party found clear weather at a station overlooking Basin Creek Valley near Butte, Mont. In the same state at Malta under partly clouded skies, Princeton's Professor John Q. Stewart and General Electric's James Stokley, with others in their group, made 36 photographs of the corona and the moon's shadow during the 30 seconds of totality allotted. Dr. Walter T. Whitney, of Pomona College, was partly successful in spite of threatening clouds at Opheim, Mont. Amateur astronomers reported fair weather at Francis in Saskatchewan, and the astronomers at Wolseley, Saskatchewan, including Dr. Roy K. Marshall, of the Franklin Institute, and Dr. Orren Mohler, of the McMath-Hulbert Observatory, reported perfect visibility following early morning clouds.

An elaborately equipped party under the leadership of Lt. Comdr. Donald H. Menzel reported that clouds prevented a view of any part of the eclipse. Also clouded out were Dr. Charles H. Smiley, of Brown University, and Dr. C. H. Gingrich, of Carleton College. They were located at Roblin, Manitoba, along with Professor C. M. Huffer, of Beloit College, and T. J. Bartlett, of Northwestern. At Pine River a party from the Yerkes Observatory was successful in their long-focus photographs of the eclipsed sun. The corona has been described, by those who were successful in seeing it, as of the sunspot-minimum type with extended streamers emanating from the solar equatorial zone.

Unique among the reported observations were recordings of atmospheric electric effects made at God's Lake, Hudson Bay. J. T. Wilson reports results in the September issue of the Allis-Chalmers *Electrical Review*. In addition to employing photographic and meteorological equipment, special provision was made for observing measurements of the atmospheric potential gradient before, during, and after the eclipse. By means of an electrometer in connection with a collector they reported not only an abrupt change in the potential gradient but also an actual reversal in sign accompanying or shortly following totality. While before totality the potential gradient of the atmosphere registered steadily about 150 volts negative, within 3 minutes after the duration of the total phase the gradient dropped rapidly to 0, reversed in sign and actually attained about 75 volts positive; after which it again returned to the apparently normal pre-eclipse value.

These observations are particularly significant since during many recent eclipses ionospheric observations have shown a marked drop in the ionization of the upper atmosphere, immediately accompanying envelopment in the moon's shadow. These previously well-recognized results have substantiated the belief that the principal ionizing agent for the radio-reflecting layers consists of ultraviolet radiation from the sun. Studies of aurorae by Störmer and others have indicated the existence of electrons or charged corpuscles emanating from the sun as a supplementary source

of ionization in the earth's atmosphere. Search has been made at various eclipses to detect the effect of a corpuscular radiation from the sun on the presumption that such charged particles travel much more slowly than the velocity of light. Results have been conflicting and thus far have failed to prove conclusively that the screening of such radiation by the moon could be detected by radio means.

The results of the observers at God's Lake, therefore, are of much interest, especially since their readings indicated a lag of 3 minutes between totality and the beginning of the fall in the atmospheric potential at the earth's surface. Were this potential, in part, maintained by charged particles emitted from the sun, then this time lag might be interpreted as an index of the velocity of such particles in the earth's atmosphere. On such an assumption one might reason that corpuscles travel at such a velocity as to consume 3 minutes in passing from the moon to the earth, since no change was observed until 3 minutes had elapsed after the moon had effectively screened all radiation of the sun from the shadow cone. On the other hand, if ionization of the lower atmosphere, which is related to conductivity and potential, were to be regarded as a progressive effect from the top of the atmosphere down, then such reasoning would not necessarily hold.

Unfortunately, there are other matters to be considered in interpreting the change observed in atmospheric potential during the eclipse. The weather was in general unfavorable with passing clouds. Since clouds generally carry charges of electricity, one cannot exclude altogether the possibility of their effect in the recorded readings of the electrometer. However, clouds had been passing before totality and no such effects were observed. It seems rather unreasonable to suppose that the effects recorded were due to peculiar cloud formations which occurred just after totality. It is to be noted that nearly 40 minutes elapsed following the eclipse before the atmospheric potential returned to normal. Whatever may be the correct interpretation of the atmospheric electric observations made at God's Lake, new interest has been added to future solar eclipses.

More and more we are impressed with the important part which radiation of the sun plays in maintaining the electrical state of the earth's atmosphere. Every solar eclipse gives opportunity for a study of such changes as may take place under the unique conditions staged by nature when, within the shadow of the moon, all solar radiation is suddenly screened by the interposition of our satellite. It may be noted that coming eclipses will occur during a higher degree of sunspot activity. Such effects as were observed at God's Lake and effects readily observable by radio soundings of the ionosphere may be much accentuated with increasing solar activity.

HARLAN T. STETSON

GERONTOLOGY COMES OF AGE

AGING is as old as time. But gerontology, the science of aging, is very young. Until recently biologists and physicians have been strangely content to take aging as a matter of course. Only poets and philosophers wrote about old age. Scientific interest in aging has been more active about the fringe of this vast and unexplored continent of thought; astronomers have been concerned with calculating the age of the universe, geologists have long worried over the age of the earth, and anthropologists and archeologists have struggled to estimate the age of man as a species. But the aging of men and women as *individuals* has received scant attention until recently, and very little indeed is known anent the basic mechanisms of aging as a biological process.

Man is a utilitarian creature, and few

indeed are those scientists who seek understanding with purely abstract curiosity. Fewer still are those who encourage and finance pure research free of any expectation of practical application. Until recently the problems of aging presented largely academic and theoretical interest. This is now changed. Today there is real urgency in the need to know more, much more, about aging. In the past fifty years preventive medicine, sanitation, and improved therapeutics have raised dramatically the age of our population. This increase in age continues and in fact was accelerated in the past decade.

At the turn of the century the average life expectancy at birth was but 47 years; today it exceeds 63. In 1900 only 17 percent of the total population of the United States were 45 years or more. In 1940, 26.5 percent were over 45, and conservative projection leads us to expect that in 1980—ony 40 years hence, which is not long in the life of a nation, although it may seem long to an individual —more than 40 percent of our people will be over 45. Data from the 1940 census reveal that the population of the United States as a whole increased 7.2 percent since 1930, but that the number of persons aged 65 or more increased 35 percent in this past decade. There are now in this country about 9 million people who are 65 or more. It is predicted that this fraction of our population will more than double in the next forty years.

Obviously, the aged are here; there will be many more of them in the years to come. This older fraction of our population represents an immense but thus far largely unutilized and unappreciated resource. The increasing millions of older men and women will remain an urgent problem and a potential menace to national economic equilibrium until we know enough about aging to maintain health into senility and to use wisely the changed capacities of those past the meridian.

Gerontology, like all Gaul, is divided into three parts. The divisions are intimately and inseparably related, although they involve widely differing disciplines. All living matter ages. Thus the study of aging requires correlated cooperation between the many branches of biological and physical sicences. These interrelationships are pragmatic as well as theoretical; advances in knowledge in any one field depend largely upon parallel or preceding progress in the other categories.

The logical divisions of gerontology are:

(1) The biology of aging.
    Phenomena of evolution or development.
    Phenomena of involution or senescence.
(2) The clinical problems of aging man.
    Pediatrics.
    Geriatrics.
        Normal senescence and senility.
        Diseases of the senescent period.
(3) The socio-economic problems of aging mankind.

Gerontology is growing up. For several years a group of scientists, each busy working in his own field of study and applying highly specialized techniques to some aspect of aging, has felt the need for some medium by which the various observations concerning aging could be correlated and made more meaningful. This small group, meeting annually for the past six years as the Club for Research on Aging, has now extended the scope of its activities by forming the Gerontological Society, Inc., and conceiving the *Journal of Gerontology,* the first issue of which is to appear this month. The Journal will be published by Charles Thomas, Inc., of Springfield, Ill., for the Society.

The *Journal of Gerontology* is more than merely another scientific periodical. It is unique in the breadth of its field, for it will try to present and correlate all

three of the major subdivisions of gerontology. Man is the core of interest. Therefore clinical geriatrics will receive special prominence. But man is composed of myriads of cells functioning in complex harmony. The fundamentals of the biology of senescence form the basis for advances in clinical knowledge and application. Accordingly, the biological sciences are recognized as being highly significant. Similarly, men and women constitute the basic units of collective society or the body politic. Sociology, biology, and medicine constitute the triad of facets. It will be a major function of the *Journal of Gerontology* to correlate these three viewpoints into a unified whole.

The editor-in-chief, Dr. Robert A. Moore, of the Department of Pathology, Washington University, St. Louis, has a herculean task of orchestration before him. However, the first issue of this new journal demonstrates his capacity as conductor. Dr. Moore has obtained the assistance of an extensive and widely recognized group of editorial advisers; consequently no subdivisions of the field will be neglected. A wholly original innovation will be the publication of a nontechnical supplement with each issue. The supplement will contain carefully prepared digests of all the articles in the main journal, written in nontechnical language so that the sociologist may understand the chemist, the anatomist the psychiatrist, or the aged themselves the whole conglomerate orchestra. It will be more than interesting to observe the effectiveness and development of this completely new editorial concept.

The future of gerontology is bright. There is much to be learned about aging and the aged. And we need to learn *now*. Gerontology is now of age. It will continue to mature, as do we all. If its further maturation be wisely guided, mankind will be enabled to make good use of the increased longevity which is already here. Let us not forget that it remains for mature senescents to explore fully and develop all the wondrous potentialities of man himself.

EDWARD J. STIEGLITZ

BOOK REVIEWS

G.I. SNAKES

Reptiles of the Pacific World. Arthur Loveridge. 259 pp. illus. 1945. $3.00. The Macmillan Co., New York.

MR. LOVERIDGE was given a difficult task when he was assigned to cover the reptiles of the Pacific World. The very title gives a hint of the territory covered —from the Galapagos west! It sounds like the charters of our early Colonies. His next hurdle was the time element— the book had to appear in time to be of use to the G.I. overseas. Another handicap—his personal field experience had been in Africa. Add to this that many of his sources of reference were old enough to be almost legendary. Only limited parts of the area had been covered by the reports of herpetologists— great spaces remained to be filled in by study of relatively few museum specimens. Lastly and most important, he was not writing for an audience of herpetologists or even for beginners in herpetology—he had to make the book so interesting that it would recruit followers for herpetology, or for any branch of natural history for that matter.

The result is a great success and it was cleverly accomplished. He uses the inimitable style of Ivan Sanderson, making the best of the exciting size of overlarge crocodiles and snakes and stressing the gruesome data of their liking for human flesh. He uses keys to an extent sufficient to acquaint the beginner with their use without boring him. The plates are clear, well-drawn, and serve as a glossary. He coins the word "scansor," meaning "climber"—a welcome addition to the herpetologist's vocabulary, but his use of "cadaverous" for feeding on corpses is too much of an innovation. Had he coined "cadavorous" it might have passed in lieu of "necrophagus," the word usually employed to express his meaning.

The body of the book first acquaints us with how reptiles arrived on these isolated specks of land. Some of us may not visualize Galapagos tortoises "floating" from one island to another, but like the man who was contradicted when he stated that in his country buffaloes climbed trees, his challenger had to admit that he had never seen one try. He makes clear what is meant by the word "reptile." In sequence he takes up turtles, crocodiles, lizards and snakes. These are treated so that anyone having read the book will have a good idea of at least what genus he is looking at or where to look it up. Among the closing chapters he treats of snake bite in such a manner as to remove the hysteria and inject common sense. Having abated the unnecessary fear of poison, he logically takes up the beneficial aspect of reptiles as destroyers of vermin.

The title of the book does not include Amphibia, but Mr. Loveridge gratuitously inserts an interesting survey of that subject so that the reader obtains a good idea of the amphibia he may meet in the Pacific World.

Throughout the book he mentions localities in which to search for species desired by museums. The last chapter treats of the methods for collecting, preserving, and shipping specimens to museums. The theme of being able to kill and at the same time advance science appeals to a G.I. as well as to a big game hunter.

A distribution chart is valuable as a preliminary check in that it eliminates and reduces possibilities for identification. A very good selection of works on oriental herpetology published in English is appended for those who may wish to pursue the study.

The book closes with what is called an "Index to, and a systematic list of, species mentioned." This is really not an

index since it is not alphabetically arranged, but a table of contents of chapters 3 to 9 inclusive inserted at the back of the book. It takes the place of an index for a person familiar with the book.

A small but unabridged edition was published in handbook form for distribution to the Armed Services. The Editor of the *Infantry Journal* writes me: "Several hundred thousand of the Pacific Series have been printed and most of these have been distributed to the Armed Forces." Thus Mr. Loveridge's book probably has had the greatest distribution of any book ever written on herpetology.

CHAPMAN GRANT

THE USE OF PSYCHOLOGY AND PSYCHIATRY IN WAR

Psychology for the Armed Services. Edited by Edwin G. Boring. 533 pp. Illus. 1945. $3.00. The Infantry Journal. Washington, D. C.

PSYCHOLOGY, like other sciences, has many uses in war. That statement appears so obvious it should be hardly necessary to labor the point. Even a superficial survey would reveal plentiful evidence of psychology's contribution towards the solution of countless military problems. Yet, there is great need for a well-organized presentation of the application of psychological principles to war situations. Necessarily, a book attempting to cover the subject should address itself to military and naval personnel generally, and to leaders of men in the armed forces especially. For while it is almost self-evident to psychologists that the possible ways in which the science can be applied are numerous, experience in two World Wars has shown that military leaders are unconvinced or unenlightened in that respect. There is therefore a definite need for a lucid enumeration and thorough discussion of the uses of psychology in wartime.

Towards the fulfillment of that need, this book was prepared by a Subcommittee of the National Research Council. The members of the Subcommittee and the contributors to various sections of the book comprise a body of outstanding experts in psychology. With few exceptions, the text is aptly illustrated. Diagrams, tables, and charts are ample and well-planned. A select list of references follows each chapter. The result is a handy, well-written textbook nicely suited to the college level.

The subjects, for the most part, are treated in a practical manner. The physiology of the sense organs is summarized and illustrated. The applications to military situations are enumerated in discussions of night vision, camouflage, war gases, efficiency, and fatigue. Certain topics, on the other hand, are handled with kid gloves. It seems as though here and there a punch is being pulled. This is especially noticeable in the treatment of the problem of fraternization with enemy peoples. The discussion sounds very much like an Army directive recast in psychological terminology. Here one feels that psychology has made no real contribution but has been content to follow established military policy. Sometimes topics are introduced in a roundabout and pointlessly involved fashion. For example, the subject of leadership is approached by a discussion of hypnosis. According to the author, the relationship between hypnotist and subject is an "extreme leadership relation." That is a highly debatable question. Certainly the argument presented for it seems forced. At any rate, the leadership attributes listed bear little or no relationship to what might be the attributes of the hypnotist, which in turn will depend on the theory of hypnotism one supports. Despite such occasional lapses, we can agree with the prefatory claim that this isn't simply a war book,

but a work of broad and continuing military usefulness. It should prove of value to men in positions of responsibility and leadership in all the services. It can be recommended for use in military or naval academies. Projects of this nature are necessary steps in attaining the goal of a military leadership intelligently aware of the most effective and appropriate utilizations of psychological services.

Psychiatry in Modern Warfare. Edward A. Strecker and Kenneth E. Appel. 88 pp. 1945. $1.50. The Macmillan Company. New York.

In the first World War, one out of every seven battle casualties was neuropsychiatric; in World War II, the proportion is one out of three, or about thirty percent. The increased frequency, therefore, poses a problem of paramount concern to military authorities, psychiatrists, and educators. Adequate discussion of the problem necessitates an examination of the nature of warfare in the two World Wars and evaluation of the psychological consequences of warfare. Also necessary, or at least desirable, would be a comparison between the two wars as regards the organization of military psychiatry, diagnostic and therapeutic procedures, and the incidence of psychiatric conditions in each war. The authors have combined their intimate knowledge of military psychiatric practices to present, in this brief treatise, an admirable study of psychiatry in the two wars which more than meets the minimum requirements for handling satisfactorily a subject of such scope. Part I contrasts the nature of warfare in World Wars I and II and considers the psychological effects on combatants and noncombatants. It appraises the record of psychiatry in each war with reference to organization, statistics, etiology, symptomatology, psychopathology, treatment, prognosis, and incidence. The second half of this concisely written survey discusses the more important readjustment problems facing the returning veteran. Under the general heading of Demobilization are considered such aspects as the need for readjustment among civilians and veterans alike, special difficulties confronting the veteran, concrete measures designed to help in the process, and an enumeration of the suggested improvements in facilities for veterans. This second part is a more popular presentation of the general subject.

Such concentrated treatment of a complex subject, as the study accomplishes so well, is bound to skip some phases or suggest them only by implication. Thus, the authors state, "It is interesting to speculate as to why conversion hysteria is relatively infrequent in World War II as compared with World War I, and why anxiety reactions have so frequently replaced it." The speculations that follow explain the incidence of anxiety reactions in World War II without discussing the greater incidence of conversion hysteria in World War I. The anxiety reactions are ascribed to "increased stimulation, alertness, . . . tension of the organism; . . . lessened security, increased fear." These are the psychological consequences of global warfare. Apparently, there is a tendency to ignore the possibility that these psychological effects occupied parallel roles for the soldiers of World Wars I and II. In fact, these factors may be psychologically parallel for the combatants of all wars. Why, then, was conversion hysteria more frequent than anxiety reactions in World War I? Have diagnostic criteria undergone some subtle modifications which might account for the difference? These aspects of the problem have not been covered.

However, the positive merits of the study are so many, that any final evaluation must judge it to represent one of

the most helpful sources of reference pertaining to the record of neuropsychiatry in the two world wars. Few readers will fail to find much of practical interest in this handy survey. All, undoubtedly, will gain the impression of having been given a rich and comprehensive insight into the problem of neuropsychiatric battle casualties.

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CATALYSTS IN ACTION

Catalytic Chemistry. Henry William Lohse. 471 pp. Illus. $8.50. Chemical Publishing Co., Inc. Brooklyn.

PRODUCERS of desirable substances from the time of the alchemists, who sought the philosopher's stone, to modern man's endeavor to attain artificial photosynthesis have continued to search for accelerators of production.

The number of workers in this field of research is becoming so great and the published results of their achievements are so voluminous that it becomes almost imperative that from time to time some worker in the field pause long enough to make a survey of findings and abstract them for himself and others. Such a service the author of the book under review has performed. His author index of over 1000 names and footnote references in equal or greater numbers testify to the magnitude of his labors.

His program of presentation is in five chapters entitled: Brief History, Catalytic Theory, Nature and Properties of Catalysts, Specific Types of Catalytic Reactions, and finally Industrial Catalytic Reactions. In the second chapter he groups the theories under the captions of those that have to do with homogeneous and those that have to do with heterogeneous reactions. The subdivisions under the first include: chain reactions, vapor and gas reactions, liquid phase reactions, acid-base catalysts, salt, and solvent effects, hydrolytic and finally intermolecular changes. Under the heterogeneous grouping are: phases, adsorption, desorption, activity, selectivity, surface specificity, activation energy, heterogeneous chain and wall reactions.

The nature and properties of catalysts are systematically treated for the elements and compounds in their relation to their classification in the eight groups of the Moseley-Mendeleff periodic chart.

The specific types of catalytic reactions are listed in chapter four as: oxidation, dehydrogenation, cyclization, hydrogenation, hydration, hydrolysis, dehydration, esterification, halogenation, alkylation, isomerization, condensation, polymerization, sulfonation, desulfurization, amination, ammonolysis, enzymes and organic catalysts.

Obviously not all catalytic reactions of industrial interest can be included in a one-volume digest. Those considered of major concern, by the author, are: the industries of nitrogen fixation; those stemming from acetylene derivatives; industry related to carbon oxides and "water gas," industrial alcohol, hydrocarbon cracking and coal hydrogenation, synthetic rubber, processing vegetable and animal fats including dehydration and hydrolysis of oils and fats.

Aside from a brief nine-page gesture toward enzymes and organic catalysts the volume could be considered as exclusively concerned with inorganic catalysts. However, a strict reminder should be voiced that a major number of the present uses of catalysts is in the field of organic syntheses. Dr. Lohse might, with propriety, have alluded in his preface to the growing use of organic and enzyme catalysts in the industrial field as well as in natural biochemical processes. Is it in order to suggest the need of a volume devoted to progress in that phase of catalysis as a companion to the one under review?

The reviewer is quite convinced that

an interested student or investigator will not find elsewhere so much, in so brief a span, devoted to the field of catalysis. It is highly gratifying to find an adequate thirty-four page subject index as a part of its offering. It is regrettable, however, that so desirable a working tool should be kept away from the work tables of everyone interested in catalysis by a price which is about double what it should be.

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MAN—HIS SUCCESSES AND FAILURES

Modern Man is Obsolete. Norman Cousins. 59 pp. 1945. $3.00. The Macmillan Company, New York.

Between Two Wars: The Failure of Education, 1920–1940. Porter Sargent. 608 pp. 1945. $5.00. Porter Sargent, Boston, Mass.

A State University Surveys the Humanities. Edited by Loren C. McKinney, Nicholson B. Adams, and Harry K. Russell. 365 pp. 1945. $4.00. The University of North Carolina Press, Chapel Hill.

These three volumes emphasize the urgency of adapting designs of social living to scientific fact.

Modern man is obsolete, says Cousins. By surrounding and confounding himself with gaps between "revolutionary technology and evolutionary man, between cosmic gadgets and human wisdom, between intellect and conscience, he has made himself an anachronism." To span the gap man must become cooperative instead of viciously competitive, must develop critical intelligence, and establish a common world sovereignty expressed through world government. What the world needs, when time no longer works for peace, is not another conference but a Constitutional Convention to organize man's social institutions to meet the range and tempo of scientific achievements.

Between Two Wars is a typical "Sargentesque" manifesto to educators to "maintain conscience against the insiduous conditioning to our atavistic ideas of sovereignty." It is a declaration of freedom from "fixed traditional practices" which condition each new generation to make "good citizens" by blind loyalty to those in authority. Sargent wants teachers who can "start the pupils' internal combustion engine without the present-day sputtering and backfiring." He wants students to discover their own self-starter and "freed from the chains dragging from the past, with fluid drive, with clearly seen objectives ahead," . . . to "take to the road onward and upward." With the exception of the concluding chapter the volume is a reprint of Sargent's annual prefaces in *The Handbook of Private Schools.*

One of a series of volumes by divisional faculties to appraise the condition of the University of North Carolina on the occasion of its 150th anniversary, *A State University Surveys the Humanities* presents essays outlining what some professors understand by the leavening influence of the humanities in fields ranging from history through medicine and even journalism. While the chapters vary in quality as in the case in most symposia, the common theme is that the humanities are not a group of traditionally liberal studies, but "a timeless ideal that unifies, inspires, and invigorates the work of the scientists, artists, specialists, professional men, and workers, permeating all human activity and exalting the dignity of man's personality." The editors define the humanistic ideal as "a broad view of man's struggle to place himself in his environment," offering a noble tradition to guide him in his development. The volume ends with a pedestrian performance by Norman Foerster in which he sets forth the "Great Plan" for liberal education, based on a sweeping pattern of "The Great Curriculum," "The Great

Faculty," and "The Great Administration."

Cousins' volume is a challenge to social and political scientists to meet the issue set by natural scientists before it is too late. The other two volumes leave the reader with the wish that the use of a dispassionate and objective method of scientific inquiry, the courage to accept as valid the results of experiment, and the submission of logical proposition to empirical verification might be more widely used in dealing with our problems of group living in an age of atomic energy.

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A CRITICAL PERIOD IN AMERICAN BOTANY

American botany, 1873–1892: decades of transition. Andrew Denny Rodgers III. 340 pp. 1944. $3.75. Princeton University Press, Princeton, N. J.

THIS is the fourth of a series of works in which the author is presenting, with vastly more detail than has ever been attempted before, material for a history of the development of botany in the United States. The volumes have not followed one another in the chronological order of their subject matter. The first, *Noble fellow: William Starling Sullivant* (1940), stands somewhat apart both in its subject and in the method of its development. Sullivant, America's first real bryologist and still its greatest name in that branch of botany, was the author's ancestor, and the substance of the book is nearly as much historical and genealogical as botanical. There is no mention of botany in the first hundred pages, and Sullivant's interests were so nearly confined to mosses that the work is rather an account of the early development of bryology in this country than a history of its botany.

The second, *John Torrey: a story of North American botany* (1942), is in some ways the most successful of the author's four volumes. During the period it covers, essentially the years from about 1817 to 1873, "botany" in America meant taxonomy and floristics and little else. For much of that time Torrey was the recognized leader of North American botanists, in contact with practically all the active writers, collectors, and explorers, and intimately concerned in the elaboration of the material brought back by the numerous and important Government surveys in the western states, so that this account of his work gives a fairly comprehensive view of the development of botany in the United States during the period it covers.

The third volume, *John Merle Coulter: missionary of science* (1944), treats in detail of Coulter's botanical activities and teaching during more than half a century. Although his significant work began on his trips with Hayden's surveys in 1872 and 1873, just at the time Torrey's was closing, the field soon became so much broader and the number of botanists so much greater that the picture of botanical work with Coulter as a center here presented lacks the comparative completeness of the volume on Torrey.

The fourth of the series, *American botany, 1873–1892: decades of transition* (1944), naturally revolves about Asa Gray, with the paleobotanist Leo Lesquereux as the next most important figure, and Engelmann and Parry not far behind. Practically all the prominent and most of the minor botanists of the time are mentioned in its pages. Of its 14 chapters, 6 are concerned mostly with botanical expeditions and field work performed by many botanists; 1 with the establishment of botanical and agricultural laboratories and stations; 1 with paleobotany; 1 with a survey of Gray's work in fields other than taxonomy; 1 with the Gray-Greene controversy; and

4 are decidedly miscellaneous in character.

The book begins with an estimate of Gray's position in American botany after the death of Torrey, mention of many of the active workers of that date, and a discussion of Gray's activities or interests in such fields as evolution, heredity, insectivorous and climbing plants, and so on. The second, third, fourth, fifth, sixth, and twelfth chapters deal with the work of Government surveys and private collectors, particularly in the western states, but also in the southern United States, Canada, Mexico, and Central and South America. Lesquereux, Coulter, Rothrock, Parry, Greene, Palmer, Pringle, Brewer, Lemmon, the Parishes, Jones, Gattinger, Garber, Chapman, Curtiss, Mohr, Fendler, and Rusby are among the botanists mentioned, with copious extracts from letters. A long chapter, almost the only unified one in the book, is assigned to Lesquereux's investigations in fossil botany. The transition in the teaching of botany from the older purely taxonomic study to the wider curriculum inspired by European textbooks and graduate studies—a change in which Rothrock, Bessey, Beals, Farlow, and Goodale, only the last two of whom had European training, were especially concerned—is described in the tenth chapter, along with the establishment of experiment stations and the work of L. H. Bailey in scientific horticulture. A rather short but interesting chapter, with hitherto unpublished extracts from letters that passed between the three men, is devoted to Greene's controversy with Gray and Sereno Watson.

The index, which occupies 18 pages, seems very complete as to persons, localities, and institutions, although not so as to titles of publications cited in the text. A large amount of previously unpublished material from letters is incorporated in this as in the author's previous volumes. All of them would have been improved by competent editing.

Mr. Rodgers' four volumes are notable contributions of material for the history of North American botany that still remains to be written. In themselves they are too unsystematically arranged, too full of unessential detail, too lacking in trained botanical perspective, to qualify for this title. But until that history is written, they are likely to remain the outstanding source of information for it.

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FREEDOM OF RESEARCH

Science and the Planned State. Dr. John R. Baker. London. George Allen & Unwin Ltd. 1945. pp. 120. 7/6 net.

Professor Baker's book is perhaps even more important now than when it was written, for a couple of atomic bombs have been dropped on Japan, and the vernacular press, supplied with press releases by the politicians, is representing the development of the bomb as if it were the main line of progress and legitimate offspring in the field of radioactivity or nuclear physics, instead of a grotesque and disgusting aberration and abortion. This supreme manifestation of malevolence, the atomic bomb, has confirmed the ill-informed public in its appraisal of the scientist so long presented to them by the comic strip and dime dreadful. The scientist is essentially a fiend, who studies science, not for its own intrinsic interest, but solely for the purpose of acquiring control over men. According to the comics, the chief interest of the scientist is the destruction of the human race, and devising tortures for it. Professor Baker believes the man in the street is friendly to science and scientists. "The scientist is likely to be able to influence the mass of humanity more easily than the philosopher or the historian, because people tend to suspect

philosophy and ignore history, while trusting and respecting science" (p. 99). This optimistic appraisal of the popular attitude towards scientists was surely a very inaccurate judgment before Hiroshima, and is even more inaccurate today.

The politicians are playing up another angle: "Look," they say, "what a band of regimented scientists can do, when diverted from their frivolous pastimes of using radioactivity to date the strata of the earth, to modify chromosomes, to alleviate cancer, or make the faces of clocks shine by night. All we have to do is to provide a few billion dollars of taxpayers' money, pen the scientists up, and the loveliest bombs materialize. This shows that scientists ought to be under the control of politicians, because otherwise bombs might not be forthcoming, or, worse yet, the scientists might produce bombs not at the disposal of the political authorities." Arguments on these lines, to frighten the people into clamping vicious controls upon scientists, are now more insistent than when Professor Baker's book was written.

His book should be widely read, though the reviewer cannot concede that it is a very successful piece of writing. Although the subject matter is sufficiently clear that it should make possible a single forceful volume, it has not done so in Professor Baker's hands. The start is auspicious. The first three chapters go well. Here Baker is appraising accurately and understandingly the attitude of a true scientist toward the study of nature. The remaining two chapters must frankly be regarded as a detraction from the first three, and as leaving the reader with a feeling of uneasiness, not at the man-eaters to which Baker is pointing, but as to the ability of Baker to line up his gun with his target.

Chapter IV, "Science under Totalitarianism," is in large measure a denunciation of Russian control of its scientists. Much as most of us detest the whole Russian philosophy, and willingly though we might applaud a clear analysis of it, and of its effects, we feel that the author has not been too successful. Perhaps his personal knowledge in this field is not so good as in the field of his first three chapters; perhaps, however, it is that no man is emotionally equipped to sing a Schubert love-song and follow it immediately and convincingly with the German "Hymn of Hate." Or is it fair to expect a single author to write both the fourteenth chapter of St. John and a Catiline oration? And if he tries, is it fair to the reader to expect him to read them one after the other? And though Lenin be, as many of us think, as much a traitor as Catiline, and totalitarianism in Russia, as in Germany and Italy, a barbarous and vicious thing, yet Baker lacks the fire of Cicero and the analytical subtlety of Mark Antony, and the result is inadequate.

If the fourth chapter is unsatisfactory, the fifth and last is still more so. Here we have a potpourri of ideas, concerned indeed with the general subject matter of the ethics and social duties of the scientist, but having little to do with the Planned State as such. The reader feels the treatise petering out as he reads on: he wonders whether the chapter is put in to pack the covers of the book apart, and the good effect of the first three chapters, and the modest effect of the fourth, is largely dissipated.

Professor Baker could have written half a dozen valuable essays, and published them in the periodicals. Instead he has produced a rather rambling discourse, which is neither homogeneous nor frankly divided into separate items. The main subject matter, however, is so important, and the writings on the subject so few, that the reviewer must commend the author's efforts, and urge a reading of the book upon all who are interested in the future of science.

F. W. PRESTON

VALEDICTION

THEODORE HENRY FRISON

ONE day in the late summer of 1944 a charming and distinguished visitor came to the Brownstone Tower—Ted Frison, Chief of the Illinois State Natural History Survey Division. Although he had nothing pending with the SM and had many things to do elsewhere, he came to the Tower just to pay a friendly visit, for I had known Dr. Frison ever since the summer of 1922 when we worked at the Japanese Beetle Laboratory, then at Riverton, N. J. What a pleasure it was to see him! He always seemed youthful in appearance and spirits. Only his vanishing hair and his mirthful crow's-feet indicated that he had attained the age of responsibility. He was never thin but always slim and fit. In conversation he would lean forward and talk with enthusiasm about whatever projects were on his mind. And chuckles never failed to emerge as he told of his plans for the State Natural History Survey. It seemed incredible that one so young could hold such an important position. And yet he was primarily responsible for the growth of the Survey in physical facilities, personnel, and accomplishments which made it outstanding and unique.

At the time of Dr. Frison's last visit to the Tower he was beginning to suffer from the pain that incapacitated him during 1945 and resulted in his untimely death on December 9. One of his old friends, Dr. Alvah Peterson, wrote to me as follows: "Ted's death came as a distinct shock. He put up a wonderful fight but finally lost. I think of Ted as a close friend. He was a senior in the Champaign High School when I started my graduate work at Illinois. We played many games of tennis together and I learned to like him very much."

That a graduate student in entomology should have become acquainted with a high school senior is most unusual and calls for explanation. Dr. Frison was born in Champaign, Ill., on January 17, 1895. His family happened to live near the late J. W. Folsom, who was Professor of Entomology at the University of Illinois. Dr. Folsom noticed and encouraged the interest of his young neighbor in insects and introduced him to Stephen Alfred Forbes, who was then Head of the Department of Entomology at the University, State Entomologist, and Chief of the State Laboratory of Natural History. Impressed with his sincere enthusiasm, Forbes and Folsom allowed him to audit University courses in entomology before he graduated from high school. It was of course while taking this advanced work that he met Dr. Peterson.

Upon graduation from high school, Frison entered the University of Illinois, but his work was interrupted in his senior year when, in April 1918, he was commissioned a second lieutenant of infantry. At the end of 1918 he returned to the University, where he received his master's degree in 1920 and his doctorate in 1923. While at the University he played the violin in campus orchestras.

The State Natural History Survey was organized in 1917 with Forbes as Chief. In 1923 Dr. Frison joined it as Systematic Entomologist. Upon Forbes' death in 1930, Frison became acting head of the Survey and on July 1, 1931, he was named Chief. By this time he had made his reputation as a student of bumblebees. Later he specialized on stoneflies, despite his administrative work on the Survey, which took him outside of entomology and into all phases of the biolog-

THEODORE HENRY FRISON (1895–1945)

ical resources of the state and their conservation. To me, who early succumbed to paper work under burdens far lighter than those carried by Dr. Frison, it seems marvelous that he never ceased to make his own contributions to the natural history, identification, and classification of insects. He not only had to obtain appropriations for the work of the Survey and guide the work, but he had to plan the new Natural Resources Building in which the Survey is now housed. And with what enthusiasm he developed his plans and how proud he was of the completed structure! Under his administration his staff increased from sixteen in 1930 to thirty-eight in 1941.

Dr. Frison never forgot his fellow-entomologists and was always performing some service for them. By 1935 the *Journal of Economic Entomology* was in need of modernization. Dr. Frison undertook the editorship with the help of the Survey Editor, James S. Ayars. He changed the format of the *Journal* and working closely with a publication committee, of which I was a member, made the *Journal* a first-class technical periodical. How he found time to do it is still a mystery. He gave up the editorship in 1939. Had he lived, he would undoubtedly have become president of both national entomological societies, which he had already served as vice-president. He was, of course, a member of many societies and committees that had to do with the widespread activities of the Survey. The first meeting of the Midwest Wildlife Conference, held in Urbana in 1935, was largely the result of Dr. Frison's insistence on more and better wildlife research.

Dr. Frison was married to Ruby G. Dukes of St. Joseph, Ill., in 1919. Their son, Theodore Henry, Jr., who was born in 1924, has recently returned from Germany, where he served with the 99th Infantry Division. He was among the first ground troops to enter Germany and spent several months in a hospital as a result of injuries received during a winter campaign. The Frison's daughter, Patricia Ann, born in 1929, is a junior at Urbana High School. Dr. Frison spent many of his vacations driving with his family through the United States and Canada and was always on the alert to collect stoneflies, the nymphs of which are found in swift-running streams.

And now Dr. Frison has gone. He has done much for his fellowmen, but I think nothing he accomplished as a scientist and administrator will be remembered longer by those who knew him than the impact of his cheerful personality.

F. L. CAMPBELL

COMMENTS AND CRITICISMS

The Limitation of Creative Years

I read with much interest the article by Dr. Lehman [SM, August 1945] and the extensive compilations upon which his conclusions are based.

It would seem that this work might have been still more valuable if an additional factor had been considered.

As a man advances and attains recognition in the intellectual fields, he is usually imposed upon by an increasing load of administrative, social, financial, or other noncreative duties. These will sharply reduce the time he is able to devote to his creative work.

In the case of the industrial scientist, it is well known that the man who has made his mark at the age of a few and thirty will usually be ''promoted'' to functions in which he no longer has the opportunity to devote himself principally and exclusively to personal creative work. The same holds true in large sections of academic work.

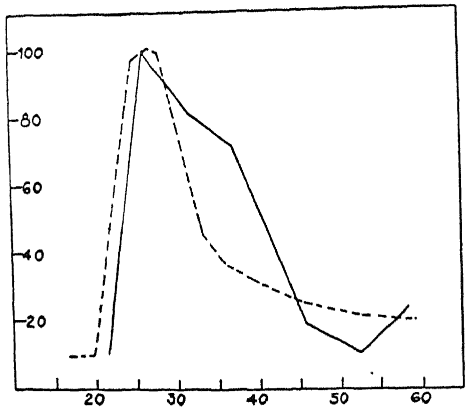
Therefore, the curves showing absolute performance in relation to the age factor are in reality composite curves between performance curves and a curve showing the decreasing amount of time which the scientist is permitted or able to devote to creative functions. In the accompanying graph is shown the percentage of creative working time, obtained by interviews with only a small number of successful chemists within the writer's acquaintance. While obviously the possible limit of error is large, there is no question that a large decrease in the time free for creative work generally occurs at or somewhat before the age indicated as the top creative age in Dr. Lehman's study. The curve for output in chemistry has been copied from Dr. Lehman's Fig. 9.

This would appear to indicate that the decrease in absolute results is due not to a sharp decline in creative capacity, but to a decline in the actual daily working hours freely available for undisturbed pursuit of creative tasks.

Accordingly, it is misleading to conclude, for example, that a man has reached his peak capacity at 28, because at that age he turns out his most valuable result.

If his result at a certain age is rated 100 and is attained with an average working time of 10 hours per day this obviously would indicate a lesser producing capacity than if a result rated 50 were turned out in only 4 hours per day, the remaining 6 hours being devoted to equally strenuous but noncreative duties such as administration, teaching, representation, etc. Yet this is exactly the position encountered by most successful men in the scientific professions, as they attain wider recognition with progress in time.

If the true ability of output or creative ability is to be computed for any age, it is therefore submitted that the absolute value of this output is not sufficient but that the output



WHY GENIUS WITHERS

Solid line, AGE VERSUS OUTPUT IN CHEMISTRY (LEHMAN'S FIG. 9). *Broken line,* AGE VERSUS PERCENT OF TIME AVAILABLE FOR CREATIVE WORK.

divided by the number of hours free for such use is much the truer indication and that neglect of this time factor will very significantly lead astray in any inferences drawn.

In conclusion, the suggestion might be offered that the sharp decline at the age of about 30 is a result of the fundamental characteristic of human organisms of indiscriminately loading all types of work on those who possess ability to carry it without regard for the desirability of reserving peak capacities exclusively for truly creative work. If this tendency could be checked, certainly results of the utmost importance to *Homo sapiens* would ensue.—JOHAN BJORKSTEN.

Dust–devils?

I learn from the September SM that controversy about the Carolina bays is not yet stilled, and that a new hypothesis is promised [see Major Grant's hypothesis, December SM]. In that case may I submit some fresh information that seems to bear upon the problem?

As land-forms the Carolina bays are not unique. Very similar characters are apparent in some of the South African salt pans. I have visited some of these pans, but I am no geomorphologist, so their shape failed to impress me at the time and the observations I now offer are mostly second-hand.

The Grootpan, or great salt pan, at Zwartkops, near Port Elizabeth, is an almost perfect oval, about 1500 yards long and 1000 yards wide. It lies on a sandy coastal plain, and it is surrounded by a sandy rampart 30 to 50 feet high which is covered with scrub. The almost flat floor of the pan carries a shallow lake of brine, surrounded by a wide crust of salt. No adequate topographic map of the coastal plain has been made, but it is likely that aerial photographs are now available. There are many other pans in the neighborhood, but their shape has not been recorded. I think I am right in saying that the Carolina bays were almost unnoticed until aerial photographs drew attention to them. There are thousands of large and small salt pans in South Africa, many on the coastal plain, and many more in the dry interior. I know that many of them are quite irregular in shape, but some of the pans in the Kalahari are "circular or oval." The following, tantalizingly brief descriptions are taken from F. C. Cornell's book, *The Glamour of Prospecting* (New York, Stokes & Co., 1920):

p. 251—The pan "was about a mile in width, almost a perfect circle in shape."

p. 260—"Though the majority of the dunes trend between WNW and ESE, this uniformity is broken in the vicinity of the numerous pans, around which the dunes are often formed in concentric rings."

p. 261—"Many of the circular or oval pans are surrounded and protected by dunes of well over 100 feet in height, as with a wall." The dunes are exceptionally high on the south side.

p. 262—"The big pan Kobo-Kobo . . . is one of the most perfect pans in the desert, as true a circle as though drawn by a compass, surrounded by extremely high dunes, and with a perfectly level, spongy alkaline floor."

p. 266–267—"The pan is about 2 miles wide by 5 miles long. . . . Its perfectly level floor of light blue shale [is] surrounded by hills of reddish sand." Tall dust-devils play always over the floor.

The photographs on pp. 242 and 254 of Cornell's book illustrate these brief descriptions.

It seems to me that the fundamental factors in any explanation of the Carolina bays or the South African salt pans are: (1) a region of wind-blown sand such as a recently formed coastal plain; (2) either salt springs or else shallow depressions where salt water lies close to the surface, and consequently there is (or was formerly) no vegetation to bind the sand together; (3) the occurrence of "dust-devils," or whirlwinds, which waltz around on the hot, bare surface in the dry season, drawing sand and dust up into the air, to fall again outside the cyclonic area.

By this means a low rampart of sand may accumulate around the pan, and because of its higher elevation it will in time have its salt content leached out by rain water. Plants will then take root in the rampart and fix it in place (the Salt Pan condition); and if the rainfall is sufficient the floor of the depression may eventually be washed free from salt, and vegetation will spread all over it (the Carolina Bays condition).

I realize that this does not explain everything, but I think you will agree that there is a remarkable parallelism between the South African salt pans and the Carolina bays.—S. JAMES SHAND.

The Social Significance of Jewish-Christian Intermarriage

Intermarriage is not only a means to bring about the blood mixture of the two racial-confessional groups and thus to further a biological assimilation, i.e., a gradual fusion of the one group into the other; the social importance for both parts is much farther reaching. It consists first of all in the fact that in a great part of mixed marriages the respective relatives of the marriage partners come into close contact. The anti-Semitic attitude of the governmental circles in Hitler's Germany would never have

been criticized at all if through intermarriage many parts of the Christian population had not also been affected. Thus not only the Jews, those by faith and those who had been baptized, but many Christian families received an immediate idea of the barbarism of this new racial and pseudoscientific doctrine. People who had lived with one another in great harmony were torn apart; family ties that had long existed had to be forcefully severed. This makes evident the great importance of intermarriage on the social side, a psychological and sociological importance which cannot be esteemed high enough for the future solution of the problem.

In this way, contact was established between parts of the population which previously had been, if not hostile, at least strange to one another. Often true friendship arose in these circles which withstood even Hitler's reign of terror. On the whole, the mixed marriage has been the best means to bring the two groups together socially, to draw the Jewish minority out of the intellectual ghetto, in which they voluntarily still frequently live; and furthermore to convince the Christian majority that the Jews, whose Holy Writ they have completely taken over into their new edition of the Bible, are as contemporary human beings not very different in beliefs and sentiments from themselves. They have only held on to their older faith and thus bred certain "racial characteristics." The real existing differences can be easily explained biologically through inbreeding and selection resulting from the fanatic persecutions over many centuries and sociologically from the fate of the remaining Jews in a strange, often hostile environment.

Thus it is the most important task and almost a *social mission* of the mixed marriage to bring the two population groups together on an equal basis and fuse them also in a spiritual union. The same function is fulfilled by true friendship among Jews and Christians, for which there are many historical and personal examples; the same function is fulfilled by the common intellectual and social participation in all branches of modern culture, in art and science, in business and trade. No social ties, however, can cause such intensive approach as intermarriage with its bonds of blood through the offspring and the close social relations which derive from it.

To be sure, just in recent years there have been some severe objections to assimilation from the Jewish side. The events in Germany and especially the racial hatred against the Jews have, according to these arguments, furnished the best and most impressive demonstration that assimilation is impossible, because it is undesired. Even the new German idea of isolation and racial superiority will not be able, after the total defeat of the Nazi rule, to resist the forces of biological assimilation and sociological rapprochement. However, one would have to discuss this important point in a special chapter on the fundaments of Jewish nationalism and German anti-Semitism, which certainly is not a new invention. (It was as violent already in the mob assaults in Alexandria at the time of the Greek-Jewish philosopher Philo.) Just now that much may be said: Whoever joins in these arguments represents, too, the German racial idea of *Blut und Boden* (blood and soil), or the pseudoreligion of the race on the Jewish side. There are enough analogies in the Jewish religious legislation of pre-Christian time, which is frequently cited by Aryan race mythologists for this purpose. Yet it is totally overlooked, for lack of historical knowledge or for other reasons, that a large majority of the Jewish people has been assimilated in the course of the centuries and has thus proved its desire and capability of being assimilated.

It has been computed by trustworthy research workers that since the beginning of the Christian Era from two-thirds to three-fourths of the original Jewish stock has disappeared; in all probability for the larger part through merging within the surrounding peoples (Beloch, Ruppin, Fishberg, Wellisch, etc.). Instead of 15 or 16 millions of Jews there would be at least 50 or 60 millions in the present world if they had increased at the same rate as the other nations. Only a relatively small part has been biologically destroyed; i.e., prevented from further biological reproduction.

A simple consideration will make this tendency obvious without going into details of the calculation. The census taking place under the Emperor Augustus counted or estimated 54 million people in the Roman Empire, of them 4½ millions were Jews in the different parts of the Roman world (Julius Beloch). If we follow the well-known Jewish demographer Arthur Ruppin and add further 20 million white Caucasians outside the then boundaries

of the Roman Empire, including ½ million Jews, we arrive at 5 million Jews among 74 million people. That is, a proportion of almost 7 percent. At the present time, counting not the entire world population of more than 2,100 millions but only the population of European descent which may vary between 900 and 1,000 millions, the round 15 millions of confessional Jews in the different parts of this civilization before the onslaught of Hitlerism represent a share of 1.5 to 1.7 percent only. From this simple consideration it follows that the largest part of the original Jewish people has been biologically assimilated.

All such calculations are necessarily based on estimates; but the tendency of a much slower increase of the Jews since the Christian Era is, beyond doubt, correct, although naturally the absolute number of the Jews, too, is larger today than at the time of Christ. This obvious tendency of a biological fusion would prove larger yet if pre-Christian history, especially the absorption of the Ten Tribes of Israel by the Assyrian mixture of peoples, were included in the calculation. That the Jewish racial mixture, consequently, is no foreign one among other Caucasian peoples should have become plain. Extant differences can be rationally explained through inbreeding and selection like similar differences between other national and ethnic groups.

However, some *irrational* or less visible obstacles will still have to be removed in a conflict of religious doctrines and traditions before a complete solution is feasible. Intermarriage or biological fusion is only one step, though indeed a radical one, demanding the courage for genuine assimilation, a courage no less than that of persisting in the dogmatic ritual and marital laws of ancient legislation. These laws were renewed and intensified by the priests Ezra and Nehemiah (around 500 B.C.), after the return of the remainder of the Jewish people from the Babylonian Exile, through austere measures, forbidding and dissolving all mixed marriages. At that time such measures were justified or essential for the preservation of Jewish monotheism. In this respect we may quote the historian Josef Kastein who says in his new history of the Jewish people (*History and Destiny of the Jews,* p. 76):

"Ezra's measure was undoubtedly reactionary. It raised to the dignity of law an enactment which at that time was not included in the Thorah and could be justified only in the circumstances then in existence. The preservation of the race and of their religion alike indicated its necessity, while it also proved that those responsible for it recognized that the Jews might be possessed of particular qualities. . . . The Jews were engaged in an attempt to realize an extremely lofty ideology in their life as a community, an attempt which was on the verge of ending in failure. If failure were possible even in the case of a community which was the outcome of selection, how much greater must not the danger have been where foreign elements, alien both in race and culture, had been admitted. Thus Ezra was fully justified in deliberately and methodically applying the principle of isolation as an educative means."

Almost 2,500 years have passed since. The Jews are no longer the only representatives of the monotheistic idea of God. Monotheism, even though not in the exclusive form of the Jewish national religion, is today shared by around 750 million Christians and 250 million Mohammedans. The ethical ideas of the Jews have to a far-reaching extent been taken over by the daughter-religions. Therefore, at the present time the religious seclusion and the biological inbreeding are no longer justified even as principles preserving the monotheistic idea of God. Rather, social and spiritual fusion has to be added to the biological one. Only this presents an actual *reconciliation* and *reunion* of the Old and the New Faith, in order to liberate the Jewish people from its spiritual ghetto, or from its "chimeric existence among other peoples" as it was formulated a century ago by the Hegelian and religious philosopher Bruno Bauer. The same meaning is in the inspired words by the distinguished Jewish scholar Morris Jastrow, Jr., in his treatise about Zionism and the future of Palestine, published after the end of the first World War: "I would like to envisage a Palestine that may become a beacon-light for the world, that may again become a spiritual focus, furnishing further inspiration for mankind as it proceeds in its march through the ages to a still higher, albeit unknown and unknowable, goal."

Such a religious reconciliation with its biological and social consequences would certainly be a more permanent and more perfect solution than a new national state in Palestine or elsewhere such as the Zionists are longing for. "A Jewish State," Jastrow concluded his ex-

position, "would simply mean a glorified ghetto, narrow in its outlook, undemocratic in its organization, and that may well turn out to be reactionary in its tendencies." A movement for a homestead of the brutally persecuted Jews in the present world upheaval is quite different from a new Jewish theocracy in Palestine. Such a movement is an urgent necessity; yet it must not aim at a new Jewish nationalism and a new religious seclusion. Nationalism has always and everywhere narrowed the mind. A permanent solution can only be founded on the basis of an international Covenant of Nations.

Such a World Organization, however, should be inspired by a superseding religious idea rather than by the political idea of the old League of Nations. This would mean the true religion of brotherhood in the Biblical sense, and its symbolic seat could well be the Holy Land: the ancient home of the Jews, the cradle of Christianity, and the present possession of the Islamic world. That is the realistic meaning of reconciliation of the Faiths and the Nations on this shrunken earth. Towards such reconciliation a greater religious idea must take the lead, in our age of mass destruction, if there is to be a future of mankind.—GEORGE WOLFF.

"Beach Cusps" or Beach Grooves: their Genesis

Dr. O. F. Evans presented an interesting article in the October SM, "Scientific Beachcombing," describing the formation of "beach cusps." A greatly simplified version of his definition of these cusps is that they are temporary points of sand extending into the water and occurring in series.

I cannot see that it is the cusps that are formed. Cusps appear to me to be what remains after a special wave action erodes a series of grooves, leaving the intervening points of the beach intact.

Dr. Evans writes: "It was an old problem, this question of how beach cusps are formed, and had been the subject of discussion, study, and experiment by geologists and other nature students for more than a century. Many articles had been written and many theories advanced, by both laymen and scientists, yet the answer seemed as far away as ever." In a footnote he refers to his article in the Journal of Geology (1938, pp. 615–627). In this he states that W. D. Johnson, in *Shore Processes and Shoreline Development*, (John Wiley & Son, N. Y. 1919, pp. 457–486) has summarized the literature on the subject and found no acceptable solution. Dr. Evans classified cusps in five categories, but the first four cover exceptional cases, not referring to the phenomenon discussed here. It is evident that investigators have satisfied themselves by studying the beaches and not the waves.

Dr. Evans describes what he considers to be the discovery of the process which forms the cusps. He noted a small sand ridge that had been built up during the previous hours; next he noted that a "*new breeze*" was moving in from the lake and the waves were just reaching the shore. The swash and backwash of these new waves broke the sand ridge. He states that succeeding waves made the spacing of the breaks more even, but he gives no reason *why* they should become more even. He goes on to say that the formation is due to the [unexplained] ability of the waves to break the ridge into segments. He comes to the crux of the whole problem when he says: "This [breaking into segments] they can do because of the *slight inequalities in the height of the wave crest.*" The italics are mine. He does not explain the origin of the slight inequality or why these inequalities should be evenly spaced or strike at the same point on the beach. The only piece of the jigsaw puzzle that he lacked was apparently the most important one, namely: *how* the waves came to have high points and *why* succeeding high points hit the shore at the same spots.

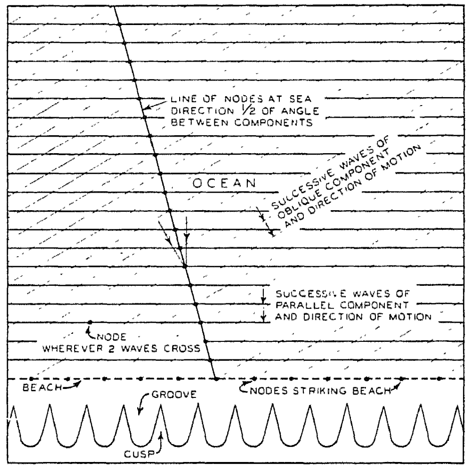
I have spent literally hundreds of hours on the bow of Army transports, passenger steamers, and sailing vessels studying marine life and wave formations. One interesting phenomenon which has always interested me is the coexistence of several systems of waves. One may see a heavy ground swell traveling in one direction, a smaller system of waves traveling merrily in another direction, and occasionally a third system of ripples caused by the local breeze traveling in a third direction. When two systems are traveling at an angle I have noted that there is an increased volume of water where the crests cross and hence greater striking power. We may call this point a "node."

Let us follow these crisscross waves to the beach. This can be done easily by a diagram

(see the accompanying drawing). Let us draw a series of heavy parallel lines on a piece of paper and another series of lines on a piece of tracing paper which we will place over the first piece. By slight rotation we can cause our wave series to cross at any desired angle. Next we place the ruler on the edge of the paper to represent the beach line. For simplicity it should be parallel to the heavier system of waves. At each point where the oblique lines cross the beach line a node has hit the beach. Thus it can be seen that the width or spacing of the beach grooves is not only influenced by the size of the waves but also by the amplitude of divergence of the oblique component—the greater the obliquity, the greater the width of the groove spacing. If the main component reaches the beach parallel thereto we have the nodes hitting fairly close together—or at a distance equal to the internodal distance measured along the crest of the component moving parallel or nearly parallel to the shore.

It must be noted that the three factors—size, direction, and speed of the waves of the two components—may cause a great number of combinations of which only a few are suitable to the formation of beach grooves. A beach remains about static from year to year, so the forces of erosion and deposition balance in the long run. A node is only a momentary fact—the instant that the oblique crest rides past the parallel component the node no longer exists, but another comes into being when the oblique motion again crosses the parallel motion. The nodes form in lines which bisect the angle made by the components. If these lines are not straight, grooves cannot form. All parts of each component strike the beach, but it is only when and where a node strikes that the additional force is exerted that starts the erosion of a groove. For the sake of simplicity we must assume that our waves are traveling at just the right speed to intersect at the beach as shown in our diagram. It is obvious that one set of waves may have farther to travel than the other and hence must move faster. In nature it is seldom that the right timing exists for the formation of grooves. The oblique component may be invisible from the beach and it need not be large to be effective.

Dr. Evans states that he has measured cusp series some of which had a spacing of over 20 feet. The photograph accompanying his article



shows the spacing to be about 60 feet by comparison with automobiles shown on the highway skirting the beach. These grooves show that many cubic yards of sand were shifted below sea level to effect their completion. Sixty feet is just about what one would expect in the case of a mild Pacific Coast surf—the photo was taken near Santa Barbara.

Dr. Evans gives figures to show that there was considerable variation in spacing of the cusps in the cases which he studied—up to 147 percent. If it is a fact that there is variation on a straight beach my theory is inadequate and probably any other theory covering repeated heavier waves striking repeatedly in the same spots would also be inadequate. Neither Dr. Evan's photograph nor his diagrams show any such variations as he mentions in his text.—CHAPMAN GRANT.

Belated Acknowledgment

It has been brought to my attention that I did not point out the contribution of Anglo-Canadian institutions to French Canada. [''Science in French Canada,'' September and October 1944 issues of THE SCIENTIFIC MONTHLY.] Maybe I should have insisted on the training many French Canadians received in the past, and still do, in engineering, medicine, and agriculture at McGill University and Macdonald College. The latter, under a special research grant from the Government of the Province of Quebec, has been especially active in plant breeding and seed-growing, and the population of this Province as a whole has benefited greatly thereby.—PIERRE DANSEREAU.

THE BROWNSTONE TOWER

In the issue for September 1944, page 243, we published a letter from Peter Hidnert, which I entitled "The Ragged Edge." Dr. Hidnert wanted to know why the cover pages of the SM were larger than the inside pages. It would be better, he thought, to trim all around so that handling would not quickly reduce the magazine to a dog-eared condition. We agreed with him, for one cannot readily name a periodical, except *Science*, which has remained unshaven up to 1945. But, because wider margins were needed for trimming and binding, we could not trim all around while paper restrictions were in force. Now we have reduced the area of type on each page to that used in 1943 and at last present a trimmed magazine of 100 pages.

The biography of the inventor John Ericsson was offered to us by *The Reader's Digest*. We accepted it not only because of its intrinsic interest but because it demonstrates the biographical technique of a well-known novelist. This biography may be published later by *The Reader's Digest* in condensed form.

Harwood's article on phenothiazine is the second personalized account of research to be published in the SM since we appealed to our contributors to provide human stories of detection. Harwood conducted phenothiazine to its triumph as an anthelmintic, but he did not know its earlier story in the Bureau of Entomology and Plant Quarantine. That story is told in detail in *Record for Party Smith*, 1937, being the verbatim report of a hearing in which the Government presented evidence to try to convince the patent examiners that the Bureau's chemist, L. E. Smith, should be granted a patent on the insecticidal use of phenothiazine instead of Du Pont's chemist, E. W. Bousquet, whose claims were in interference. After appeal, the case was finally decided in favor of the Government, but by that time phenothiazine did not look so promising as an insecticide as it had when Dr. J. W. Bulger first detected its high toxicity to mosquito larvae. But in the end, as Dr. Harwood relates, the insecticidal investigations of phenothiazine paid off by way of experiments on the control of horn flies. Thus veterinarians became aware of phenothiazine and thus its unique usefulness as a vermicide was soon disclosed. Perhaps the earlier history of phenothiazine should be written for the SM, and perhaps I should do it, for I was there.

The symposium on Early Man in Oregon was presented at a meeting of the Oregon Academy of Science. These three papers were originally written in conventional scientific form with full documentation. At our request all three authors tried to recast their papers for the benefit of our lay readers.

Our concluding essay by Lt. (jg) Platt came unsolicited. We think the Lieutenant has some sensible and pertinent suggestions to make about our national defense. Although he does not speak for the Navy, he will be identified with the Navy. It seemed courteous therefore to ask the Army to name an unofficial proponent in this debate. Consequently we requested Colonel Harold W. Kent of the General Staff Corps to find a young officer able and willing from personal conviction to write an essay for the SM in favor of universal military training as proposed by President Truman. Thus we obtained the article by Captain Curzon. As the captain won his commission the hard way—induction, basic training, officer candidate school—he knows what he is advocating when he recommends universal military training.

F. L. Campbell

THE SCIENTIFIC MONTHLY

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THEORIES OF INTELLIGENCE*

By L. L. THURSTONE
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Every one of us judges daily the intelligence of other people, and occasionally we even estimate our own. It is a touchy matter, for we would rather have our integrity put to question than our intelligence, and yet the fact is no doubt generally acknowledged that intellectual endowment is unevenly distributed in the population, that some men are brighter than others.

The nature of human intelligence has been a problem for centuries. Until about sixty or seventy years ago this problem was discussed entirely at the verbal and speculative level. A pre-experimental history of this subject would reveal a great deal of speculation in which we would find much ingenuity and insight. It would be an unstable guide for scientific work, however, because here as in other prescientific speculation there was more error than fruitful insight.

The first sustained attempt in modern times to appraise the intelligence of individuals as distinguished from examinations for scholarship or proficiency can be credited to Sir Francis Galton (1822–1911). He attempted by experimental methods to ascertain the differences among persons as to their mental endowment as distinguished from the estimation of proficiency. In 1885 he started a laboratory in London where early attempts were actually made to appraise individual differences. At that time Galton limited himself mostly to sensory and perceptual functions, which were not successful as indices of intelligence. Many investigations have been made to ascertain the relation between various sensory functions and human intelligence, and the findings have been definitely negative. In a later paragraph I shall return to the sensory functions in their dynamic aspects.

The problem was next tackled in some extensive studies of school children. These studies were motivated by the hypothesis that differential rates of progress of children in the schools were due largely to differences in mental endowment and that children should be classified for school work according to some index of intelligence. This problem called for an estimate of the mental alertness of each child which should be based on evidence as far as possible independent of formal school progress. It was recognized, of course, that a child's school progress was determined only partly by native endowment and partly by motivational conditions in his home and school environment. The independent appraisal of a child's mental endowment would be successful to the extent that the effects of nature and nurture could be analytically separated.

* This paper was read before the Chicago Literary Club on February 12, 1945.

The best known among the early methods of appraisal was the Binet test. The plan of this test was simple. Each child was asked a number of questions over a fairly wide range of content. The questions were intended to reveal the child's ability to solve simple problems with content that was drawn from his environment. Much of the Binet test depended on the assumption that, other things being equal, a bright child would gather more generally available facts and ideas about his environment than his less-gifted classmates. Each problem or test item was given to children of different ages, and it was experimentally determined at what age one could expect a half or two-thirds of the children to do the task correctly. The Binet tests have been translated into many languages. For each translation it has been necessary to modify the content in order to adapt the test to cultural differences.

The total performance of a child on a Binet test is stated in terms of his so-called mental age. A mental age of 10 years, for example, represents merely the average performance of 10-year-old children and similarly for the rest of the scale. If a child attains a mental age of 10 and if he is actually 8 years old then he is two years accelerated. Children are frequently described in this way as to their degree of mental acceleration or retardation, and this classification is of practical value in the schools.

It has been found that there is a reasonably good correspondence between the degree of acceleration and retardation of a child and his school progress, but it must not be assumed that this correspondence is perfect. The correspondence is, however, sufficiently close to make the test appraisal of considerable value in dealing not only with groups of children but also with individual problem cases. For example, when a child fails to participate in a class he is ordinarily judged to be lacking either in ability or in motivation. If it is found by the Binet test that he is accelerated in intelligence his poor performance may actually be attributed to boredom. By advancing such a child he is put in a situation that is sufficiently challenging for him, and he may again hold his own. Unfortunately, this is the exceptional type of case.

When we examine the nature of the Binet test as to the content of each question we find that it is a hodgepodge. We find questions about vocabulary, others about simple arithmetic problems, puzzles of various kinds, simple reasoning problems, and the interpretation of proverbs. One of the ways in which a child's performance is analyzed is to study what is called scatter. Some children hold their own in all types of items, while others go much higher in one type of item than in other types. It is to be expected that some children will be accelerated in vocabulary, for example, and retarded in numerical thinking, or vice versa. This phenomenon of scatter in the performance on general intelligence tests is indicative of the fact that intelligence is a complex rather than a single trait. But in spite of the crudeness of the Binet test and other similar tests of general intelligence they have been of great practical importance in the schools.

We turn now to a later stage in the history of this problem, namely, the analysis of actual test performances. If two tests of intelligence are given to a group of people you might expect that those who excel in one of the tests would excel in the other test. This would be the case if the two tests were measures of the same trait and if the results were otherwise unaffected by experimental error. The degree of association between two variables is represented in statistical work by the coefficient of correlation. This is a numerical index which takes possible values between $+1$ and -1. If two measures have a correlation of $+1$

there is perfect association between them and accordingly all the individuals in the experiment would be arranged in exactly the same order by both tests. If the correlation is -1 then there is perfect inverse relation, and the top man on one test is the lowest man on the other test. If the correlation is 0 then there is no discernible correspondence between the two measures. They are then just as independent as if both sets of numbers had been obtained by tossing coins. Correlational theory was started by Galton in a more or less descriptive manner, and it has been very highly developed by the mathematical statisticians. As an example of the meaning of correlation we might consider the well-known association between height and weight. Men who are tall also tend to be heavier than those who are short, but the association is of course not perfect. If we were to compute a correlation for height and weight of a random sample of several hundred adult men, we should find that the correlation is about $+0.5$. Correlation theory is involved in the application of statistical methods to a variety of problems, and some of these methods are also applicable to the present problem.

At the beginning of the century quite a number of studies were made to determine this quantitative index of association between abilities to do different kinds of tasks, and it was soon found that the correlations between various tests that were supposed to be indices of general intelligence were far from perfect. Two inferences were drawn from these simple observations. First, it was concluded that no test is in any sense a pure measure of the postulated general intelligence and, second, that an appreciable part of a test performance is subject to fortuitous experimental error. At this time there also began to be considerable speculation as to whether general intelligence could be postulated as a single general function. The alternative was to consider intelligence as a complex of many distinct abilities. A third possibility which has some defenders is that intelligence is determined by thousands of factors that function without any pattern or groupings.

It was in 1904 that the British psychologist Spearman wrote a simple but epoch-making paper on the relations for groups of psychological tests that had been given to the same individuals. He found that under certain circumstances, which he specified, these correlations did indicate the existence of what he called a single intellective factor which he denoted by the letter *G*. This was the starting point for a series of lively controversies which are still current in more modern form in the British and American psychological journals, forty years after Spearman's provocative paper.

Until about 1930 the central theme in these controversies was nearly always the question whether Spearman's general intellective factor did exist. It was soon recognized that even with the best available controls the postulated general intellective ability of Spearman was inadequate to account for observed relations among experimental tests. It was found necessary to acknowledge the existence of other abilities in addition to a general intellective factor, but these were frequently referred to as disturbers of the fundamental relations of Spearman. During this time there was general acceptance of special abilities in addition to the factor *G*. The main scientific interest, however, was directed at the question whether general intelligence should be postulated in addition to the special abilities which disturbed the simple relations of Spearman's hypothesis.

In 1930 investigations were begun with a different emphasis. Instead of asking whether the experimentally observed relations among the abilities, represented by a series of tasks, could be accounted for by a single intellective factor, the

question was asked how many factors or abilities were implied by the observed relations. It was then left as a question of fact whether one or more of the abilities that were identified might turn out to be more general or central in character than the other abilities. For any given set of tasks that were given experimentally to a group of several hundred people the question was then to determine how many abilities were represented by these tasks and, further, to identify the nature of these abilities. Before the analysis of such experimental data could be undertaken it was necessary to extend the earlier methods of Spearman for a single factor to the n dimensional case for any number of factors. This work has progressed for a number of years with the assistance of some British and American mathematicians and physicists who have taken an interest in the formal aspects of the problem. During the past ten years the methods so far developed have been applied to experimental data with findings that are of both theoretical and general interest.

The first major experiment with the new n dimensional methods of factor analysis was started in 1934. A battery of 56 psychological tests was designed especially for this study. These tests were devised so as to represent a wide variety of tasks which had been represented in previous studies of intelligence. Included in this battery were tests which called for verbal comprehension, verbal reasoning, various types of fluency, speed in simple numerical work, quantitative reasoning, various forms of induction, verbal, visual, and auditory associations, visualizing flat figures and solid objects, various forms of abstraction with verbal, numerical, and visual material, reasoning about mechanical movements, and memory for different types of content. This battery of 56 tests was given to several hundred student volunteers, requiring about 15 hours of work for each subject. When the records had been assembled the correlation was determined for each pair of tests in the whole battery. This required the calculation of about 1,600 coefficients of correlation. For each pair of tests the correlation tells us the extent to which those who succeed in one task tend to succeed in the other.

It is an old observation about intellectual tasks of all kinds that the correlations are all positive. If two widely divergent mental tasks are considered, the correlation between them may be low but the association is always positive. In fact no negative correlations have ever been found for intellectual tasks. There is a rather common misconception about these relations. It is not infrequently asserted that those who are superior in one intellectual task are somehow inferior in some other intellectual task. Among students such an impression is not uncommon as regards linguistic and scientific abilities. The fact is that no negative correlations for performance in school subjects or in intellectual tests have ever been found.

A large table of correlations among these 56 tests constituted the starting point for a multiple-factor analysis with the new n dimensional methods for the purpose of discovering how many abilities must be postulated in order to account for the observed correlations. It was found that 12 factors were sufficient to account for these relations among the 56 tests. Those who are mathematically inclined may be interested to know that the rank of the matrix of correlations is the number of linearly independent factors that must be postulated.

It is a curious circumstance that in the multiple-factor methods one can determine the number of factors involved in a set of experimental tasks before the nature of these factors is known. The next problem is to determine just what these factors or abilities are like. Here we come to an interesting indeterminacy the

nature of which can be represented in simple form in a two-dimensional diagram. If we plot a number of points on a diagram we ordinarily assume that the x and y axes are given. In the factor problem the configuration of points is given, and it remains for us to insert the x and y axes. These axes represent the abilities or factors, and the problem is to locate them in a configuration so as to give scientifically fruitful interpretation of the test relations. If we are dealing with a number of points on an ordinary diagram and if these points arrange themselves in two streaks from the origin, then the location of the two axes can be easily chosen by simple inspection in such a way as to give the most parsimonious interpretation. In dealing with an n dimensional configuration it is necessary to do these simple things analytically because unfortunately we cannot make physical models in more than three dimensions. If it were possible to make 12 dimensional models so that we could look at them, then the factor problem would be quite simple. Since we are living in three-dimensional space and since the factor problem involves relations among many more factors or abilities, it is necessary to handle the problem analytically, but the principle is fundamentally the same as in the two-dimensional diagram.

In choosing a set of 12 reference abilities for the experiment with the 56 tests we adopted what we have called the simple structure principle, the nature of which can be explained without reference to its mathematical form. Let us suppose that there are distinguishable mental functions in overt performance which are caused by physically differentiable functions in the organism. It is not necessary to postulate that each mental ability is represented by a separate organ or that it has a locus in the nervous system. Some mental abilities may be so determined while others may conceivably be accounted for physically in terms of parameters of the system as a whole. This is a question about which the factorial methods make no assumption whatever. All that we are doing is examining the overt performances of several hundred people in a wide variety of tasks to determine how many differentiable functions must exist in order to produce their observed differences. Let us suppose that there are differentiable functions such as number facility, one or more types of memory, one or more kinds of verbal ability, facility in visualizing, facility in auditory imagery, and so on. Let us assume that these abilities operate to produce the performance in each test. Now it would certainly be very unlikely that all these abilities would participate equally in every one of the tests. If we knew what these abilities were and if we were asked to assemble 50 psychological tests of different kinds so that every test would call for every one of the mental abilities we should probably find it impossible to design such tests. In assembling the tests that are feasible and that represent intellectual functions of various kinds we are probably necessarily featuring some of these abilities in each task, while other abilities are almost absent. If there is an ability that is characterized by facility to deal with numbers, for example, then such an ability would not be called for in doing verbal tasks or visualizing tasks which have no numerical or quantitative aspect. Facility in auditory imagery, for example, would surely be absent in the visualizing tasks. The selection of a set of fundamental reference abilities is then the problem of choosing these abilities so as to simplify as far as possible our comprehension of each of the tests. We account then for each test by the smallest possible number of abilities. This is the simple structure principle which can also be stated in geometrical form. The geometric analogue for this principle is to

locate the x and y axes and all the other axes according to the most simplifying planes in the configuration of test vectors.

Applying this principle that we have called simple structure we find that some of the abilities stand out rather clearly and can be easily interpreted. In exploratory studies we always find some factors whose interpretation is obscure. It is encouraging, however, that when experiments are carried out on groups of people who differ in age and education and with new sets of tests we have succeeded in identifying the same basic mental abilities. It is this kind of invariance which makes the search encouraging. It has been found also that factors which are obscure in an experimental study become clarified in subsequent studies with tests that are especially designed to investigate each domain.

When the analytical results have been obtained the final interpretation consists in discovering the nature of the ability that has been shown to be present in one group of tests and absent in the other tests. Sometimes we have rival hypotheses about the nature of an ability which has been shown to exist. Fortunately such questions can be resolved as questions of fact, and so we are not dependent on debating-society methods for settling such questions. We proceed then to design new tests which are crucially differentiating for rival hypotheses about the nature of a factor. The new tests go into the next factorial experiment, and we can then usually discover which of the two hypotheses is correct. We must acknowledge, however, that sometimes the subsequent factorial study shows that both hypotheses were wrong. Such a situation simply means that we must guess again.

We turn next to a brief description of each of the mental abilities that have been identified so far.

One of the abilities that have appeared very clearly in a number of independent investigations is the ability to visualize solid objects. This ability is known as the space factor and it is denoted by the letter S. It represents the ability to visualize flat figures or solid objects. As far as we can determine at present this factor covers figures in flat space and solid objects in three dimensions. At first we were prepared to find separate factors for these two types of thinking. This space factor is not new. It is undoubtedly the same visualizing ability that was described by Galton in his informal studies of imagery types. He differentiated between visual and auditory types. In any audience of educated men there will be a considerable proportion who rather dislike tasks that require visualizing and others who depend on it as their principal medium of thinking. We are dealing here with differences among persons in the readiness with which they manipulate different types of imagery. A man's style of writing and his preferred methods of exposition are likely to reveal his preferred imagery.

A number of tests are available which give fairly good indices of the space factor S, and their application in employment selection and in student counseling seems rather self-evident. For example, if a boy is a poor visualizer as determined by these tests it is a safe bet that he will not be happy as an apprentice in a drafting room, and it is also equally safe to predict that he will not enjoy descriptive geometry and similar subjects. It is known that the space-factor is heavily involved in mechanical aptitude, but it is only one of the components in mechanical aptitude.

Another factor that is quite easily identified has been called the number factor N. This is represented by facility in doing simple numerical tasks, but it must not be inferred that this factor is heavily involved in arithmetical reason-

ing or in mathematics. One should not be surprised to find some very competent mathematicians who are not high in the number factor *N*. On the other hand, one should expect to find a good cashier or bookkeeper to have facility in this factor. Arithmetical reasoning, which is represented by the familiar statement problems in arithmetic, involves a number of other factors often more important than number facility *N*. In designating this factor as concerned with numerical thinking we have been puzzled about a closely related problem. If we are here dealing with differences in native endowment, then it has seemed to us that each factor should be described in terms of the kinds of thinking and imagery rather than in such terms as *number* which is a cultural concept. Attempts have been made to interpret this factor in some more fundamental form. Eventually, when the nature of this factor is more completely understood, it may be possible to appraise it by tests which are nonnumerical in character. It might then be merely a historical accident that the factor was identified in tests that happened to be numerical in character. On the other hand, there is a possibility that there exists a distinguishable mental ability which is definitely related to numerical thinking as such. These are questions for the future to answer.

One of the socially most important factors that have been identified is called verbal comprehension *V*. It represents facility in dealing with verbal concepts. It is represented rather well by tests of vocabulary or tests of verbal reasoning, as in the interpretation of proverbs or the comprehension of difficult prose. Our educational system is built around this medium, and consequently those individuals are generally judged to be intellectually retarded who are deficient in verbal facility even though their other intellectual powers may be exceptional.

Investigations that have been completed thus far have revealed the existence of three or four verbal factors, but only two of them are at present understood. In addition to the important verbal comprehension factor *V* there has been identified another verbal factor which has been called word fluency *W*. At first it was difficult to see just why these factors, which are both verbal, should nevertheless be so clearly separated in the factorial analysis. Closer examination of the tests by which these two verbal factors were identified revealed an interesting difference. The tests which are heavily saturated with the word-fluency factor *W* have this characteristic in common that the individual subject must produce his own words in a restricted context, whereas the verbal-comprehension factor *V* requires that he understand the verbal material that is given to him. A simple test for the word-fluency factor is to ask the subject to write, for example, as many boys' names as he can think of in two or three minutes. The nature of the task can be varied considerably with the same results. The subjects might be asked to write a list of things to eat and drink. The subject who is gifted in word fluency will keep on writing, whereas a person who does not have this kind of fluency will write perhaps a dozen words and then have a block. With some encouragement he may proceed with another spurt, and after a few seconds he will have another block. These two verbal abilities are distinct in the sense that a man may be verbally fluent even though he has a limited vocabulary, and another may have an extensive vocabulary and be capable of profound verbal comprehension even though he is not verbally fluent. The social and educational implications of these differences are self-evident.

When reading a treatise by Weisenberg and McBride on aphasia I became interested in the verbal tasks that some

of the patients could do and the other verbal tasks they could not do. The differentiation between these two types of verbal tasks for one group of aphasic patients seemed to be well described in terms of the two verbal factors *V* and *W*. This particular group of patients failed on all the tests which involved the word-fluency factor *W*, but they seemed to be able to do the verbal tasks in which the verbal-fluency factor *W* was absent. This factorial differentiation among verbal factors is worthy of further study to ascertain whether the factors that have been identified on normal adults can be identified perhaps even more clearly in some types of speech pathology. There is indication at the present time that the word-fluency factor *W* may be a complex and that it can be broken up into further factors. It will be reassuring if different approaches to the same problem should agree as to what constitutes the differentiable verbal abilities.

It is a commonplace observation that some persons have good memories and that others have poor ones, and it is also a commonplace among the current psychological textbooks that memory is not a separate mental faculty. The result of factorial studies so far seems to sustain the popular impression rather than the current psychological dogma. There seems to be good indication factorially that people do differ in memory quite apart from their other mental abilities. A man who is of superior mental endowment may or may not be fortunate enough to have a good memory. The memory factor that has been identified seems to be represented best in the ability to remember paired associations. There are indications of other memory factors that may be concerned with memory for temporal sequence as distinct from paired associations, and there probably exists also a separate ability for what is called incidental memory, namely, the ability to recall past experience without previous intention to recall it. If this differentiation should be sustained, then we should distinguish between the ability to memorize intentionally and the ability to recall past experiences with no intent to recall them. The memorizing factor seems to transcend the nature of the content and so becomes applicable to numerical, verbal, or visual material. A person's performance in memorizing material of different kinds will, of course, be affected also by his abilities in the imagery types that are specifically involved.

One of the most interesting of the primary mental abilities is the ability to discover the rule or principle in the material that one is working with. This primary ability has been called induction. It definitely transcends the nature of the material, and consequently it must be centrally determined in some way. It does not seem to be directly associated with any of the sensory modalities. It is only natural that one should ask the question whether originality and creative ability are represented in this factor, but on that question we have so far no conclusive evidence. Our guess is that facility for induction in the sense of discovering the rule or common principle in the material with which one is working is not the same as originality and creativeness. It would certainly be of great value to discover any dependable indicator of creative talent, but on that problem the factorial studies so far completed can only give us a few clues.

Since induction has been identified as a primary mental ability it is only natural to look for deduction, and it has also been found to be a primary factor, but it has not been identified with sufficient clarity to be represented in the psychological tests that are recommended for use in the schools.

There has appeared in several studies a factor which we have not attempted to name but which is related to these rea-

soning factors. The best that we have been able to do so far is to associate it somehow with the ability to carry out restrictive thinking. By this we mean the ability to carry out a reasoning problem in which there is a definite answer as distinguished from those tasks in which the effective solutions are not unique.

In the past few years a different type of factorial study has been carried out on what we have called perceptual dynamics. In early work in this field it was found that sensory acuity was no index whatever to the mental abilities of the subject. In returning to the field of perception we have been primarily interested in the dynamics of perception. By this we mean apparent movement phenomena as studied by the *Gestalt* psychologists and other visual and auditory effects most of which have a temporal aspect. Five of the factors revealed in the perceptual functions are concerned with speed. These are: reaction time, speed of perception, speed of judgment, speed of closure, and rate of reversals in ambiguous perception. In addition to a factor concerned with speed of closure there has been identified another which has been called flexibility of closure. These two closure factors are of special interest because they are probably associated not only with intellectual types but also with temperamental characteristics.

The phenomena of closure can be illustrated by a simple example. If you see a word in which a part of each letter has been removed you may look at the word as if it consisted of a group of unrelated spots. You may look at it that way for several seconds in the attempt to put meaning into it. You may suddenly see what the word is. That would be an example of closure. The apparently chaotic and unrelated spots suddenly become unified into a single percept. You then see one thing instead of many disparate things. The readiness with which people get closure seems to be a trait much more fundamental than appears at first sight. It is not merely the ability to solve a trivial puzzle. It seems to extend to much more important tasks. A group of administrators who were examined in Washington in the attempt to study their mental characteristics did reveal among other things that the successful administrators scored well on certain tests of closure. A descriptive interpretation might be that they are men who readily unify the apparently disparate elements in the work for which they are responsible.

Another closure factor has been called flexibility of closure, and this factor may be socially just as important as speed of closure. Wertheimer has described problem-solving as the ability to destroy one configuration in favor of a better one. This is a good description. It implies the ability to destroy or ignore one configuration of things in order to see the same things in a different configuration. Since these two closure factors, which have been called speed and flexibility, have only recently been identified, we cannot speak with confidence about their implications. They are known to be distinct from the visualizing or space factor *S*.

The question has been raised about the primary factors that have been identified, whether any of them represent a modern form of the central intellective factor that Spearman postulated forty years ago. If we were to make a guess in answer to this question we should probably consider the inductive factor and the two closure factors. But the solution to the problem of the central intellective factor is probably more indirect. It has been found that when a group of individuals are appraised as to each one of the primary mental abilities that we know something about now, the primary abilities themselves turn out to be correlated just as the original tests were correlated.

In other words, those who have some of these abilities tend somewhat to excel in the others, but the associations are not close. In dealing with individual cases we cannot infer, for example, that a man has good number facility just because he is a good visualizer. But one can say that those who excel in any one of these primary abilities tend to be better than average in the other abilities. This raises the question about why these primary factors are correlated. Our present interpretation is that there exists what we have called a second-order factor which is more fundamental than the primary. If this central factor exists, then it facilitates the function of all the primary or special abilities. Our present interpretation is that the central intellective factor which Spearman postulated exists in the form of some central parameter which has a positive influence on all the special mental abilities we have been discussing. It has been found that some of these primary factors are more heavily saturated with this central factor than others. Rote memorizing ability has very low saturation on the central intellective factor, whereas induction and verbal comprehension and visualizing have rather high saturations on the central intellective factor.

Our conclusion regarding this old question is then briefly as follows: There seems to exist a large number of special abilities that can be identified as primary abilities by the factorial methods, and underlying these special abilities there seems to exist some central energizing factor which promotes the activity of all these special abilities.

In addition to the studies on adult subjects the same type of analysis has been made on experimental data for high-school children and lately on a group of several hundred 5- and 6-year-old children. Special tests had to be designed for the kindergarten children who were not yet able to read. Essentially the same primary mental abilities have been identified with these young children as with the adults. It has been found also that some of these factors such as visualizing and closure seem to mature very early. It is possible to determine at the age of four or five whether a child is a good visualizer, and we are certain that young children are much more clever in reasoning than is ordinarily believed. The work with young children is motivated partly by the thought that methods of instruction may eventually be adapted to the mental profile of each child. It is well known that the public schools are full of reading-problem cases. These are children who seem to have some mental ability but who are slow in reading. It is conceivable that different methods of teaching should be used with children of different mental profiles.

Finally we turn to the question of how the appraisal of special tests for each primary mental ability can be made practically useful. Instead of attempting to describe each individual's mental endowment by a single index such as a mental age or an intelligence quotient, it it preferable to describe him in terms of a profile of all the primary factors which are known to be significant. A glance at such a profile shows whether a man is generally gifted, whether he is exceptionally gifted in one or two of the primary factors, or whether he has some conspicuous limitations. The tests which have been made available for use in the schools represent six of the primary factors whose implications are already fairly well indicated. As new factors become isolated and interpreted as to their implications, they will be added to the tests which are made generally available in schools and eventually for employment selection. If anyone insists on having a single index such as an I.Q., it can be obtained by taking an average of all the known abilities. But such an index tends so to blur the description of each

man that his mental assets and limitations are buried in the single index. It seems better to use a profile and to extend it with new factors as they become isolated.

The question is often asked whether these primary factors are inherited and whether they can be trained. A study has been made of 150 pairs of identical and fraternal twins in Chicago, and it has been found that the identical twins are much more alike in each of the primary mental abilities than are fraternal twins. This finding is what one should expect, and it agrees with previous studies on twins with less differentiated measures of mental endowment. The interpretation of these genetic studies is that inheritance plays an important part in determining mental performance. It is my own conviction that the arguments of the environmentalists are too much based on sentimentalism. They are often even fanatic on this subject. If the facts support the genetic interpretation, then the accusation of being undemocratic must not be hurled at the biologists. If anyone is undemocratic on this issue it must be Mother Nature.

To the question whether the mental abilities can be trained, the affirmative answer seems to be the only one that makes sense. On the other hand, if two boys who differ markedly in visualizing ability, for example, are given the same amount of training with this type of thinking, I am afraid that they will differ even more at the end of the training than they did at the start. Both of them would no doubt improve in overt performance, but the boy with native ability would probably outdistance his less gifted partner.

In comparing the profiles of students with their known professional interests, one finds that students who are highest on the two verbal factors are usually interested in some linguistic occupation, such as writing or journalism. Combined with the reasoning factors the profile indicates an interest in the more formal aspects of language. A combination of the reasoning factors and the visualizing factor is characteristic of students of physical science and engineering. So far we have not seen any characteristic profile for medical students. This probably means that the medical profession is so broad in scope that it has opportunity for wide range in types of talent. We have seen some very striking examples of children in the Chicago schools whose special abilities were unknown until they were examined by tests of the primary factors. One child was recently brought to our attention who scored extremely high in the verbal factor *V* and low in everything else. He is a tremendous reader and yet he fails in all his school subjects. He does not seem to be able to reason with what he reads. Another child was extremely high in word fluency and low in all other factors. This child has a reputation for his great facility in fabricating stories to get out of one delinquency after another. Another child was considered to be a dunce until it was found that he was the best visualizer of his age but he was low in the verbal factors. This was a clue not only for the education of the child but also for directing him into something where he might have prestige. Such a resolution of his problem solved also a personality difficulty because he can now identify himself with something in which he excels.

Studies must be undertaken as soon as possible to investigate factorially such fields as mechanical aptitudes, musical talent, and artistic talent. It is often supposed, for example, that mechanical ability is nothing but manual dexterity, but such is not the case. Mechanical ability is mostly in the head. It is a complex of abilities whose isolation should be of considerable importance for science and industry. That is still to be done.

The multiple-factor methods that have been developed in the past decade have just begun to produce results. We have barely started in a field of investigation that will enable the next generation to be more rational than we have been in planning the education of their children and in selecting people for each kind of work. Even at the present time psychologists can do much more effective work in appraising mental abilities than was possible ten years ago.

This work is consistent not only with the scientific object of identifying the distinguishable mental functions but it seems to be consistent also with the desire to differentiate our treatment of people by recognizing every person in terms of the mental and physical assets which make him unique as an individual.

THE PATH I CHOOSE

What is this freedom which men boast they own?
My barque seems free upon the waters,
Yet no matter what the course I set
I only partly guide.
You, like a lodestar, turn my compass north
Across the trackless miles,
A feeble force but constant
And a source directive in my better hours.

For life is unknown ocean,
And winds may blow destructive rancor to my soul
Or anchor it becalmed on surface of a liquid glass
To shrivel 'neath the blazing sun.
At other times I race before a cooling draft
And speak of progress toward a port.
The tide may raise my song to buoyant tone
Or wring a minor wail at its recess,
And, under all, the oceans move in vaster whirls.

Men speak of final ports. This craft was ocean born
And knows no rest.
If port there be, it yet has sighted none,
But this is life, not yonder, and the best we have
Lies in this frame and not in distant scene.
Let me speak fair to all of those who pass,
Hail and farewell or traveling abreast.
May I no wreckage cause nor halt the travelers fast
Who know where they are bound.

Yet wandering to touch at many strands,
The known and loved, the new uncharted isles,
I seek the gems of each and hope to land
Upon your restful shore before the end.

JOHN G. SINCLAIR, 1937

SCIENCE IN THE POSTWAR LIBERAL ARTS PROGRAM*

By LLOYD W. TAYLOR
PROFESSOR OF PHYSICS, OBERLIN COLLEGE

COLLEGE teachers of science quite properly are concerned with whatever threat to the liberal arts exists in the recent diversion of our educational resources to wartime training. Perhaps the most disquieting aspect of such a threat was the war's acceleration of a trend that was becoming pronounced long before 1941, the trend toward ever more narrow specialization. Even associated departments were having increasing difficulty in understanding each other's language, and communication between curricular divisions had completely broken down. This trend, growing in large measure out of the nature of graduate training for the doctorate, is probably unavoidable in a university setting, but it has been planting the seeds of destruction of the liberal arts ideal in our colleges.

One of the fruits of this Balkanization of the curriculum is a resuscitation of the old antagonism of the humanities toward the natural sciences. Vestiges of this were in evidence before the war. Penalizing science teachers by requiring two or three clock hours of laboratory duty for one teaching credit hour was one manifestation of it. Penalizing students for taking laboratory subjects by trying to retain the old scheme of laboratory fees was another. But these were mere vestiges of an old Pharisaism. Now, as one of the consequences of the war, this attitude has been fanned into new life, and I fear that we shall have some of the old battles to fight over again, this time with certain handicaps which we did not have before.

The humanities are now definitely alarmed. Profoundly mistaken, they are nevertheless thoroughly convinced that increased interest in the sciences, stimulated by scientific contributions to the war, will act to the disadvantage of the humanities. Professional journals are full of articles to this general effect. One can almost say that the academic equivalent of mob spirit is being aroused, with a view to tarring and feathering the sciences and riding them on a rail out of the liberal arts.

To keep the record clear I am sure that it is accurate and appropriate to say that the interest of science teachers in this problem arises not out of fear about continuation of the sciences but out of genuine concern for continuation of the liberal arts. Teachers of the sciences will always be in full demand in graduate and professional schools and, accordingly, they have no fear of unemployment. But they have a major contribution to make to the undergraduate liberal arts curriculum, which their concern for the liberal arts will not permit them to overlook. The contribution that the sciences have made during the past three centuries to the current pattern of human habits of thought is such as to entitle them to more, rather than less, prominence in the liberal arts.

The typical apologist for the humanities starts with the axiom that the liberal arts is the domain in which his subject is pre-eminent. He then identifies the sciences with gadgetry or at best technology and draws the conclusion that they have no proper place in the higher realm of the spirit in which the humanities dwell. The fact is, of course, that

* The substance of a paper read before the Science Division, Oberlin College.

the sciences are *potentially* (mark that word, for I shall return to it) at least as rich a source of meaningful imponderables as are the humanities. While the practical aspects of the sciences are not to be despised, their significance lies not so much in the multiplicity of inventions, which makes the world so different and life so much safer and easier than it was a century ago, as in the subtle conception which gave birth to these gadgets and which is vastly encouraged by their use—man's confidence in his intellectual supremacy over nature. And what could be more profoundly a matter of the spirit than that?

When science is viewed in this larger aspect, it is not its *effects,* profound and far-reaching though they are, that should be the primary interest of student and teacher alike, but the nature of the instrument itself. It is utterly unique. Literature and the arts have been produced principally by special geniuses, and the rate of such production appears neither to grow nor to improve with passing time. Current masterpieces of art and literature are of no greater merit, nor are they being produced any more profusely today in proportion to the population, than the corresponding products of two thousand years ago. In the sciences, on the other hand, is to be found the first large body of knowledge that is both sequential and cumulative. As a unified army, organized for a sustained assault upon the citadel of human ignorance, there has been nothing to compare with the sciences in the whole recorded development of human thought. It is possible to question the value of the material which the sciences discover; much of it seems trivial to the lay mind. One may also be fearful of the ultimate effect of scientific philosophy on human welfare; many thoughtful men hold science responsible for some of the major ills of the day. But whether for good or for evil, the fact that science dominates modern thought cannot be disregarded.

Teachers of science are not doing as much as they can to cultivate this rich heritage. This is the more regrettable in that relatively little additional time is required, very little in proportion to the values derived. It is rather a matter of the mode of presentation of the subject than of added time for unrelated material. The great bulk of the material is already before our classes in the usual course of conventional science instruction. It is indeed more completely in hand than it can ever be in courses in history of science or philosophy of science. All that is required to bring out the significance of the discoveries being described is the deft manipulation of relative emphases which any skillful teacher uses, consciously or unconsciously, as part of his stock in trade. No science teacher who has a vision of the prominent part played by his subject in the general intellectual enterprise need make any apology to the humanities for the breadth that science can impart to education. But the number who have that vision is disconcertingly small, and still smaller is the number of those who are willing to go out of their way to acquire it.

The value which the sciences might thus contribute to the liberal arts curriculum is, as I have intimated above, largely potential. There is, to be sure, no lack of those who give lip service to such a program. It is when we examine the texts that are used, the actual proportion of the students' study time that is assigned to the pursuit of these aspects of science, and the amount of attention given to them in examinations that one realizes in what low regard they are actually held by the typical science teacher. For, make no mistake, the importance that will be attributed to these elements of science by students, and indeed the measure of the teacher's own confidence

in them as elements of science instruction in liberal arts, is the proportion of emphasis that he gives to them through the conventional channels of instruction. He is in fact only self-deceived when he says, "Oh yes! I regard these as very important and I mention them in my lectures," and then leaves them completely out of the actual corpus of his instructional program.

Yet a liberal arts curriculum has a right to demand the more liberal elements as a prominent feature of science instruction. In their absence there is nothing in science offerings to distinguish between liberal arts science and technical or medical school science. It is precisely at this point that the most damaging criticisms of the sciences could be made by devotees of the humanities if they only knew enough science to make them. H. D. Gideonse, President of Brooklyn College, has remarked:

> Science as usually taught to liberal arts students emphasizes results rather than methods, and tries to teach techniques rather than to give insight into and understanding of the scientific habit of thought. What is needed is a truly humanistic teaching of science.

President Emeritus Nielson, of Smith College, once wrote:

> Especially in the natural sciences is it the case that the temptation to early and intense specialization has produced a specialist capable of training other specialists, but ill-adapted to educating youth between seventeen and twenty-two.

Mark Van Doren has recently written:

> It will be a long time before teachers have the bravery to extend their knowledge beyond the specialties they started with. A truly coherent curriculum demands that they should, and in some millennium they may.

I hope that Van Doren is as mistaken in this outlook as I believe him to be in some of the rest of his educational philosophy, but he had good ground for his pessimism.

Against the background of the inadequacies of their own training, considering the almost complete blind spot which the great majority of science teachers have at this point, it will be a long hard road for them to travel before they are able to meet their responsibilities as real educators in liberal arts colleges. Yet unless they discharge that responsibility, the sciences will, in the course of a century or less, disappear as a significant feature of the liberal arts program. It is cold comfort to realize that with such disappearance will occur the eclipse of the liberal arts college, for it cannot remain liberal after the amputation of what is potentially its most important member.

But if such a catastrophe comes about, science teachers will bear an important share of the responsibility as the body which determined the character of science instruction in liberal arts colleges. The great danger in their teaching lies, as I have already hinted, in their preoccupation with the training of specialists. Not that they should *not* prepare specialists! They will always have to do that and do it well. The danger lies not in doing that job too well but in doing it exclusively. Professor Sigerist, of Johns Hopkins, remarked shortly before the war:

> If the German academic world surrendered so readily to reactionary forces, it was largely due to the fact that it consisted of men who were specialists and nothing else. If we wish to educate a citizen to be able to think in terms of science and a scientist prepared to participate in social action, we must change our teaching.

There is reason to fear that we ourselves are not immune to the malady that overtook the German academic world. Because of the narrowness of specialization characterizing our graduate training and because of the pressure in later professional experience toward developing a fertile and rapidly expanding field, our science teachers have all become primarily subject-matter specialists, and only secondarily educators. In some cases their preoccupation has been with

research; in others with the training of specialists, a very different undertaking than the problem of fitting one's subject into a matrix of general education. Many of them seem, in fact, not to sense at all the change in the teaching problem which has been brought about by the great mass movement toward higher education that has occurred in this country during the past fifty years, still less the tremendous sharpening of that problem that has occurred with the cessation of hostilities. They will have to learn it, as the price of survival of their subjects in the liberal arts program.

The very mold in which all modern thinking is cast was shaped in large measure by such men as Copernicus, Galileo, and Vesalius in the sixteenth century, and the oftener and the more emphatically we say it and tell why, the better. Prior to their time all technological advances were thought of in terms of necromancy—black magic. These men, together with their contemporaries and successors, re-established the faith which had died with the Ionian Greeks, faith that the world was knowable to any man of intelligence, not merely the world of art and literature but the world of nature as well. That conviction had been lost for a thousand years, an era which we now call "the dark ages." When it was regained, a world that had been palsied for a millenium began once more to move forward with the might and majesty that only a deep and abiding faith in its own potentialities could have generated.

Once again, in this twentieth century era of specialization, we have all but lost the conviction that the world is knowable. If the second world war, with its vast acceleration of technology, brings back the prevalent feeling of the dark ages that all technology is necromancy, and encourages an attitude of frustration on the part of the educational world toward the sciences, then the liberal educational program is doomed, and for that doom teachers of science will bear a major responsibility. The fact is that, viewed in the large, scientists have done a very poor job at interpretation of their own field to their friends outside of it. To be sure, the warfare at the research frontier is exacting, and it is natural that those engaged in it should be loth to turn away from it to explain the fine points of the campaign to the people back home. But such preoccupation is suicidal.

The worlds of art, music, and literature have learned that lesson, perhaps because they had a head start of two thousand years on the sciences. Efforts to interpret art, music, and literature to the common man have, at least in recent centuries, been a major concern to men in those fields, not an incidental, perfunctory, and largely unwelcome activity as is the case with the sciences. Interpretation of the sciences is a major responsibility of the science teachers themselves. They cannot delegate it to philosophers or historians, by very reason of the effects of curricular specialization from which the educational world is now suffering.

The opinion is frequently expressed that the wartime demand for the sciences will result in boosting their prestige enormously in the educational scheme. There is reason to fear, on the contrary, that unless wartime acceleration of the applied sciences is followed by a corresponding acceleration in general comprehension and appreciation of the *spirit* of science, a serious rift will have been introduced into the structure of higher education which in the long run may cripple or even destroy it. The greatest and almost the only hope of salvation lies in the colleges of liberal arts. There is no undertaking to which such colleges could more appropriately or effectively address themselves.

RIVER COME CLOSER TO MY DOOR!

By CHARLES MORROW WILSON

IN TROPICAL, little-publicized Honduras the long incorrigible Ulua River is being made to build from silt wastes thousands of acres of the richest of farming lands. In this remote and least-developed of American nations, engineers and farmers from several of the Americas are causing one of the most violently destructive of rivers to change big areas of festering jungle swamps into highly productive farmsites.

The Honduran venture, already well begun, is both a new page in inter-American history and an unusually timely answer to the enormously urgent challenge of soil saving. Further, it is providing a new and timely approach to the age-old problem of man versus the river.

For rivers are far and away the most formidable looters and destroyers of productive lands. By primer economics for the great majority of nations productive soil is the ultimate savings bank and reservoir of survival. Yet fertile soil is what we, like most other nations of the earth, keep right on losing by the billions of tons, and the millions of acre feet. Soils are gouged away by ice and blown away by winds. But the really appalling and decisive losses are by way of the streams and rivers which each year in the United States alone carry away enough topsoil to feed at least ten million people—if the precious stuff only could be anchored and used.

Year after year these stupendous soil losses go on—spreading like some nightmarish and wasting disease over continents and islands, involving tonnage losses which nobody can accurately compute—nor define in credible arithmetic. But one fact is certain: The displacement of soils by surface drainage waters and the misplacement of these soils by rivers is one subject about which exaggeration is next to impossible. For example, experiments made by the University of Missouri in collaboration with the United States Department of Agriculture, with a medium clay loam soil on a slight (3.6 degree) slope, located several miles from a major river channel, showed the following soil losses from water erosion:

With a yearly rainfall of about 38 inches, one acre of the test land, when plowed and left bare, loses about 40 tons of topsoil; when planted in corn it loses about 20 tons; planted in wheat, about 10 tons; and in a sequence of corn, alfalfa, and wheat, about 2.7 tons. If one begins multiplying such typical soil losses by the hundreds of millions of acres in cultivation, he stands a good chance of stalling his adding machine and wrecking his confidence in man's prospect of survival. The experimental findings in soil losses via surface shed overstate those of level field lands, but definitely understate those of tens of millions of areas of sloping cultivations.

There is nothing remarkable about the fact that people generally resent and fear rivers and regard them as chronic liabilities, despite all the pretty songs, nostalgic poems, and cozy local color books about rivers; or the common knowledge that rivers are important, sometimes decisive, to national economies and histories. Yet even in these United States, where rivers have done so much to open frontiers, foal industries, and provide basic transportation, we, a nation of inveterate builders and unrivaled engineers, keep right on spending billions of man-hours and dollars in working against rivers—not with them. Our citizens or government agencies have succeeded in "harnessing" rivers for generating electric power, irrigating dry

lands, providing city water supplies and, in the illustrious instance of the Tennessee Valley Authority, effecting a really farsighted and well-rounded reclamation program; nevertheless, for every dollar or work-hour that we spend to cause rivers to produce value, we spend considerably more than another dollar in toilsome and oftentimes futile efforts to prevent destruction by rivers.

In the United States, as in other countries, a great part of these distinctly negative efforts have failed. We have built thousands of costly miles of levees. In some, though not in all instances, this has caused the bed levels of a given river to rise ever higher above the adjoining valley lands. Sometimes the levees tend to restrict the river's channel and thereby deepen it; frequently, they accomplish the opposite. We built the levees higher, and thereby engendered still bigger overflows and ever graver hazards to more millions of acres of essential valley lands.

It is true that some of the newer plans for river valley authorities and some of the recent works in headwater impoundment are far more constructive and ingenious than the old-fashioned "levees of fear and doubts." But by and large our prevailing estimates and strategies of river control are still the defensive sort. We keep on struggling to keep rivers from hurting us instead of devising ways to make them help us.

"Ol' Man River," the standout song from *Showboat,* reflects with folk-ballad honesty the riverbank worker's solemn awe of the mighty Mississippi—which unlike the poor Negro stevedore is not obliged to sweat and strain, loading barges, lifting cotton bales, or serving time in jail. Ol' Man River just keeps rollin' along, carrying uncounted millions of tons of silt in suspension—which spells more and more millions of acres of once-rich lands grown poorer, and more and more ruinous floods for the future. In terms of the valley man's viewpoint, the folk ditty "River Stay Way from My Door," with its somewhat lyrical pronouncement, "I ain't breakin' your heart, so don't start breakin' my heart," is unusually pertinent.

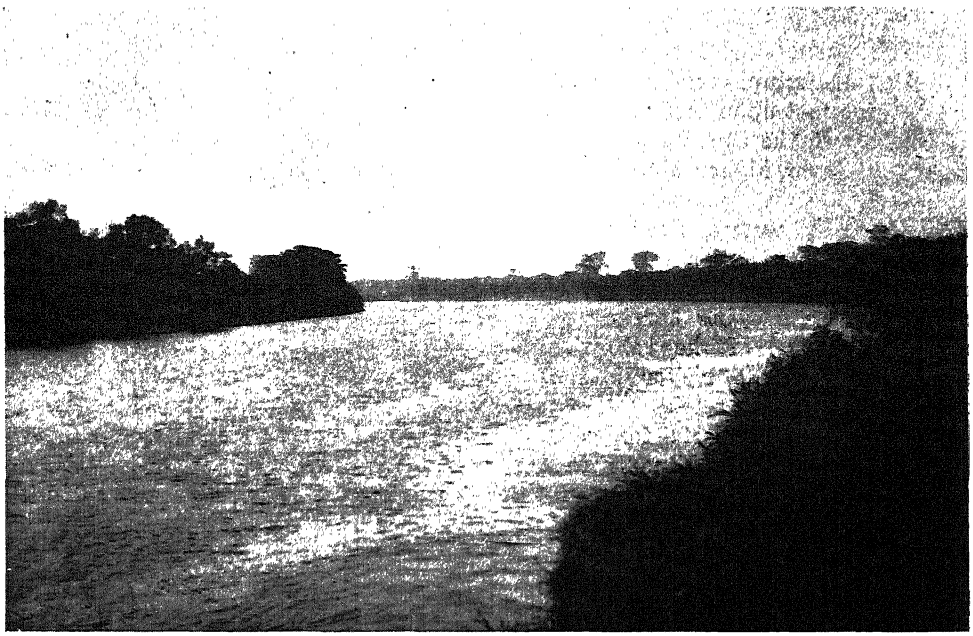
But the ever-troublesome case of man versus the river is not limited to any particular country or continent. It is a difficulty of mankind generally and a particularly serious problem for most of the Americas, especially true of the American tropics. The story of the Ulua of Honduras is broadly apropos of the grim battle because the Ulua is generally typical of many other difficult and temperamental rivers. Long before the coming of white men native Indians had listed it as a "bad river." The remarkable Hernando Cortes, who had a distinctive skill for appraising farmsites and settlement locations as well as harbor sites, even while dubbing the Ulua *Rio Malo,* helped to found a port settlement near the river's mouth and pointed out the extreme richness of the valley's land.

But the early Spanish farming settlements at the Ulua's mouth presently died, and for three more centuries the swamps waited as a farflung reservoir for mosquitoes and pestilence. In Honduras, as in most Latin-American countries, the towns and capitals grew inland among the foothills and mountains. Except for occasional fever-ridden squatters who built *manaca* shacks and tilled scraggly milpas or gardens along the scantily drained fringe of the built-up riverbanks, the lush coastal valleys of the Ulua were shunned and forsaken. The situation was broadly typical of a great many tropical rivers and of silt-bearing rivers generally, which tend to build up by silt deposit during overflow periods the areas immediately adjacent to their banks to a higher elevation than the more distant lands of their flood plains.

This holds for the general topography

of the Ulua Basin which in a straight line extends in a southerly direction from the Caribbean coastline to the *cordilleras* of the interior near the town of Potrerillos. The actual distance is only about 60 miles but in its snakelike course the river meanders through 120 miles. In the course of time and twisting the river has formed an alluvial plain about four miles wide at its upper end where the Ulua, Blanco, and Comayagua rivers converge, broadening to about 15 miles as the river proceeds towards the sea.

The Ulua emerges westerly from the mountains into this extensive valley. The Rio Blanco, which originates in the Lake de Yojoa region, cuts in from the central section, and the Rio Comayagua emerges from the eastern side to form the mighty Ulua proper. The Comayagua River flows through a break in the Sierra del Mico Quemado (Burnt Monkey Mountains) to join the twisting channel of the larger Ulua in its tortuous course through the sodden, swampy valley that spreads to the sea. In nine miles of its course the tributary Comayagua drops about 44 feet from the Santa Rita gap to its junction with the Ulua, which in turn drops about 36 feet more in 23 miles to the somewhat tenable area of the banana operating center of El Progreso, thence by flattening grade of about 80 feet more in 85 miles to the open sea. The Chamelecón River is another silt-carrying tributary from the interior Copán region.



ULUA RIVER NEAR THE TOWN OF PROGRESO

The worrisome habits of the Ulua system are habitual and recurrent. In Honduras, autumn and early winter, from late September to March, are the heavy rain seasons, though minor spring floods are also experienced. October usually finds the Ulua and its tributaries at flood stage. For an average of 30 days each year the river runs channel-full, and as the heavy interior and local

rains continue, the grim brown waters regularly spill over into the broad valley, in their haphazard manner sweeping away or burying in mud all farms, fields, buildings, bridges, and railroads in the course of the overflow, and eventually spilling the floodwaters farther down into a huge crescent of permanent and pestilential swamp lands. This had been going on for centuries, confirming the Ulua's name as a bad or incorrigible river. Its transgressions tended to grow with the passing years.

As with many alluvial streams, including the Mississippi and others in our country, as foot upon foot of silt settled in its channel, the Ulua's bed and banks were built ever higher above the level of its adjoining valley, and the overflowing silt-loaded waters, flowing without control, deposited the silt haphazardly, choking up smaller tributary streams and otherwise forming barriers to the natural drainage of the valley. The swamp areas grew bigger and more menacing.

The over-all prospects for effecting flood control for the Ulua were bad. One could build up embankments, revetments, and levees, the river bed would become higher, the flood would break farther down perhaps, but next year, or sooner, man's puny work would have to be done again. However, in the instance of the Ulua there was only one reason why people wanted to take farmlands in the swath of such a "mean bastard" of a river. Ulua lands are exceptionally good banana lands, well forward among the world's best. The climate, rainfall, and wind shelters are basically right for bananas, which in matters of soil, drainage, and climate are among the "choosiest" of crops. Hondurans and others had learned this even before the turn of the century, while the international banana trade was still in its creeping infancy.

As United States demands for bananas climbed and multiplied, peasants, local planters, and banana companies alike began to venture deeper into the wet, rich valley lands where the most coveted of tropical fruits grows so lushly. With passing years Ulua banana planting became decidedly important to the expanding banana trade.

By 1919, along with a considerable number of citizen planters, two major banana companies were clearing and planting costly and (they hoped) high-yielding and long-lived banana farms in the Ulua valley. On the east bank, or Tela side of the river, were the men and farms of the already strong United Fruit Company, headed by the shrewd and thoughtfully quiet Andrew Woodberry Preston, of Boston. On the west bank, or Cortes side, were the farms and railroads of the Cuyamel Fruit Company, headed by the even more brilliant Samuel Zemurray, previously of Mobile, New Orleans, and Warsaw.

As a matter of reason and expedience, both banana companies relied heavily on their respective engineering departments. More or less coincidentally, two young civil engineers, Patrick H. Myers of Tarheel, N. C., for Cuyamel, and Tom J. Barnett of Eureka Springs, Ark., for United Fruit, began independent studies of the rambling, flood-gutted Ulua.

The initial work was tough, sweaty jungle-fighting; exploring and surveying the mosquito- and alligator-infested swamps, lagoons, and arroyos; clearing the mighty forest trees and jungle bush, planting the bits or rhizomes of the bananas; laying first railroads, building field hospitals, commissaries, and quarters; and otherwise making the wet, hot valley lands livable and tenable. The banana-growing pioneers, like their predecessors, promptly learned to respect the Ulua's moods and might. Seeking the usual goals of protection from destroying floods and providing essential

drainage of the farmsites, they first raised levees and by use of large dragline excavators discharge channels were opened through the series of clogged and congested swamplands to carry standing waters away from the planted fields and to reclaim for planting thousands of acres of silted lands adjacent to the riverbanks.

It was exhausting work. On both sides of the river big planters and little planters alike continued to view the Ulua apprehensively. At first only fringes of riverbanks and minor areas of the valley lands could be planted. Many of the earlier banana fields were mere strips of two or three rows of the giant, pink-blossomed banana plants, closely paralleling the banks, which provided natural drainage for slim strips of lands immediately adjoining.

Through urgent necessity the banana engineers and farmers began a closer study of the Ulua. Pat Myers and his assistants identified the fact that when the Ulua is at high mark, or "banks full," the river carries approximately one thirty-second of an inch of silt per square foot of land surface per cubic foot of water.

They approximated the Ulua's average annual discharge of water as some 550 billion cubic feet per year. About one-third of the total volume passes through the channel during the principal month of floods. They estimated that as a year-around average the Ulua carries about 137 parts of silt per 100,000 of water by weight. All this suggested that during an average year the Ulua's waters carry in suspension enough highly fertile soil to cover about 25,000 acres, or about 40 square miles, of valley land with a nine- to twelve-inch layer of the rich new soil—provided the silt loads could be dropped at the providential time and place.

Therein lay the real rub. Preliminary analyses of silt contents of the river water, both as to soil chemistry and physical properties, were encouraging. But how could the rapacious river be made to part with the valuable soil? The textbook answer is: "by controlling the volume and velocity of the water flow." That defines the basically simple, highly constructive tactics of "practical sedimentation." In view of the indisputable fact that soil is the most vital of natural resources, why has ingenious man paid so little heed to so vital a salvage?

There is no satisfactory answer to this question. There are a few precedents, notably the century-old silt recovery program as practiced along the Nile Basin, in some instances at distances as great as 50 miles from the actual river channel. But rivers, like people, are never precisely alike. The Nile is one of the longest of rivers. The Ulua is comparatively short. The lower Nile is excessively sluggish. Its grade, or "longitudinal slope," is only one foot to 91,000; the Ulua's is one foot to 4,500. The Nile's silt load is proportionately slight. It deposits silt at the rate of about four inches in 100 years, or around one thirty-second of a foot per year. The Ulua is probably capable of building land at the rate of a foot per year—1,020 cubic feet of the Ulua's waters, containing one and one-third ounces of silt per cubic foot are theoretically capable of placing one foot of silt. But during much of the year the silt loads are greater than one and one-third ounces per cubic foot of water.

Along the Nile the beneficial strategy of soil creation via sedimentation emerged as a by-product of the necessary and vital irrigation practiced in that area. For centuries Egyptian farmers have drained or drawn the Nile's water into settling basins for eventual distribution to valley fields or garden plots. They permit the river silt to settle to the basin bed, built parallel to land contours. After the bottoms of the basin

have become spread with a replenishment of rich soils, they plant the earlier basin sites to crops.

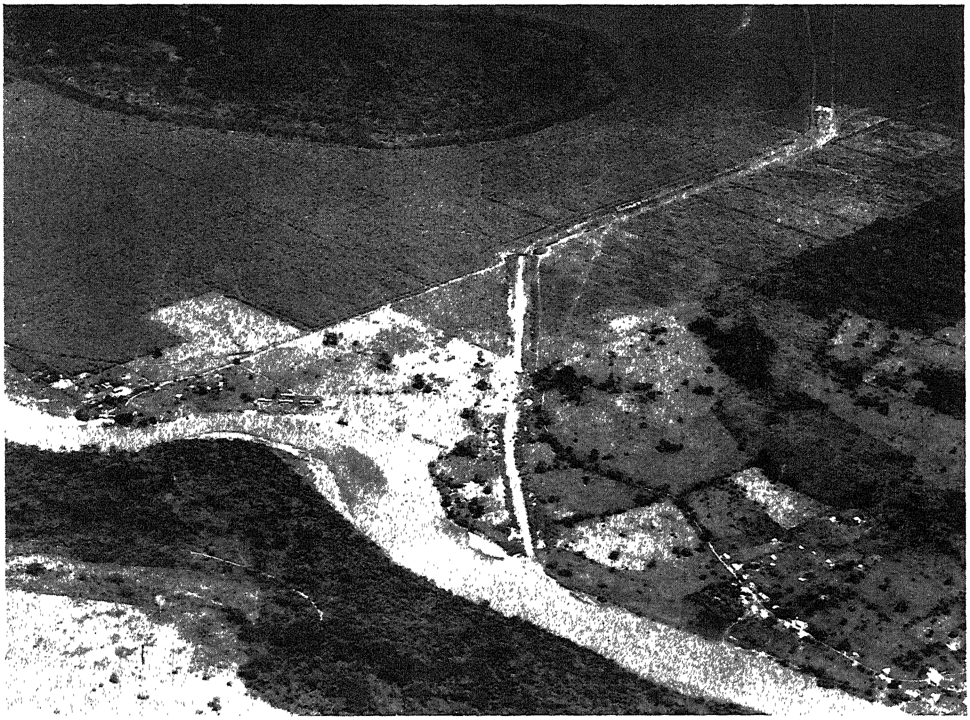
But irrigation was and remains the real motive back of soil building along the Nile. Essential flood controls and farm drainage paved the way to land building along the Ulua. But as an engineering feat, soil building by way of sedimentation along the Ulua is far more difficult than silt recovery along the Nile.

Late in 1919, Pat Myers, the gracious and imaginative civil engineer from Carolina, abetted and supported by Cuyamel's president, Samuel Zemurray, who knows the Ulua Valley as thoroughly as any man living, began a first soil-building venture on the left or west bank of the Chamelecón, shortly above its confluence with the Ulua. The primary objective was that of protecting low-lying farms from violence by floods. The first trial sought only to recover a small fraction of the silt loads available. Nevertheless, within twelve years a first 5,000 acres of land, with silt deposits ranging from six inches to ten feet, had been built up by planned and controlled flooding and silting operations. By moderate estimate, these first river-built lands are worth at least $200 to $400 per acre, or a million to two million dollars.

Still more valuable were the practical lessons learned, and the work manuals provided. In the beginning Pat Myers and his helpers had determined that a current velocity of around 4.5 miles per hour is best for purposes of land building. Faster currents are almost certain to spread heavy gravel, stone, driftwood, and other undesirable materials over the prospective fields without depositing the finer and lighter silt.

The successful exploitation of current

DIVERSION CANAL TO DRAIN A SWAMP AREA IN THE ULUA VALLEY



ULUA RIVER AND CULTIVATIONS DEVELOPED BY SYSTEMATIC SILTING

velocity calls for extreme finesse. Pat Myers and his men began to experiment with spillways at points along the riverbank to encourage the inflow of the floodwaters, and the use of various types of collapsible check dams, or wide-crested weirs, the latter built to facilitate the spreading of silt by waters drawn directly from the river channel and guided by inlet channels leading from the spillways to the lower waste areas. They employed dragline dredges and steam shovels to dig surface canals, or *bouqueras,* by means of which the river waters that poured through the spillways during high river stages could be swept back and forth at controlled velocity to the low stretches of valley where channel grade reduction and the lush swamp growths retarded the current, to cause the silt deposition where desired. That permitted the silt to settle on, and so build up, the swamp sinks that particularly needed building up.

The earlier phases abounded in toilsome and expensive trials and errors. But the rudiments were encouragingly simple: to minimize levee building; to cut into the river channel and take out the muddy water before it reaches bankfull flood stage by diverting it into *boqueras* so built and arranged that the silt-bearing water can be distributed or "swish-swashed" in a manner to retard its current and cause the silt load to be dropped as advantageously as possible; then to provide exits for the silt-free waters.

The method in principle also controls the flood stages of the river and thereby reduces or avoids the former flood damage to improved areas of the valley lands.

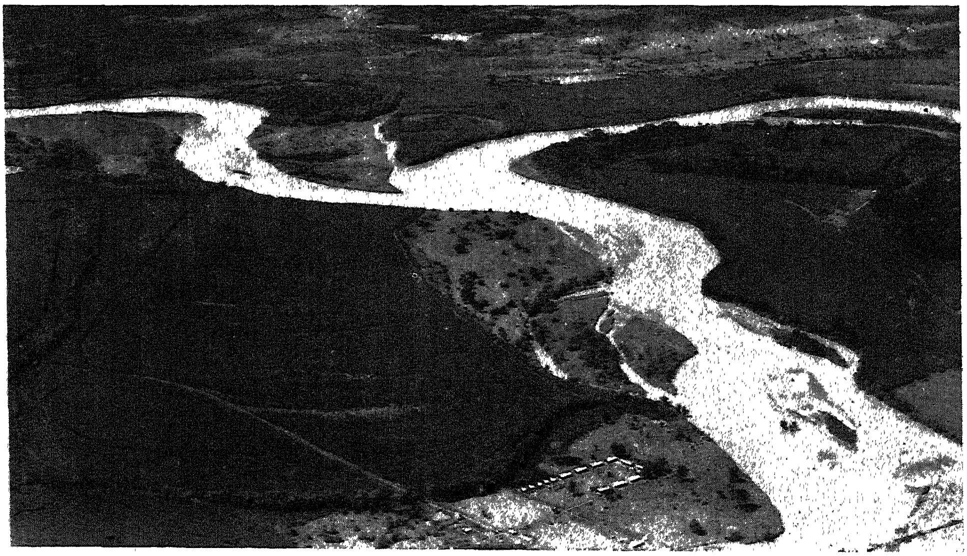
Cuyamel's first successful silting projects involved changing 5,000 acres of low,

miry valley swamps to tenable lands by the processes and strategies of silt recovery. A typical recovery district included 19 surface channels, each an average of two yards wide and from 2 to 2.3 miles long, spaced about 400 yards apart. To fill these canals silt-bearing waters were taken out of the river channel by way of a broad, shallow intake canal at the riverbank, the canal about 240 feet across and eight feet deep. For discharging the spent waters from the *boqueras* Pat Myers and Cuyamel designed two broad, shallow drainage canals, one about ten miles long, the other about seven.

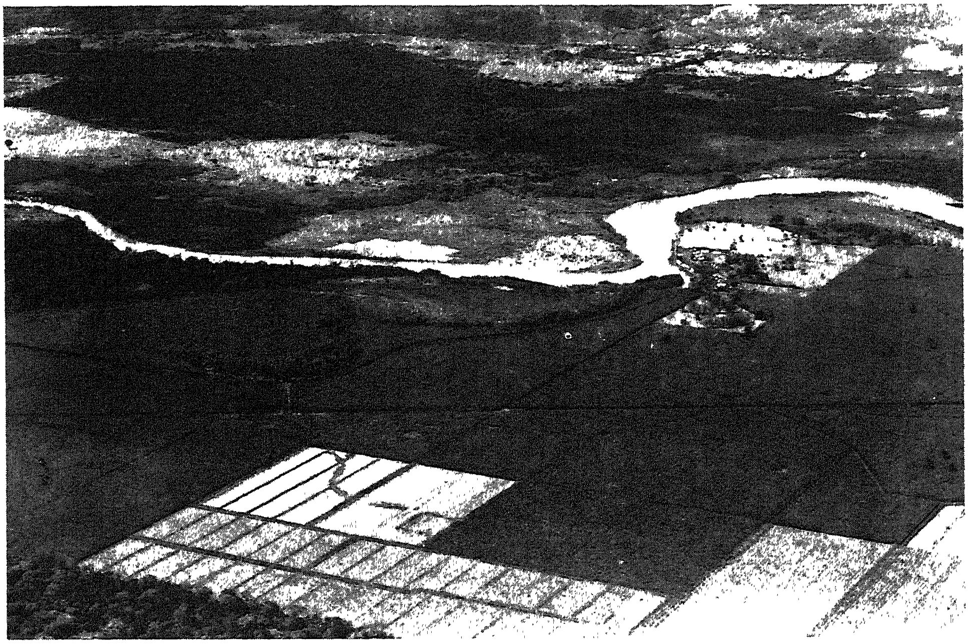
The first planning, though far from perfect, presently began to "take." In time the *bouqueras* and the swamplands between them became leveled and enriched by silt deposits ranging in depth from six inches to ten or twelve feet. By 1934 several thousand acres of the "made lands" were bearing profitable crops of bananas. Valuable lands had actually been created by controlled sedimentation.

Without any widespread notice the land building continued. Along the east side of the Ulua, the United Fruit men were finding more thousands of acres of bothersome swamps which by drainage and sedimentation could eventually be changed to valuable farming sites. Though the estimates have varied, Barnett and Myers and their assistants confidently predicted that in the valley as a whole at least 90,000 acres, or perhaps 100,000, of useless and troublesome swamplands could eventually be built into superb farming lands by the processes of sedimentation. By late 1929 when Cuyamel and United Fruit joined forces and properties, the land-building technique was rather clearly proved, even though some of its tactical details had not been perfected or agreed on.

But the banana *hombres* continued the land building along the Ulua. They have effected about 9,000 more acres of "upbuilding" on the east bank—in the Palomas and La Fragua sections, on the Tuaymas, Mezapa, Toloa, Melcher and Meroa banana farms, and other banana farmsites, including Guanacastal, Ticamaya, Kele Kele, Tibombo and Manacolito on former Cuyamel lands to the



BUILT LANDS AT THE CONFLUENCE OF THE ULUA AND THE COMAYAGUA



BUILT LANDS ALONG THE ULUA WITH GUANCHIA FARMS IN FOREGROUND

west. More recently privately owned banana properties, such as Finca Oro and the Birichiche Estate, have joined in the land-building venture.

The total was well past 15,000 acres as World War II began, enough to place the work clearly beyond the experimental stages. War shortages of the needed heavy machinery and available ships delayed the work considerably. But the Ulua land-building enterprise goes on—with the strong likelihood that the goal of 90,000 to 100,000 additional acres of river-built farm lands will ultimately be reached.

For the relatively little Ulua that is a big order. It requires, among other things, that the deep lower swamps be striated with immense discharge canals leading 25 miles or more indirectly into the Caribbean. This calls for the excavation of around 75 miles of such canals and the removal of at least 60 million cubic yards of mud, which in turn requires the use of enormously expensive floating and other equipment. The costs are certain to be heavy, at least $100 per "built acre," most probably even more. The background work alone will cost millions. The time factor is likewise considerable; an 18-inch silt deposit in five years is the mean rate of land building contemplated.

Good civil engineering has been and will continue to be essential, a caliber of engineering that can coordinate the necessary flood control and surface drainage work with effective land building. Mechanical engineering is also requisite since the work requires ingenious controls, such as automatic floodgates, to keep the flood loads within river channels and to avoid spreading the heavier sands and debris upon the tillable lands. The task requires skillfully built intake canals; the building of costly cement tunnels to carry silt-laden waters to the more distant field sites; and stubborn succes-

sions of well-designed spillway channels, or sluice cuts, to be filled with silt, blocked off, and replaced with other *bouqueras*.

Drainage of the built lands, which is essential for growing bananas, or almost any other profitable crop, poses further problems. Where "natural" or gravity drainage is not possible the standing water must be pumped away. And there are few absolute rules for building lands from river-borne silt. Each river, indeed each section of river with its immediately adjoining lands, presents specific problems and challenges. In some instances the native vegetation—trees or bushes or grass—can be used advantageously as anchorage for the silt deposits. But in one way or another the river water must be taken out of its regular channel and guided at prescribed velocity to low areas.

Pat Myers, now returned and retired to his North Carolina farm, agrees that building land by sedimentation is not magic—"It only looks and acts like it." Like other apparent feats of magic, inveigling a scoundrel of a river into building valuable land calls for exact timing. The general planning has to be shaped months or years in advance of the location work. But the latter frequently defies rules and calls for improvisation.

The Ulua adventures have proved that valid soil recovery and soil building can be accomplished. The land so built is not necessarily cheap, but it is immensely fertile. With its generally favorable topography and climate, the progress in river building of lands can assure the otherwise untenable Ulua Basin a permanent place as the No. 1 reservoir of the international banana supply, a fact of more than casual importance to consumer markets throughout North America, the British Isles, and Western Europe, with their more or less habitual banana eaters. Only an infinitesimal percentage of tropical lands, probably no more than one-twentieth of one percent, is suited to growing bananas profitably.

As already noted, the Ulua Valley, as refurbished by sedimentation, is probably the world's most favored banana site. But the possibilities of the Ulua land building outreach and outweigh the perpetuation or the well-being of any one crop. Most other crops also thrive on the silt-built lands—more lushly than on any other. Banana growers, in the line of business and with a routine profit motive, have made the Ulua Basin an international proving ground for land building via recovery of river silt.

Though it provides no absolute formula for land building in other river basins, the Ulua venture is significant laboratory work. A great deal of detailed study is requisite to venture a guess as to how applicable its reclamation may be to silt-bearing rivers in the United States or other Americas. But the challenge is defined rather clearly and the scope of importance keeps right on growing. Here in an American background, people are reclaiming at least a fragment of the colossal, incessant soil loot that is collected and in most instances destroyed by rivers.

FROM DUNGEON TO TOWER

By F. L. CAMPBELL

THE continuing appeal of the A.A.A.S. to its members and friends to provide funds for a building to house its administrative, business, editorial, and service personnel has stressed the inadequacy of space that the Smithsonian Institution is now able to provide. Indeed, the activities of the Smithsonian could easily absorb the space that we now occupy, and it is possible that we, as guests, have become a burden to our generous hosts.

Few members of the Association have seen our quarters in the Smithsonian Institution Building. Therefore, we thought it might be appropriate and helpful to demonstrate by means of four photographs the conditions under which we are now working and the use that is made of present space. We are now occupying one room on the second floor (22 × 25 feet), three rooms on the third floor (14 × 14, 17 × 18, and 14 × 14 feet), one room on the eighth floor of the Tower (9 × 13 feet), and one room on the ninth floor of the Tower (9 × 13 feet).

Let us begin at the bottom and work up. To reach the second-floor room, formerly used for storage only, one must go to the third floor, walk to the rear of the building through the herbarium, and descend a stone stairway only 35 inches wide. One feels that he is going down to a dungeon, and so we have come to speak of this room as "the dungeon." Before fluorescent lights were installed the room itself was reminiscent of a dungeon because only a little light enters through the high portholes shown in Figure 1. During the past year it became necessary to use this room for an office as well as for storage. Supplies, symposium volumes, back issues of the SM, and various records were stacked on shelves to the ceiling on three sides of the room. Office furniture had to be taken apart to carry it down the narrow

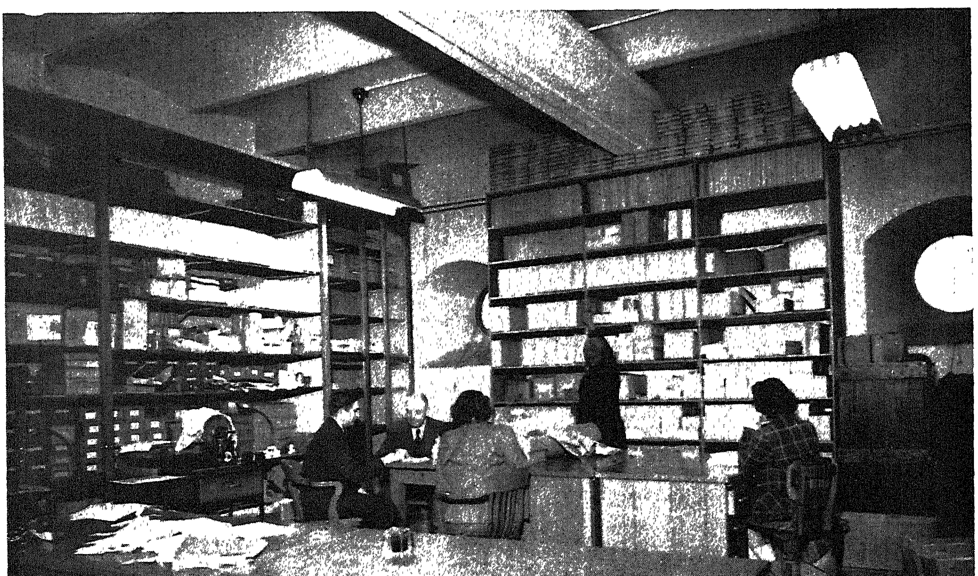


FIG. 1. THE DUNGEON, OFFICE OF THE PERMANENT SECRETARY

stairway. Recently, the Permanent Secretary, Dr. F. R. Moulton, felt it necessary to vacate his office on the third floor and move to the dungeon in order to provide more space for the business office. Thus in Figure 1 we find the chief administrative officer of the Association (facing the camera) talking with a visitor in a room that would be spurned by any executive less determined than Dr. Moulton to serve the Association. We shudder to think of the impression of the Association that visitors receive when they finally reach the dungeon.

We are glad to ascend again to the third floor and go to the front of the building where the Association's business office has fairly normal windows. The photograph shown in Figure 2 was taken from the doorway of Dr. Moulton's old office, now used by the new Administrative Assistant, Dr. John M. Hutzel. Only the central office and a glimpse of the room beyond can be seen in this picture. The filing drawers in the background contain cards bearing the names and addresses of members of the Association and a record of their payment of dues, etc. Desks and other filing cabinets leave little room for passage. Here must be handled thousands of checks, subscriptions to the SM, inquiries regarding membership and journals, changes of addresses, orders for copies of symposium volumes and the SM, solicitation of new members, and—perfection being impossible—complaints. The volume of record-keeping and mailing done in this little office is staggering, and the lights burn there nearly

FIG. 2. RECORD AND ACCOUNTING OFFICE OF THE A.A.A.S.

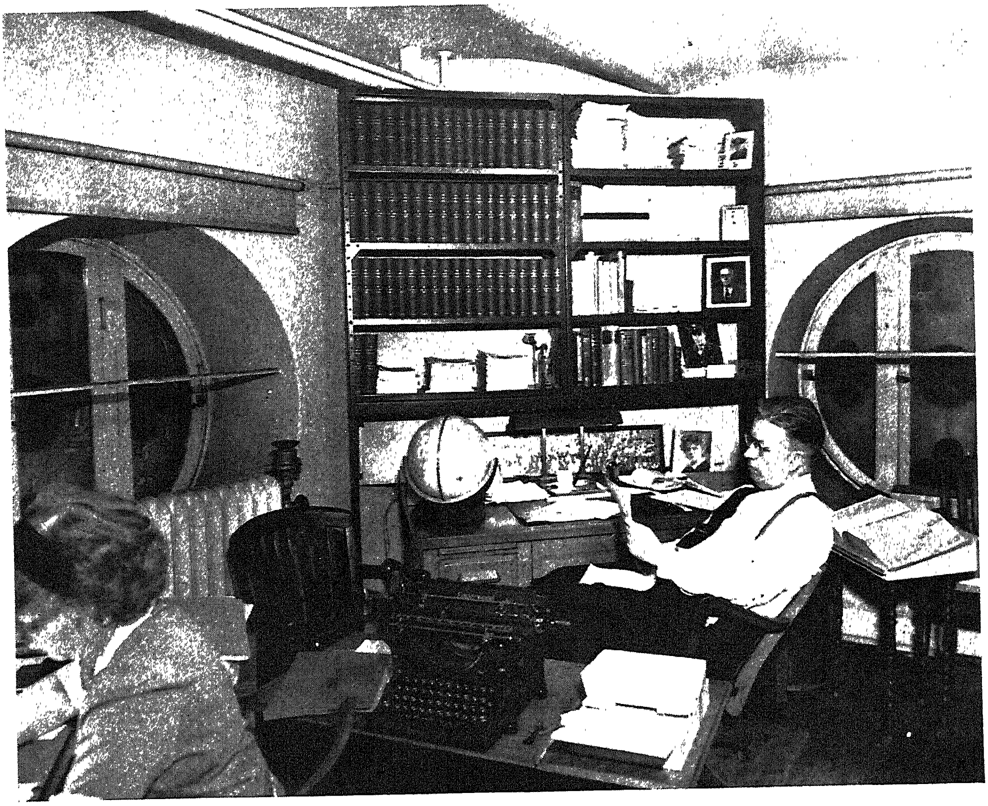


FIG. 3. OFFICE OF THE EDITOR OF THE SCIENTIFIC MONTHLY

every night. In the far room are the files of addressograph plates and the addressograph and stamping machines, together with supplies and a large collection of college and university catalogues. We wonder that the clerical help is willing to work under such pressure and crowding, and, indeed, the turnover has been high. The training of each new employee adds to the almost intolerable burdens of the manager of this office.

Let us leave this sweatshop and go to the best of the Association's rooms—the office of the editor of the SM on the eighth floor of the Brownstone Tower. To be sure, it is very small and a firetrap, but the view is inspiring, and it is warm in winter and cool in summer. The photograph (Fig. 3) shows about half of the room including the north and west windows. The editor has his feet on the visitor's chair, and his assistant works at her desk. We could not show the vertical steel ladder in front of the south window, nor the elevator, sink, and filing cabinet. In case of fire we would have to open the trapdoor in the floor, climb down the ladder to the floor below, and open a window to a fire escape that leads to the roof of the main building. After having reached the roof, we could probably find a spot from which we could jump into the firemen's bouncer. If it is necessary to enter or leave the room when the elevator breaks down, the ladder must be used. Fortunately the elevator is fairly dependable, and I have had to climb the ladder only about twice a year. For a woman, how-

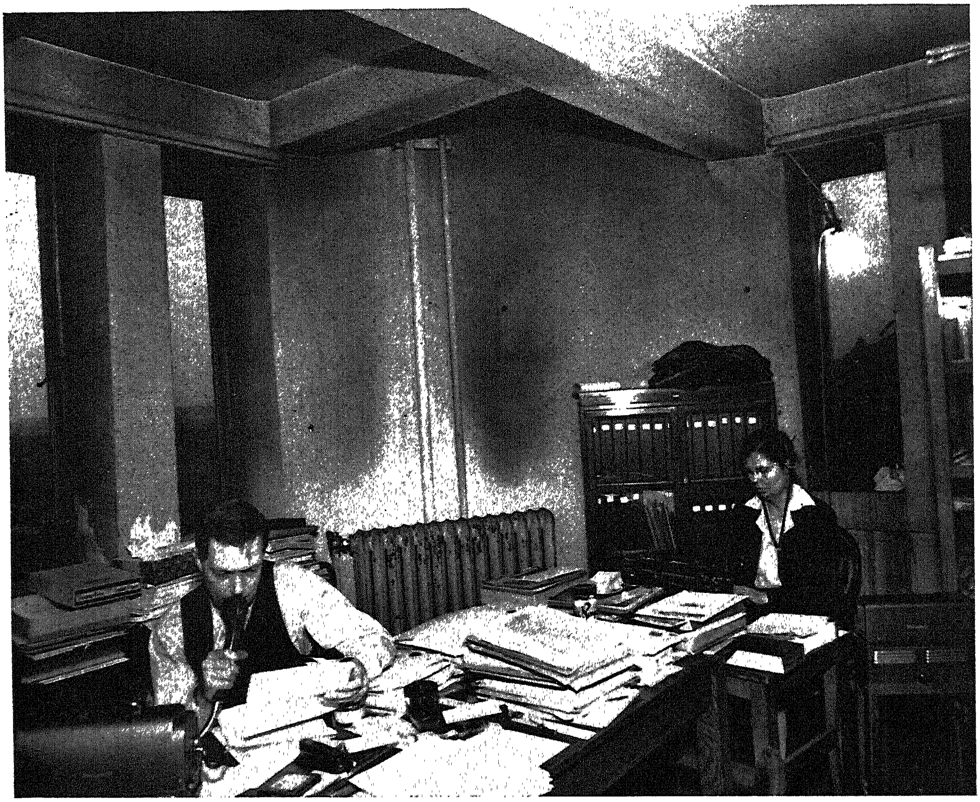


FIG. 4. THE EXECUTIVE SECRETARY'S OFFICE IN THE BROWNSTONE TOWER

ever, the prospect of using the ladder is not pleasant.

The same hazards and inconveniences apply to the floors above. The ninth floor of the Tower (Fig. 4) is occupied by Dr. Howard A. Meyerhoff, Executive Secretary, and Mrs. Meyerhoff. When Dr. Meyerhoff moved into this room, containing only a table and chairs (the cabinets are used by the Smithsonian), it was understood that he might be asked to vacate at any time. He could move into the dungeon, but his makeshift room in the Tower seems the lesser of two evils, so he clings to it. Because his space is temporary, he does not have filing cabinets or a telephone. A geologist, he must use what Austin H. Clark calls the geological filing system, that is, the building up of sedimentary deposits of letters on a table top. If he can remember the various strata on the table, he may find what he wants when he wants it.

Dr. Meyerhoff uses our telephone, and we call him to it many times a day either by shouting or pounding on his trapdoor. Unless the elevator is standing at his floor, he opens the trapdoor and climbs down the ladder to answer the telephone, then he climbs up again. It is very good exercise for an office worker, but somehow it seems ridiculous for an executive officer of the Association to spend so much time climbing like a monkey.

Our medieval excursion from dungeon to tower is over. Tell us what you think of it—preferably by sending us a slip of paper inscribed, "Pay to the order of the A.A.A.S. Building Fund."

AFRICA NEEDS PALMS AS TREE CROPS

By O. F. COOK

THE native flora of tropical Africa is remarkable for the paucity of palms. Each of the continents has its distinctive palms, but the African series is extremely short. In the absence of undergrowth palms, the African forests appear much less tropical than those of Central and South America, and most of the open bush country in Africa is without any palms. African coast settlements have a few coconut, date, or oil palms planted in gardens or allowed to grow from chance seedlings, and the oil palms often spread in wastelands, but relatively few interior districts have palms in abundance. Thousands of photographs of interior landscapes have been published in works of travel during the past half century, but they seldom show palms, and in these books the native kinds are rarely mentioned.

Although the tropical area of Africa is the greatest of any continent, most of it is wasteland, denuded and depopulated through recurring cycles of native agriculture and the repeated cutting and burning of the forest to make temporary clearings for rice or other field crops. The tillage methods often urged by visitors from temperate regions are destructive, inducing more rapid erosion. The practical alternative in tropical agriculture is to develop the use of trees and tree crops to obtain permanent protection for the soil. In such a reclamation of Africa by tree crops the palms may be expected to contribute more than any other group of plants. Some of the native palms are capable of being utilized extensively, and many useful kinds from other countries could be introduced. The range of specialized adjustments to particular conditions of growth is remarkably varied among the members of the palm group, from shaded forest depths and seething tropical swamps to chilly mountain summits, windswept seacoasts, and the barest, hottest deserts.

In the tropical belt of Africa only four families are represented among the native palms, and the indigenous genera are much fewer than in Asia or in the islands of the Indian Ocean. Other palm families are highly developed in the Malay region, but are wanting in Africa. All the genera in tropical Africa are the same as or closely related to palms found in Asia or in Madagascar, the island itself, however, having many more palms than the continent. Queensland, the tropical corner of Australia, has more native palms than the entire continent of Africa.

From tropical Africa about fifty species of palms have been named, whereas from the American tropics more than twenty times as many palms are known, and many new species are still being discovered. In contrast to the four families of palms found in Africa, fourteen families are recognized in America, most of them not represented in the Old World. Cuba, Haiti, and Trinidad have notably richer palm floras than tropical Africa. Even the tropical extremity of Florida has its native palm flora with nearly as many kinds as the tropical belt of Africa. The palmettos frequently appear as dominant features of the landscape in Florida but never in Africa. Two of the African genera, Chamaerops in North Africa and Jubaeopsis in South Africa, are not found in the equatorial belt, but are definitely extratropical, like the palmettos of our southeastern states and the molasses palm of Chile, *Jubaea chilensis*. The Chilean and the South African palms are closely related, as are the

Mediterranean Chamaerops and the Serenoa, or saw palmetto, of Florida.

On the American continent the tropical vegetation includes diversified local floras of small undergrowth palms, often a dozen or twenty kinds in the same locality. There are hundreds of species of these small shade-tolerant palms in tropical America, classified in numerous genera of several families, with various adaptive features. Other groups of plants in tropical America are remarkably specialized for life in forests as undergrowth or as epiphytes, notably cyclanths, orchids, begonias, bromeliads, cannas, and Heliconias. The Asiatic series of specialized shade plants also includes many palms not represented in Africa.

The comparative development of the undergrowth types of vegetation may be taken to indicate the relative age of forest associations of the continental areas. In such a reckoning the forests of tropical America appear to be the oldest, those of Asia intermediate, and the forests of Africa more recent, a relation consistent in general with the westerly direction of the trade winds. The world-wide distribution of the ancestral palms doubtless was attained in advance of the forest period. Continuance of the open preforest conditions in Africa is indicated by the wealth of terrestrial mammals, whereas in the forests of South America the mammals are largely arboreal, hardly less specialized than the birds for living in the treetops, as Bates so vividly described in *The Naturalist on the River Amazons.*

Rattans the Only Forest Palms. In most of the African forests the only palms, if any, are the very slender, long-jointed rattans trailing like lianas among the treetops, where travelers seldom see them. The leaves of the rattans have long midribs extended far beyond the pinnae to form slender climbing organs, beset with hooked spines. Botanists have recognized Calamus and four other genera of rattans in Africa, but with differences apparently no greater than among the hundreds of Malayan species referred to Calamus or to Plectocomia, all with similar habits of climbing above the forest canopy and keeping out of sight, unless from airplanes.

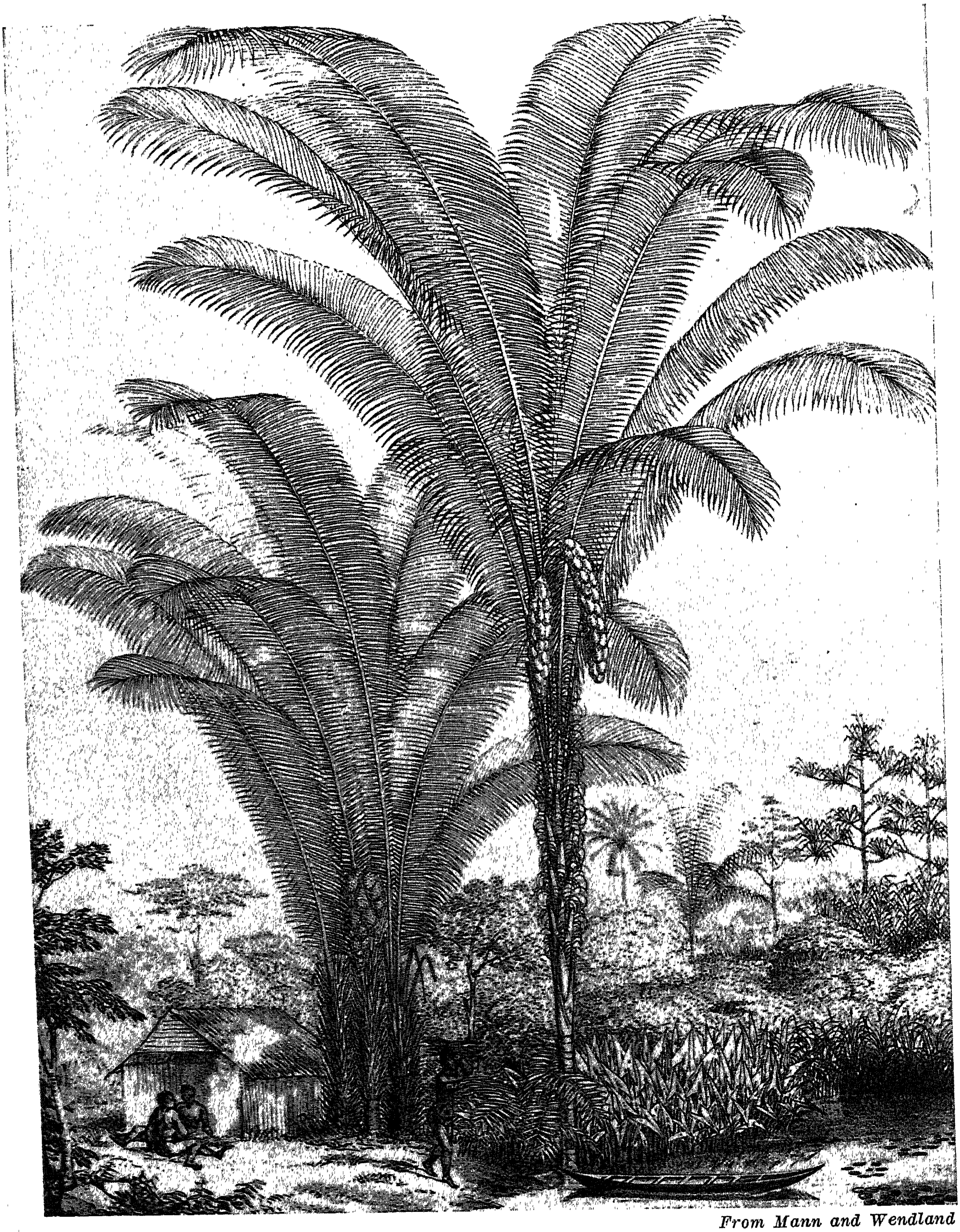
Two localized genera of very small palms, Sclerosperma and Podococcus, were described in 1864 by Mann and Wendland from swampy places along the Gabon and neighboring rivers, but apparently not spreading as forest undergrowth. Excellent drawings of these palms and of two of the African rattans were published and are reproduced in Figure 1. Young rattan palms, before the stage of climbing, are shown at the lower right-hand corner of the illustration. Other localized species of Sclerosperma and Podococcus have been reported from neighboring districts, but the later explorers have not discovered additional genera.

The Raphia, or Wine Palms. After taking account of the rattans and the oil palms only four other African palm types remain. The type that occurs most widely, scattered along riverbanks or clustered in open swampy lands, is known in Africa as the wine palm, the genus Raphia, and is found also in Madagascar and in tropical America. The trunks usually are short and the leaves few, very long, and spreading. The African wine palms formerly were supposed to represent only one species, *Raphia vinifera,* but several local kinds have been named. Two were described by Mann and Wendland from the island of Corisco off the coast of Cameroon: *Raphia longiflora* and *Raphia hookeri,* the first with short trunks, the second a rather majestic palm, with trunks thirty feet high and leaves forty feet long (Fig. 2). The larger species was reported to have been cultivated by the natives at old Calabar.

From Mann and Wendland

FIG. 1. FOUR SPECIES OF AFRICAN PALMS

ON THE LEFT ARE TWO DWARF PALMS, *Podococcus,* WITH SEPARATE SLENDER TRUNKS, AND IN THE CENTER IS *Sclerosperma,* WITH THE LEAFSTEMS CLOSELY CROWDED. IN CENTRAL BACKGROUND AND ON THE RIGHT ARE TWO CLIMBING HOOK-LEAVED RATTAN PALMS: *Calamus laevis* (CENTER) WITH RELATIVELY SHORT LEAVES AND TAPERING PINNAE, AND FIVE STALKS OF *Calamus secundiflorus* (RIGHT), THE THREE UPPER STALKS WITH LEAVES OF THE MATURE FORM AND TWO YOUNG STALKS BELOW.



From Mann and Wendland

FIG. 2. TWO AFRICAN WINE PALMS

THE WINE PALMS ARE CHARACTERIZED BY THE VERY LARGE LEAVES WITH REGULAR SPREADING PINNAE. TWO SPECIES OF WINE PALMS WERE FOUND BY MANN AND WENDLAND GROWING ON THE ISLAND OF CORISCO IN THE GULF OF GUINEA. THE SMALLER SPECIES, *Raphia longiflora*, HAS ONLY A SHORT TRUNK LIKE MOST OF THE OTHERS, BUT *Raphia hookeri* IS REMARKABLE FOR ITS SIZE—THE TRUNK MAY BE 70 FEET HIGH AND THE LEAVES 40 FEET LONG WITH 190 TO 200 PINNAE ON EACH SIDE OF THE MIDRIB.

The fruits of Raphia are characteristic, two to four inches long, elliptic in outline, and covered with smooth, shining scales like pine cones. Each fruit contains a single, large ivory-hard seed, the surface coarsely wrinkled and surrounded in the fresh state by a thin flesh, yielding an edible yellow oil like that of the oil palm, Elaeis. Fibers of two kinds are obtained from Raphia: a soft so-called raffia stripped from the epidermis of the young unopened leaves, and a coarse piassava fiber retted from the leafstalks.

The Borassus, or Palmyra Palm. The massive palmyra palm, *Borassus flabellifer,* extensively cultivated in the East Indies, is represented in Africa by a closely related species, *Borassus aethiopum,* one difference being that the trunk of the African species is more definitely thickened near the middle. A height of 100 feet or more is attained, with a diameter of two to three feet. Forests of Borassus are reported in the western Sudan and along the coast of Angola, and it occurs locally from Senegal to Mozambique. The stiffly radiating segments give the leaf blades a spread of about five feet. A vigorous young palm growing in the open forms a cylindrical crown of deep green leaves reminiscent of the early stage of *Washingtonia robusta* in California. A young Borassus palm on the St. Johns River in the Bassa district of Liberia is shown in Figure 3.

The fruit of Borassus is nearly round, six to eight inches in diameter, the largest of any palm except the coconut and the so-called double coconut of the Seychelles Islands near Madagascar, which is related to Borassus. The husk of the fruit is smooth outside and fibrous within, normally containing three flattened, fiber-covered, hard-shelled nuts, embedded in a sweetish pulp. In India the palmyra palm is valued chiefly for its sugar-bearing sap, while the trunk, leaf stalks, and leaves are a source of wood, fiber, and paper. The germinating seedlings form a starchy, edible underground bulb like an inverted parsnip. As in most of the palms, the terminal bud is tender and edible, like cabbage, though in a few kinds the tissues are too bitter or acrid to be eaten raw.

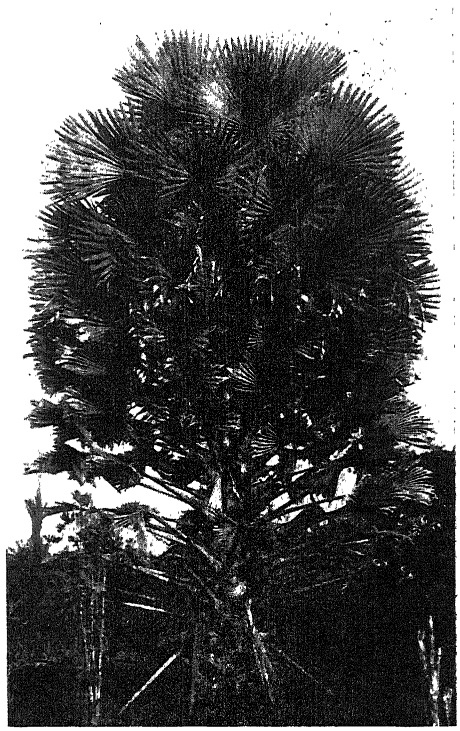


Photo by Guy N. Collins

FIG. 3. A YOUNG BORASSUS PALM

THIS SPECIMEN, NEAR ST. JOHNS RIVER, LIBERIA, IS GROWING IN A NEGLECTED COFFEE PLANTING.

Commercial Importance of the Oil Palm. One palm in tropical Africa has outstanding commercial importance, the African oil palm, *Elaeis guineensis* (Fig. 4), which furnished the principal exports of the West Coast of Africa after the slave trade was suppressed. Because palm oil was used for feeding the Negroes on the slave ships, it was generally believed that the oil palm was indigenous to Africa, but, as I have shown in the SM, June 1942, it now appears to have

been introduced (Fig. 5) from Brazil in the early colonial period to the Portuguese settlements on the West Coast. The oil palm grows in a wild state on the coast of Brazil, and a closely related genus (Alfonsia) occurs throughout northern South America and in Panama and Nicaragua. The oil palm is now widely distributed in tropical Africa, especially along the West Coast, and often in great numbers, since it spreads readily in open places.

The trunk of the oil palm is somewhat thicker and more erect than the coconut palm, with the leaves more numerous and forming a dense, feathery crown, on account of the pinnae standing at various angles to the midrib, and not in regular lines as on coconut leaves. The plum-like fruit grows in densely crowded heads protected by needlelike spines. The yellow palm oil comes from the outer flesh surrounding the black palm nut, with its hard, brittle shell and its white palm kernel, in taste and texture like the meat of the coconut. The kernel oil is clear, transparent, and tasty like coconut oil. In addition to the yellow palm oil, vast quantities of kernels are exported and the oil is expressed in Europe and America. In Africa, for emergency use, a little kernel oil may readily be extracted by parching handfuls of kernels for a few minutes in a hot kettle, for use with rice or cassava.

The Date Palms, Cultivated and Wild. The cultivated date palm, *Phoenix dac-*

Photo by Guy N. Collins

FIG. 4. OIL PALMS IN LIBERIA

THESE PALMS DEVELOP ONLY IN OPEN PLACES. HERE THEY ARE GROWING BY THE SEA NEAR MONROVIA.

From Monteiro

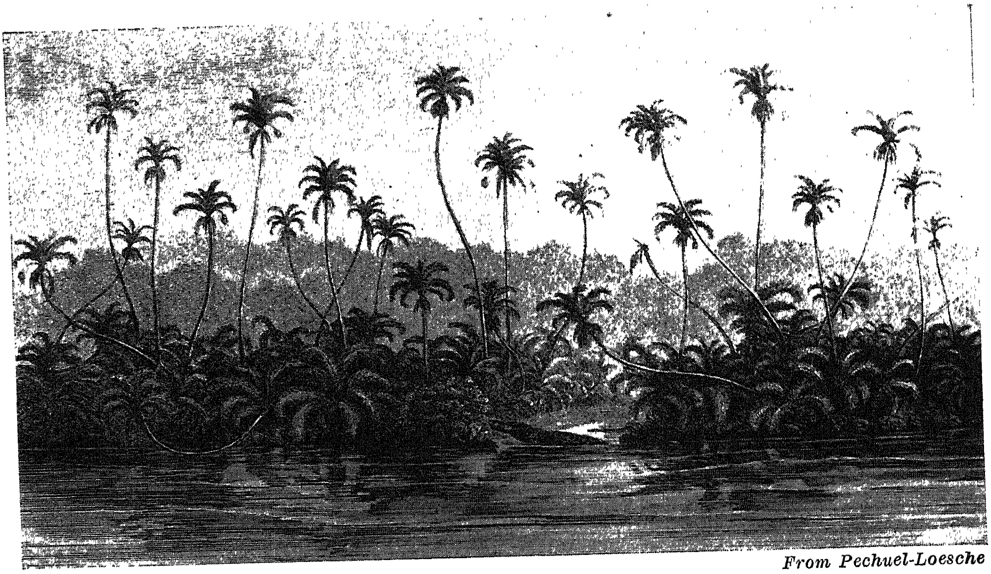
FIG. 5. OIL PALMS ON THE QUANZA RIVER, ANGOLA

IN THIS DISTRICT THE EARLY PORTUGUESE MISSIONARIES MADE PLANTATIONS OF COCONUT AND OIL PALMS. ANGOLA WAS COLONIZED BY WAY OF BRAZIL, WHERE THE OIL PALM OCCURS IN A WILD STATE.

tylifera, is a desert species, planted chiefly in Egypt and the oases of the Sahara, also in Senegal and the Sudan. As recognized in Pickering's *Races of Man* (1876), the cultivated date palm probably was not indigenous to Africa. Bertram Thomas in *Arabia Felix* (1932) shows a photograph of wild date palms growing on sandy flood banks in a previously unexplored district of southeastern Arabia, these conditions affording full exposure, with no competing vegetation. The photograph of these Arabian palms shows large symmetrical crowns of spreading leaves, only remotely like the wild dates in Africa. The cultivated date is definitely a sun palm, not making vigorous growth under shade conditions.

The native dates (Fig. 6) in tropical Africa, usually referred by botanists to *Phoenix reclinata* or to *Phoenix spinosa*, are slender, small-fruited palms, forming clusters of offshoots among stunted vegetation along the seacoasts, and sometimes on riverbanks or in swamps. These wild date palms are little used by the natives, but because they sometimes furnish fiber and palm wine, they are often confused with Raphia palms. A critical distinction is that the pinnae of the date palm leaves are V-shaped in cross-section, because in the bud stage the pinnae are folded upward, whereas Raphia and all the other palms have the pinnae folded downward, with the groove underneath. The date palms in reality are related to Chamaerops and other fan palms but appear different on account of having separate pinnae on a strong midrib. The leaves of fan palms are formed of radiating segments folded upward like the pinnae of Phoenix, but not separated.

The Doum, or Ginger Bread Palms. The doum palm of the Egyptian deserts is *Hyphaene thebaica*, and similar species are widely scattered in the Sudan and other dry districts, south to Mozam-



From Pechuel-Loesche

FIG. 6. TROPICAL NATIVE DATE PALMS

THE NATIVE DATE PALMS OF TROPICAL AFRICA HAVE SLENDER, CURVING TRUNKS AND SEND UP SHOOTS FROM THE GROUND, FORMING LARGE CLUSTERS WHEN GROWING IN OPEN PLACES. UNABLE TO GROW IN FORESTS, THEY ARE GENERALLY CONFINED IN NATURE TO SWAMPS, RIVERBANKS, AND SEACOASTS.

bique and Angola (Fig. 7). The leaves are fan-shaped as in Borassus, but the palms are much smaller. The fruit, only two to three inches in diameter, is somewhat pear-shaped or irregular. It has a hollow kernel, white and very hard, like vegetable ivory, encased in a black, bony shell, and this is surrounded by a rather woody, brownish rind with a pungent, sweetish, "gingerbread" taste and smell, not unlike the edible pods of the mesquite or the carob. A similar palm called Medemia is reported from Nubia as having bitter, inedible fruits. Seeds of Medemia have been found in ancient graves.

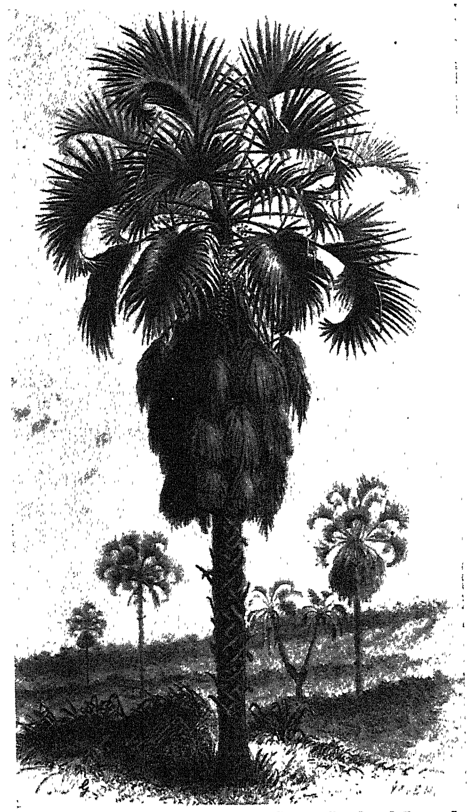
Remarkable resistance to heat and dryness is an outstanding feature of the doum palms and may render them useful in the reclamation of districts that now are badly deforested. Even in the superheated depressed valleys explored by Nesbitt between the Abyssinian highlands and the Red Sea, doum palms were found growing in the salt-sinks, beyond the range of any other vegetation and under the most extreme conditions. The title of Nesbitt's book, *The Hell-Hole of Creation*, seems appropriately to mark this outstanding adaptation of the doum palms to difficult conditions. The lack of a proper desert flora of surface succulents or other plants specialized to grow in the open in the Egyptian and Arabian deserts may mean that original forest covering was general and that the present extensive wastes are artificial.

Reclamation with Tree Crops. The need of tree crops is urgent in many districts on account of the rapid increase of native populations during the period of colonial control of tropical Africa by European nations. Many of the primitive tribes have reached the limit of their food supply. The native life may be said to have ended, since a different footing must be found for the future. Cutting and burning the forest to make temporary clearings for rice or other field

crops has resulted in extensive replacement of forests with open wastes or fire-swept grasslands. Tree-crop agriculture is more desirable, because erosion is avoided and permanent systems of production become possible.

Tree crops have been little used in tropical countries, and those contributed by families of plants other than palms are mostly fruits or nuts, such as the avocado, the sapodilla, the mango, the cashew, and the Brazil nut. However, in the palm group alone, five of the principal requirements of human existence—starch, sugar, oil, fiber, and building materials—are obtainable from tree crops. Some of the useful palms, such as the date, the palymra, the sugar palm (Arenga), the coconut, and the peach palm (Guilielma), have been cultivated elsewhere for long periods, and several others have been exploited extensively in the wild state. Examples are the sago palm (Metroxylon) and the rattan (Calamus) in the Malay region; the wax palm (Copernicia), the babassu (Attalea), and the assai (Catis) in Brazil; the vegetable ivory (Phytelephas) in Ecuador; and the molasses palm (Jubaea) in Chile. Other oil palms of potential value have only recently become known, such as Bornoa in Haiti and Ynesa in Ecuador.

Wider utilization of the oil palm in Africa may be a first project in tree crops. Palm oil is produced extensively in West Africa, but usually from wild palms. The seeds are widely scattered, and a little care in keeping the weeds from shading and smothering the volunteer seedlings might increase the food supplies of many tribes in a few years. A basic ecologic fact has been overlooked, even by scientific writers; the oil palm, like the coconut, is definitely a sun plant, requiring full exposure for normal development. Among the natives of Liberia fruits of the oil palm often are carried to eat on the road, the pulp chewed off and the nuts thrown aside, so that native pathways miles away from the palms may be bordered with seedlings, even in forests too dark for any of the seedlings to grow up. An indigenous palm commonly used and scattered by the natives should have spread long ago completely across the continent, instead of being restricted largely to the West Coast. Varieties with thick pulp and thin shells are being cultivated on a commercial scale in the British and French colonies. Oil palm plantations in the East Indies were expected to prove more profitable than coconuts. A general utilization of oil palms may be foreseen in many tropical countries.



From Pechuel-Loesche

FIG. 7. A DOUM PALM

FROM WEST AFRICA, CLOSE TO THE CONGO RIVER.

THE LENGTHENED SHADOW OF A MAN AND HIS WIFE—I

By JAMES G. NEEDHAM
PROFESSOR EMERITUS OF ENTOMOLOGY AND LIMNOLOGY, CORNELL UNIVERSITY

THIS is the story of the development of a new department of university instruction, the Department of Entomology in Cornell University. In attempting to tell it briefly I will first set down what I have heard from the lips of others, my academic elders, and then something of what I myself have heard and seen.

THE MAN who founded the Department, John Henry Comstock, came to his lifework by a very circuitous route. There was little in his early environment to urge him toward a scientific career. His deep love of nature he seems to have had from his mother. His parents were poor. His father, Ebenezer Comstock, had been a schoolteacher in Massachusetts. His mother, Susan Allen, of the family of the colonial patriot Ethan Allen, was a woman of quality. After their marriage they went west and settled on a small farm at Janesville, Wis. There in 1849 John Henry Comstock was born.

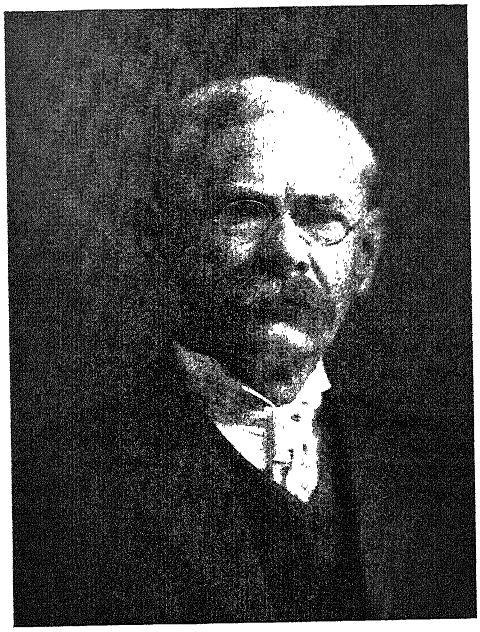
That was the year of the big gold rush to California. The prospects of the farm were poor, and the father thought to better the affairs of his family by joining other adventurers who set out westward so eagerly to find gold. The covered-wagon emigrant train with which he journeyed met with sudden disaster. Cholera broke out, and Ebenezer Comstock was one of the many who died of it. His wife was left to struggle alone with the care of her baby and the farm.

Further misfortunes followed her. The farm was lost on a mortgage. She returned to New York State to be among her own people and found work as a nurse. Her own illness and hospitalization followed, and the boy had to be cared for among several families of relatives until at the age of eleven years he began to make plans for himself.

He chanced to meet Captain Lewis Turner and his good wife, Rebecca, and to win their interest. The captain was a master of schooners that sailed the Great Lakes in the grain and lumber trade. He had four sons who were sailors, already gone from home. The captain offered the boy board and clothing and three months' schooling a year in return for his services about the place, and the offer was accepted gladly.

The captain's wife was an intelligent, kind, motherly woman and an excellent cook. She took the little stranger to her heart; cared for his health, which was none too good; encouraged him to study and to cultivate good habits. She called him "Hanky," and he called her "Ma Becky." Her house became his second real home. He lived there five years, going to school in the short winter sessions and doing all kinds of chores about the place. As long as Ma Becky lived he returned to it at intervals as to a home.

Living among sailor folk, he naturally looked toward sailing as his first remunerative occupation; but he had never been robust and was not strong enough for the heavy work of a sailor on the deck. So Mother Turner, with kind and generous and competent foresight, taught him to cook, and thus equipped him for the place of steward on a sailing vessel. For the next five years he sailed the Great Lakes in summer and attended school in winter. He saved his wages to buy books and to pay tuition in the



Lent by Professor G. W. Herrick

JOHN HENRY COMSTOCK AND HIS WIFE, ANNA BOTSFORD COMSTOCK

private academies that then gave the preparatory work for college, which now the public high schools have largely taken over. On shipboard he always had some book at hand, and he studied it during intervals of work.

It was in reading that he first found opportunity to follow his bent for natural history. He found some books on botany that enabled him to acquaint himself with the flowering plants of the Great Lakes region. Then once, when his ship was at anchor in the port of Buffalo, he went to a bookstore to see if he could find a book that would tell him something about flowerless plants. He found instead another kind of book that opened for him a new door to the house of knowledge. It was *A Treatise on Some of the Insects Injurious to Vegetation,* by Thaddeus William Harris, M.D., Librarian of Harvard University. It was, and is to this day, a beautiful and worth-while book. Its illustrations quickly led him to see that here was something that would interest him beyond anything he had ever read. Its finely colored plates fascinated him; alas, the price ($10.00) seemed entirely beyond his means. He went sadly back to his work without it; but even as he cooked he could not forego the possession of the precious book. He returned next morning and bought it, with money taken from the fund he had been saving for a course in college; and for a time it was his chief possession. That book had a large part in determining his future career. His own well-worn copy is now one of the chief treasures of the Comstock Memorial in the Library of Cornell University. On its flyleaf is written such a tribute as few books can ever claim:

> I purchased this book for $10.00 in Buffalo, New York, on July 2nd, 1870. I think it was the first entomological book I ever saw. Before seeing it I had never given entomology a thought; from the time that I bought it I felt that I should like to make the study of insects my life's work.
>
> J. H. C. Nov. 19th, 1876

At the age of twenty he was ready for

college. He had heard of a new kind of college recently opened at Ithaca, N. Y., where courses of study in science were placed on a par with the classics, and that appealed to him; so he sent for a catalog. From the catalog he learned that the founder had provided means of earning a living while studying, and had said he wished to found an institution where any person might find instruction in any subject; and that all seemed suited to his circumstances and wishes. So he went to Cornell University to study entomology.

When he got to Cornell he found there no course offered in entomology. There was, however, a department of zoology, with Dr. Burt G. Wilder at its head, ready and willing to give him every aid he could to study entomology by himself. Dr. Wilder gave him facilities for study, helped him with books and counsel and encouragement, and after a short tryout of his help at chores about the department, took the youth to be his own personal assistant.

That was a happy association. The eager, industrious, and resourceful boy was exactly the kind of helper that Dr. Wilder needed, and he, the kind of teacher and friend that the boy needed.

Dr. Wilder was a gentleman of the old school, tall, dignified, precise, and exacting almost to a fault, but kindly and generous, and more exacting of himself than he was of others. He was a gifted musician who wrote songs, both words and music, for his own diversion. He was strictly opposed to the use of alcoholic beverages and tobacco; also—alas, for his popularity with the Hurrah Boys of the student body—opposed to fraternities and intercollegiate athletics. He had graduated from the Lawrence Scientific School at Harvard University in that day when the scientific studies that he pursued were kept apart from the classics as representing a lower order of intelligence. During the War between the States he had been, first, a medical cadet, then an assistant surgeon, and then a surgeon in the 55th Massachusetts Infantry. At the time of his appointment to the original faculty at Cornell University he was an assistant in comparative anatomy in the Museum of Comparative Zoology. But he professed no knowledge of entomology beyond the pittance of it to be had in a course in general zoology. That he was not far from being an entomologist himself is shown by what he wrote about himself for publication in the fourth edition of *Who's Who in America:* "In 1863 [he] discovered n[ea]r Charleston, S. C., a large spider (since named Nephila Wilderi by McCook) from which while alive he reeled 150 yards of silk."

He published a number of small papers on spiders; and in the third issue of the *Cornell Register,* a list of donations to the University includes this item: "6. A collection of insects deposited by Professor Wilder."

By the end of his sophomore year Comstock had made such progress with his studies on insects, and such an impression for zeal and competence in all his work, that thirteen of his fellow students petitioned the University faculty to allow him to give them a course of lectures on entomology, with university credit. Dr. Wilder approved the petition and it was granted. The title that Comstock gave his course, "Lectures and Field Work in Entomology," indicated that he meant to make the course practical; that he was not content to have his pupils know insects only as dried specimens stuck on pins. He took his class afield for first-hand knowledge of what insects do; of the roles that they play in the world of life; how they get along together; and how they affect human interests. Many years later he told me that President White had charged him to keep ever in mind the interests of the farmer.

The first course was given during the third term of the following year, his junior year. By that time the second permanent building of the University, McGraw Hall, had been completed, and Comstock was assigned a small room in its square tower as his first entomological laboratory.

The members of that class were Comstock's fellow-undergraduates. Among them were David Starr Jordan, Leland O. Howard, and William Trelease. Jordan wrote of the situation forty years afterward: "I have to remember Comstock as one of the very ablest of my students and one of the most inspiring of my teachers. . . . We stood doubly in the relation of teacher and student. . . . Comstock taught me all that I know and most that I have forgotten on insects, and I taught him the names and habits of the flowers of Western New York. We were boys in those days."

Dr. Wilder taught Comstock academic decorum. He was insistent on gentlemanly conduct, and on the use of good English in written reports. He stressed five characteristics of good scientific writing and called them "The 5 great C's": Correctness, Clearness, Conciseness, Consistency, and Completeness. His oft-reiterated warning to students who were preparing papers for publication was: "Print in haste and repent at leisure."

Comstock's zeal for the study of insects did not kill his interest in other fields. He eagerly attended the University concerts and the lectures by distinguished scholars, whom President White brought to Cornell as members of his "non-resident faculty." These were later called Visiting Professors. Among them were James Russell Lowell, George William Curtis, Bayard Taylor, John Stanton Gould, and others of like prominence in America, and the English historian, Goldwin Smith, who soon became a member of the "resident faculty," and a dear friend of Comstock. Foremost among these distinguished visitors in Comstock's interest was the great Swiss naturalist, Louis Agassiz, with whom Dr. Wilder had been closely associated at Cambridge and at Penikese.

Agassiz gave a course of twelve lectures in 1872 and again in 1873. At Cornell as elsewhere they won instant and enthusiastic interest, and gave an impetus to the first-hand study of nature that has never been wholly lost. His startling advice: "Study nature, not books" (not meant to be taken too literally), was intended to rescue zoology from the bookishness of the schools and to put American students in the way of discovering facts for themselves by the use of their own eyes.

While at Ithaca Agassiz taught by example as well as by precept. He climbed the hills and viewed the smoothly glaciated slopes that drop down to the deep blue lake in the valley. He walked the shores; he waded the streams and searched their shoals for the nests of fishes. Dr. L. O. Howard told me that he once came upon Agassiz, clad in a long black coat and rolled-up trousers, standing in the water in the rocky bed of Cascadilla Creek in downtown Ithaca. He had just lifted a flat stone from the water and held it in his hand. Seeing Howard, he said: "Come here, boy, and let me show you someting eenteresting." It was the roof-stone from over the nest of the little sculpin fish called Miller's Thumb that he held in his hand. Attached to the underside of the stone were the pearly eggs that that little fish in some way manages to hang from the ceiling of its nest.

Dr. Wilder watched Comstock's progress with a fatherly interest; saw that he was going "pretty fast and pretty far" on the basis of very limited experience; saw that he needed contact with the minds of those more experienced and more eminent in his field. And it was doubtless in aid of Comstock's work,

and on Dr. Wilder's recommendation, that Charles Valentine Riley was brought to Cornell for a short course of lectures on entomology.

Riley was then a young man, and already among the best-known of American entomologists. He may have been the only one who was devoting his time wholly to entomology and earning his living by it. There were others, of course, studying insects: several physicians who obtained time for the study by more or less neglecting their medical practice; and a few teachers who were paid for teaching other subjects and managed to slip in a little entomology on the side. But Riley was an out-and-out entomologist. He was widely known for publications that were well-written, beautifully illustrated, and based on accurate and extended observations. His series of nine *Annual Reports* on the insects of Missouri was destined to be epoch-making in the history of economic entomology.

Riley was a handsome man and a ready speaker. He was tall, slender, dark, and had the long flowing mustache of a buccaneer. He had a pleasing approach, a ready pen, and a still more gifted pencil. With his endowment as an artist went also an artistic temperament and a flair for showmanship. Comstock listened and learned; learned much about the way to present the results of his own studies to an audience, from the man who was in his day doing more perhaps than any other to teach the people that the study of insects is worthy of public support.

After this contact with Riley, Comstock was more than ever conscious of his own lack of training and experience. With the first summer vacation following came Comstock's first opportunity to sit at the feet of a great entomologist. Dr. Hermann A. Hagen had just been brought from Germany by Louis Agassiz to take charge of the insect collections in the Museum of Comparative Zoology at Cambridge, Mass. Comstock hastened to Cambridge to study with him.

No happier choice of a master in entomology could have been made. Hagen was then at the height of his powers. He was a great scholar, educated in the classics, well-acquainted with the whole range and history of entomology, and well-grounded in the field of morphology that was at that time coming into marked prominence. He was also entomology's first great bibliographer. However, he had more complete command of several other languages than he had of English. I remember that Comstock told me that when he and Hagen sat down at opposite sides of the little table over which came most of the instruction of that marvelous summer, Hagen began by saying:

"Do you shbeak Cherman?"

"No, sir."

"Do you shbeak French?"

"No, sir. Sorry."

"Do you shbeak Latin?"

"No, sir."

"Vell den," (with a sigh of resignation), "I guess ve vill half to shbeak English. Come now and I vill tell you some tings vat I know aboud entomolochy."

So the work proceeded, with Hagen lecturing and making rapid sketches on loose sheets of paper that lay before him on that little table, and Comstock listening and taking notes.

New realms in the field of entomology were being opened to him, far and away beyond those with which he had already made himself familiar. Between sittings he expanded his notes, and he told me many years afterward that he was still finding these old notes very useful in the preparation of his own lectures. It was with some pride that he said, "I was Hagen's first pupil in America."

As from Wilder he learned methods of comparative anatomy, so now from a distinguished specialist in his own field he learned morphology, and the impor-

tance of collecting and studying the life histories of insects along with their adult stages. From this master bibliographer he also learned much about how to deal with the infinity of detail that the study of insects imposes.

Then Comstock went for instruction to a man of great competence in economic entomology. He went to see Dr. Asa Fitch, the first state entomologist of New York, who was publishing excellent annual reports on the economic insects of the state. Dr. Fitch was a modest, kindly, scholarly gentleman, a country doctor, who, while practicing medicine, had built himself a little museum in his own dooryard. In this little retreat he kept his collections and did his writing. Comstock sat down with him there, and asked him about his methods of work. Fine as Fitch was as a student, investigator, and writer, he was not, like Hagen, born to be a teacher. When Comstock asked him how to study entomology, he said, "You just sit down with an insect and study it."

Comstock also went to Yale, where he found no entomologists, but he was glad to make the acquaintance of the zoologists Verrill and Hyatt. He also took advantage of excursion rates to the Centennial Exposition in Philadelphia. While there he spent most of his time at the Academy of Sciences, where he noted the fine library and large collection of adult insects. He also noted the (at that time, usual) lack of their immature stages.

The following year he received his first regular appointment to the teaching staff of the University as an instructor in entomology, and the course of his own life was set for the future. He began at once to build up a teaching collection of insects and thoroughly to acquaint himself with the entomological resources of the region round about the University.

In the second term of Comstock's senior year, Dr. Wilder was called away to give a course at Bowdoin College. He left Comstock to substitute for him and to give lectures in the regular course in invertebrate zoology to a class of 150 students. During this year one of Cornell's distinguished visiting professors, John Stanton Gould, taking note of Comstock's zeal and industry, presented the Department of Zoology with its first compound microscope, especially for Comstock's use. His best optical equipment up to that time had been a cheap low-power pocket lens. This microscope was substantial aid. It was wonderful progress, when, a little later, he was authorized to go to Rochester and purchase for his laboratory three compound microscopes.

Following Comstock's graduation at Cornell in June 1874, Charles V. Riley, who was then Entomologist of the U. S. Department of Agriculture, asked him to investigate the depredations of the cotton leafworm of the South. He went forthwith to Selma, Ala. This was his first real labor in the field of applied entomology.

That was the summer of a great yellow fever epidemic in the South. He spent it in the study of the life history and habits of this very destructive cotton insect. He saw how vast are the losses that an obscure little moth may cause, and how great the need of public support for the study of such pests.

In the winter of 1876 he made a trip to Florida to collect insects in a new faunal region. He went up the St. Johns River and spent a time at Ft. Reed where he collected diligently. He wrote home: "This is the richest entomological field I have ever worked." And he predicted that many species new to science would be found in the material he was gathering there. New species are still being described occasionally from that material.

In the spring of 1877 after his course

in entomology at Cornell was finished he gave a course of lectures on entomology at Vassar College on invitation of Professor James Orton.

In July 1878, he went again to Selma, Ala., to continue his field studies on the cotton leafworm. In the following September he stopped awhile in Nashville, Tenn., with C. V. Riley while they together prepared a quarterly report. Comstock then returned to his place at Cornell, and sent L. O. Howard, his most advanced and one of his ablest pupils, to be Riley's assistant in Washington.

An overturn occurred in national politics and Riley was dismissed from his position in Washington and Comstock was appointed Entomologist of the U. S. Department of Agriculture in his stead. Comstock got leave of absence from Cornell and went to Washington for two years. During this interval William Stebbins Barnard carried on the course in entomology at the University.

THE WOMAN who assisted in the establishment of the Department of Entomology in Cornell University, and who had a very large share in its success, was Anna Botsford. She was of Quaker ancestry and came from a good farm home in Cattaraugus County, N. Y. On the farm she acquired a lasting enthusiasm for outdoor life. Her father was something of a naturalist, and a good, intelligent, and worthy citizen. Her mother, Susan Allen Botsford, was a woman of refinement; and the two gave their daughter a gentle upbringing and a happy outlook on life.

Anna Botsford was socially gifted; she had the blessed habit of looking for good qualities in everyone she met. She was also a fine student, both of books and of human nature. When she arrived at Cornell in 1875 she was chiefly interested in English and in history; but in order to balance the subject matter of her studies she signed up for a course in invertebrate zoology. Thus young Comstock became her teacher. She found invertebrate zoology both interesting and informing, and she made a good record in the course. She next enrolled in Comstock's much smaller class in field and laboratory entomology. Here Comstock was at his best. Here she caught his enthusiasm for the study of insects. Here began their field trips together—many more of them, it so happened, than were on the class program. Happy days of further exploration followed, when together they climbed the hills and tramped through the gorges about the campus; went canoeing on Cayuga Lake[1] and on the flower-fringed bayous that meander through the Renwick woods at its head. Comstock had the haunts of insects in many interesting places to show her. So she began to share his interests.

The country about Ithaca in that day offered a fine opportunity for field work in natural history. The campus was as yet little more than hilly woodland farm, with the teeming insect life of shady ravines all about its doors. Its nearby lakes and streams, its hills and valleys, its waterfalls and deep gorges, its upland bogs and lowland swamps, each had its share of the rich fauna and flora of the Finger Lakes region. The shy and dainty trailing arbutus could still be found growing in places that later became parts of the main quadrangle.

The Cornell campus, lying as it does, outspread on a terrace of the broad East Hill of Ithaca, is bounded (as is well-known to all) on two sides by swift streams that break into alternating waterfalls, plunge basins, and riffles all the way down the hill; Fall Creek on the north and Cascadilla Creek on the south

[1] A gem from Anna Botsford's diary: "The day was perfect; a soft Spring haze covered the hills, and the lake was so mirror-like that two wild ducks swimming across it made a W that spanned half its width."

side. On the high ground at the northeast corner of the old quadrangle beside the Fall Creek gorge Comstock "staked a claim." He obtained from the University Trustees a long-term lease on a piece of ground there, and built a house on it. It fronted on East Avenue, and from the front there was a superb view of lake and valley and the distant, checkered fields of West Hill. There were tall oak trees growing on the steep slope down to the gorge; trees that reached their long arms so close to the windows of the house that, looking out from them, one seemed to be in the treetops among the birds and the squirrels. And there was the low sound of running water coming up through the trees from a waterfall far down below. A cultivated garden was at the rear, and, an orchard; and farms beyond the orchard. What a wonderful environment for two naturalists! They were married in October 1878.

The remainder of this narrative is a story of teamwork in education.

THE COMSTOCKS began housekeeping in their new home on the campus. Mrs. Comstock records that the first thing she had to do was to learn how to cook, and that her first pies were not a success; but that they were very happy. Soon they were working together across the quadrangle in McGraw Hall on a report of his last season's field work on the cotton leafworm.

Out of a clear sky came a call to Washington. As a result of a political overturn, Riley was out, and Comstock was invited to come to Washington and take his place in the U. S. Department of Agriculture. They were loath to leave their new home; but to a young assistant professor, married, and living on a yearly salary of $1,000, working in a new and little appreciated field of university interest, the call to the top entomological position in a great Department of the national government seemed a golden opportunity. In Washington he would have a budget of $5,000, a salary for himself of $2,000, with two paid assistants: L. O. Howard at $1,200, and Theodore Pergande at $750; and he could employ George Marx as an artist at $5.00 per day. So they went to Washington. They took with them, very properly, the unfinished manuscript on the cotton leafworm, and there expanded it into the well-known *Report on Cotton Insects.*

They found the office of the entomologist lacked a compound microscope. There was a Division of Microscopy then in the Department of Agriculture, and anything too small to be examined with the unaided eye had to be taken over there for study. Comstock bought a microscope. He also spent $200 of government money for a Remington typewriter that is said to have been the first used in the Department of Agriculture.

Before the report of cotton insects was out of the way there came insistent demands from Florida and California for help in control of citrus insects, and Comstock and Howard began a general investigation of them. Soon Comstock became aware that he needed to see the general situation in the field, and he went to Florida for field work in the orange groves. Mrs. Comstock stayed in the office to keep things going. She attended to the correspondence of the office, writing letters first in longhand, and then on the typewriter, after she had learned to use it. She also typed and filed her husband's field notes, and made drawings for his reports. She distributed silkworm eggs to 120 applicants who wanted to assist in an experiment at silk culture in America, and she did whatever else the situation demanded.

Later Comstock made a trip to southern California, with the same objectives and for a longer stay. There he did the field work on which was to be based a

report on scale insects that was destined to be one of the outstanding documents of economic entomology. It was reprinted in 1916 as a Bulletin of the Cornell University Experiment Station.

It is not for me to try to tell the story of the struggles of the office in Washington to keep up with the nation's growing demand for help on insect problems. That story has been committed to writing in a biographical manuscript by Mrs. Comstock, which is still unpublished.

After two years in Washington another political shuffle brought a new head to the U. S. Department of Agriculture. Dr. C. V. Riley was restored to favor and reappointed to his former position. Comstock was out, but an equitable arrangement with him provided for continuing his pay until completion of the reports he had in hand, and for their proper publication thereafter. Dr. Howard stayed on in the office as Riley's assistant; stayed for a distinguished career as Chief after Riley's death.

In the two years at Washington Comstock had made contact with the principal insect problems of the nation at large. He had widened his own horizon by travel. He had acquired some useful, if disappointing, knowledge of how the scientific positions in the government service were peddled about in his day by the politicians. If he felt himself limited by a return to a position in which he could command lesser resources, he expressed no disappointment. Quite the contrary. Just before leaving Washington he wrote to his wife, who had returned to Ithaca earlier:

> We will take up the work at Ithaca with confidence. We will have a happy home. We will give my students the best facilities for obtaining an entomological training that can be found in the world. And we will do some original scientific work. . . . Just now I am at work on a plan by which the students, after the first term, will do original scientific work. In that way I shall have a large corps of assistants and they will get the best of training.

At Ithaca, White Hall had been completed during his absence, and he was given greatly enlarged quarters on the second floor. He now had an office, a lecture room, and a separate laboratory. He had a telephone installed in his office, one of the first on the campus, and a typewriter. He had cabinets built after his own design, to hold insect cases, with enough space in them for large future collections. He inaugurated the system, now in general use, of keeping his pinned insect specimens in glass-topped cases pinned to movable blocks. This provided for easier rearrangement, and the interpolation of new material without the necessity of repinning the specimens.

He returned eagerly to teaching. Besides the large course in invertebrate zoology that he inherited from Doctor Wilder, he had a class of twenty students in entomology. And in the spring he took on a new course in apiculture in which seven students enrolled.

From the beginning he gave his students in entomology all the field work that the strict hour-limitations of the academic program would allow. His pedagogical procedures do not appear to have been standardized as yet, for he made a class exercise out of the cutting of a bee tree in the woods at night, getting out the honey, and luring the bees into a box hive. And (doubtless at his own personal invitation) President White and Professor Goldwin Smith went along as spectators!

He spent part of a summer vacation in bee-tree hunting and in trout fishing. His hunting was not so much for honey as for more knowledge of the ways of the bees in the air, and their homing instincts. With a keen eye he followed single bees from their feeding places, as they wheeled away and swiftly disappeared. Then he followed lines of bees onward in a straight course to their home in a hollow tree.

After finishing his report on the Coc-

cidae (scale insects), along with which went Dr. Howard's report on their parasites, he began the preparation of a textbook for the use of his classes. He built it from the ground up, basing it on materials that his students could see and handle. He gave his keys for insect determination the severe test of repeated use by beginning students, and he revised them again and again, always striving for simplicity and clearness of statement.

His aim was, and continued to be until the end of his teaching, to have a basic course in general entomology, from which later specialization might proceed. After mastering the fundamentals in this course his students could specialize to their heart's content. He began to accumulate an entomological library with the same regard for essentials.

Then he got an insectary, the first of its kind. I recall that he once told me with great glee how he got it. For several years he had been hinting to the administration that he needed a building in which he could keep living insects under observation. Professor I. P. Roberts, then head of the Department of Agriculture at Cornell (not yet a college), called him into the office one day and said: "There is an unexpended balance of $2,500 in my fund. It will lapse if not used before the end of the fiscal year. That is six weeks from now. If you can get your bug house up and get the bills paid in six weeks you can use that money."

Comstock leapt to the occasion. He drew the plans for the building that night, but not, of course, without much previous forethought; staked out the ground for it next morning, and got the work of excavation going next day. The building started a few days later. It was completed almost on time, and by private arrangement with reliable workmen, he had the bills all in and paid by the end of the fiscal year—a plain case in which the law was "more honored in the breach than in the observance."

I am amazed when I think what he got for the money: a two-storied head house with a two-section standard greenhouse in the rear; in the head house, four large rooms, two on each side of a central stairway, and steam furnace and toilet in the basement; in the greenhouse, hot and cold sections, steam, water, and gas!

One room on the second floor was assigned to a student who, in return for its use, took care of the building. A long succession of budding entomologists came to occupy that room, one of whom (Stocking) years later became acting dean of the College of Agriculture.

The insectary was located well back of Comstock's house between the woods and the garden, where its lack of ornamentation did not matter. It was built for service, not for appearance. It sheltered some of the most important work done in entomology during two generations.

Comstock was now equipped to take advantage of the rising tide of interest in entomology, so he pushed ahead. But his alloted space in White Hall was becoming inadequate. It was crowded with students and accumulated teaching aids. The equipment for photography that he had to have near at hand for use in his own research and in that of advanced students he squeezed in by applying his wits to economizing space. For darkroom use he adapted a small closet, equipped it with lights, running water, and shelves high and low, on which everything needed was in reach from the middle of the floor. He bought a bulky camera with rectilinear lens.

The operating table was a contrivance of his own that every student of its day will remember. It was wide enough only to carry the big camera and long enough for full extension of its very long bellows. It was high enough for Comstock's own convenience when standing erect

while focusing; taller persons had to stoop. On it sat the camera perpetually, under a shroudlike sheet of black cotton-flannel, that served as a focusing cloth while in use and as a dust cover between times. Underneath was a built-in storage compartment in which accessories were kept: plateholders and kits for glass plates of various sizes (it never knew films). It stood in a corner by the closet against the wall. It was rolled out on its own casters for use when no class was occupying the room. It saw great service.

Meanwhile Comstock's publications were winning for him high professional standing. His *Report on Cotton Insects* brought letters of congratulation from entomological colleagues and others. Especially appreciated by him were two of the letters commending it, one from his master in entomology, Dr. Hagen, and one from Charles Darwin. When his report on the Coccidae appeared it became at once the standard reference work for students of the scale insects the world around.

He was invited to join the editorial staff of the *American Naturalist,* and for three years he edited a section on entomology in that journal. He also wrote articles on insects for the *Standard Natural History.* He answered queries about insects for several agricultural journals; and he appeared more or less regularly for an address before the annual meetings of the New York Horticultural Society.

(*To be concluded*)

AN URGENT MATTER

After almost one hundred years of existence as the over-all scientific association of this country, the A.A.A.S. finds itself choked by its own growth (see pages 127–130). The officers of the Association do not have time to give the attention to the campaign for a building fund that it deserves. We hoped that our members and friends would recognize our plight without repeated reminders and take action on the merits of the case as presented in our prospectus. Many have done so, but many more have not yet contributed. May we suggest to those who have contributed that they do what they can to persuade others to contribute and that they find out whether their own affiliated societies could not contribute from their permanent funds as the American Association of Economic Entomologists did at its December meeting in Dallas, Texas? Members who are employed by commercial laboratories should also see the officers of their companies and point out that their Association is in danger of bogging down.

To those who have contributed to the centennial building fund we are extremely grateful, but we hope that they will not be satisfied until they have used every means of persuasion on every prospective contributor whom they can approach.—F.L.C.

CASTE, CLASS, AND COLOR IN INDIA

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CASTE, as a social institution, is not peculiar to India. Throughout the ages all societies have divided their people into various groups on grounds of particular traits and talents, equipment and opportunity, character and culture. And when such groups pursued their particular ideals and professions to the entire neglect of other groups' interests, they tended to grow up as isolated, individualistic, and even mutually antagonistic units in the same society. Nevertheless, only in India did such broad divisions of mankind crystallize into a code and an ethic which govern and dominate every detail of the workaday life of particular groups.

Once these divisions, based on individual and group tastes, traits, and talents, were effected, occupational and professional differences strengthened them even more, for the fact that one kind of occupation was more lucrative than another led to economic differences which became yet another prop of the caste system. And, human nature being what it is, it became common for the son to follow his father's profession. Consequently occupational differences became hereditary. Thus birth and occupation became the twin basic principles on which the caste system in Hindu society rested and revolved.

In the course of some centuries an elaborate code of conduct affecting meals, migration, marriage, and morals came to be drawn up. Later centuries added further ramifications pertaining to ceremonial purity and pollution, commensal restrictions, connubial taboos, unapproachability, and even untouchability. To the original basic principles of birth and occupation were added heredity and endogamy, and the subsequent development of caste has rested on all these four attributes. The result is that today India's caste system has become a complex and irrational system, defying all classification.

This does not mean, however, that the caste system is a rigid, static institution persisting in a society where the need for it has vanished. On the contrary, it has been dynamic, but only to the detriment of India's national growth. Whereas the caste system, at its inception, recognized only four or possibly five divisions, based on the essential fourfold or fivefold functions in a primitive rural economy, today the system has divided and subdivided itself to such a profuse and alarming extent that about 3,000 castes have been enumerated in modern India; and new castes are in the process of formation even today! Thus the popular supposition that caste is immutable is erroneous. On the contrary, it is and has always been susceptible to change at the slightest pretext of altered social conditions. This change has been highly unhealthy, for it has led to endless segmentation of Indian society.

Several factors have contributed to this interminable fission, such as change in occupation (usually from the lower to the higher), adoption of a new or the abandonment of an old religious principle or social custom, or merely increased prosperity leading to further gradations of economic equality. Despite these qualifications, caste in theory must be explained largely in terms of: (a) an unchangeable "social inequality" based on birth; (b) the gradations of the professions and their inequality; and (c) the restrictions on marriage outside one's own group or caste. Change in occupation, however, is the primary fac-

tor that creates new castes in rural India, even today.

Keeping all these attributes in view, we may define caste in India as an endogamous group or collection of endogamous groups, generally bearing a common name, membership of which is hereditary, imposing on its members certain restrictions in the matter of social intercourse; either following a common traditional occupation or claiming a common origin; and generally regarded as forming a homogenous community.

The Origin of Caste. Several theories have been put forward to explain the origin of caste. The one hypothesis on which there seems to be most agreement among sociologists explains the genesis of caste as an outcome of the clash of cultures: In the dim past, at a time which may roughly be called the pre-Vedic period, a tall, fair-skinned people inhabited central Asia. While trekking in search of food and a new habitat, they entered India through the northwest frontier passes. These incoming "Aryans" had no caste system as such, but they recognized three broad functional divisions in their own society: the priests, who offered prayers and performed sacrifices; the chieftains and soldiers, who fought in battle; and the rest, who, although normally nomadic, sometimes plowed the fields and raised food. Soon these newcomers began to move into the interior of India, eastward toward the Gangetic Valley, where several Hindu kingdoms were then flourishing. Increasing in numbers and constantly moving in search of shelter and sustenance, these new arrivals inevitably clashed with the original inhabitants. And in the eyes of these newcomers the indigenous peoples seemed uncivilized. Wars were waged and the newcomers gained what should roughly be called "political control" in parts of India. Then, when these two groups of people, culturally alien, settled down to live side by side, marriages, both approved and unapproved took place. Each culture-group tried in vain to maintain its so-called racial purity. The ultimate result of this anxiety to safeguard the conflicting group cultures was the caste system.

On account of certain stray references in the writings of the period to the "fair-skinned" incoming Aryans and the "dark-skinned" native inhabitants of India, some scholars have jumped to the conclusion that caste was originally based on *Varna Bheda,* or the difference in color. But neither cultural antagonism nor color alone can explain the origin of caste, because four castes based on vocational differences soon came into existence.

Another hypothesis was advanced by Sir Edward Blunt, who implies that the caste system was imported into India by the invading Aryans. Moreover, he also emphasizes the color bar, which, he believes, created an endogamous tendency that developed into a rigid custom. It is his theory that the Aryans were already socially organized into three classes: the *Kshatriya* nobility; the *Brahmin* priesthood; and the *Vaisya* commonalty. The intermarriage that did occur with the indigenous race produced a fourth class, *Sudra.*

This hypothesis is untenable, for it does not explain how the Aryans themselves came to have three castes before they came to India, and why, if the first three castes were Aryans, these groups do not intermarry or even interdine. Nor does this analysis explain the fact that there is no mention of caste in Vedic hymns. According to Professor Max Müller:

> There is no authority whatever in the hymns of the Veda for the complicated system of castes. There is no law to prohibit the different classes of the people from living together, from eating and drinking together; no law to prohibit the marriage of people belonging to different castes; no law to brand the offspring

of such marriages with an indelible stigma. There is no law to sanction the blasphemous pretensions of a priesthood to divine honors or the degradation of any human being to a state below the animal.

Most Indian scholars agree with the theory of the French scholar Senart. After a comparative study of Aryan institutions in ancient Greece, Rome, and India, Senart concludes that caste was the Aryan answer to the problem of culture contact. He believes that when the Aryans penetrated into northern India they met with a "race" alien to their own culture and character; and the consequent anxiety of the Aryans to preserve their "racial purity" in addition to the distinction between conquerors and conquered led to the demarcation of their own people from all other people.

Were this the true explanation, there would be only two castes, the rulers and the ruled. Although the fact that "higher castes" form only a small minority of India's population might seem to support this view, how can we explain the absence of castes among later invaders from the Moslems down to the British? Furthermore, this view also ignores how a small minority—the *Brahmins*—who were not rulers, achieved social superiority in northern India. According to Senart, the conqueror or ruler should constitute the highest caste, but in the existing caste system it is the *Brahmin* priest who is above the ruler. Moreover, Senart's theory does not explain the all-important fivefold divisions of society based on occupational differences.

All these surmises about the origin of the caste system being found in an inherent Aryan-Dravidian conflict are not only highly unreal but are sharply in contrast with our modern anthropological evidence. As Professor S. Radhakrishnan points out:

The system of Caste is in reality neither Aryan nor Dravidian but was introduced to meet the needs of the times when the different racial types had to live together in amity. The only way of conserving the culture of a race which ran the great risk of being absorbed by the superstitions of the large numbers of native inhabitants was to pin down rigidly by iron bonds the existing differences of culture and race. Unfortunately this device to prevent social organization from decay and death ultimately prevented it from growing.

Although Professor Radhakrishnan appears to come closer to the truth as we see it, it is not clear what he means by "race." Are we to presume that some five "races" came into India and were rechristened as five castes; or was caste merely the ancient Hindus' passion for labeling professional and ethnic groups of people?

Although there is no denial of the grains of truth contained in the various hypotheses, neither cultural antagonism nor "color" alone can explain the origin of caste, and we must seek elsewhere for a more logical explanation for the origin of the Hindu caste system—one that will explain the divisions into four groups based on professional or vocational dissimilarities. Perhaps, with the development of group consciousness in India and the consequent desire to end the social anarchy and confusion of a small primitive society, some self-chosen group leader deliberately planned a division of labor. The need for worship and the love of colorful ritual would have created the necessity for a class of clever men who would officiate as priests and propitiate the gods. The need for an army or police would have created the second class. The need for exchange and barter would create the third; the need for raising food, the fourth. Originally there would have been no question of superiority or inferiority. Even the *Panchamas,* the fifth class, composed of those who had no tastes and talents for anything but menial labor and so were assigned to do the cleaning and scrubbing, sweeping and washing, would not have been looked down upon. Thus *at the*

beginning everyone chose his vocation on the basis of ability, inclination, and aptitude.

In the course of centuries, these occupational groups lost their elasticity and mobility and tended to become inflexible. The son of the *Brahmin* priest became a priest, and the son of a *Panchama* sweeper became a sweeper; the hallmark of caste became birth and heredity. And now the priest, who took two baths every day in the nearby river to be ceremonially clean before he stepped into the sanctum sanctorum of the Hindu temple, did not think much of the *Panchama* sweeper who was externally unclean. Human nature being what it is, marriage among members of the same professional group became common. And we can understand why sometimes the daughter of the *Brahmin* priest refused to marry the son of the *Panchama* sweeper, and vice versa. It was thus that endogamy became an additional principle of caste. This, to us, is roughly the genesis and growth of caste.

The conversion of this originally elastic system, after a long stretch of time, into a rigid order based on birth and heredity, with commensal and connubial restrictions, does not seem so absurd even in the light of modern conditions. This is not a defense, for there can be no rational defense of caste, but a description of these restrictions. In other countries there are parallel restrictions, less rigidly defined perhaps, but existent nonetheless. Let us look at the West through the eyes of George Bernard Shaw:

> Suppose a middle class British mother said to her daughter, Miss Smith or Miss Jones, "Follow the promptings of your heart my dear, and marry the dustman or marry the duke, whichever you prefer." But she cannot marry the dustman; and the duke cannot marry her, because they and their relatives have not the same manners and habits; and people with different manners and habits cannot live together. And it is difference of income that makes a difference of manners and habits. Miss Smith and Miss Jones have finally to make up their minds to like what they can get, because they can very seldom get what they like; and it is safe to say that in the great majority of marriages at present, nature has very little part compared to circumstances.

Exceptions to this general tendency of social immobility in marriages are not wanting in modern democratic countries, but they are not frequent. We do not find every day a king marrying a commoner or a duchess marrying her chauffeur.

As for Hindu commensal restrictions, Western parallels are not wanting either. It is true that a *Brahmin* priest does not break bread with a *Panchama* sweeper, nor does a *Sudra* peasant pour a cup of tea for a *Vaisya* trader. This is nothing more than the Western social custom—or snobbery, if you please—that forbids the parlormaid to sit down in your drawing room or prevents your asking the gardener to dinner. True, these modern analogies do not go on all fours. Citing these Western parallels to explain caste may seem as if we were confusing the Western class system with the Hindu caste system. We are considering caste in a fossilized state, centuries after its meaningful inception. But if the system originally was nothing more than a class system or a division of the people according to their primary occupations, how can we account for the hundreds of castes which exist today? The answer of the Indian scholar S. S. Nehru is worth quoting:

> To understand the rationale and raison d'être of caste and its place and function in rural economy, let us make the following hypothesis: If there had been no industrial revolution in England; if the struggle for existence in that island, which is geographically a unit but economically the reverse, had been less keen than it is; if the passive acceptance of life on any terms had smothered the inventive genius of the islander; if the brotherhood in arts and crafts had continued to guard their secrets jealously; if their principle had been accepted and expanded to include all rural professions and social services; if the guilds and similar associa-

tions had been armed with dire sanctions in the way of forbidding vis-à-vis others such as intercourse, interdining, intermarriage, wherewith to keep their membership economically, professionally, socially entire; if the watertight compartments created and fostered in that way had continued to accumulate traditions like a dead weight to retard their otherwise healthy growth; if the cleavages of such bodies became more and more acute and the linkages correspondingly less and less firm, in course of time—

Why, then England too would be having a caste system quite as strict and complicated as any in India today. The artisans and craftsmen, the butcher and botcher, barker and baker; the carrier and cartwright and cowherd and cutter; the dyer and dustman and dairyman; the farrier and fowler and furrier and fuller; the grocer and goatherd and gardener; the harper and hawker and hatter; the joiner and jeweller and Jewish money-lender; the kitchener and knocker; the knacker and launderer; the minstrel and mason; the oilman and oxherd; the pursuer and poacher and pastry cook; the quarryman and collier; the squire and scrivener, salter and saddler, sweep and stonecutter; the tailor and tinker; the vintner and vagrant; the woodman and watchmaker; the yoeman and yokel—briefly told, all these categories from A to Z of human activity in the social economy would be developed into castes, each living and working in his own watertight compartment, hermetically sealed and cut off from the rest, and then the English castes would be quite as numerous as the Indian castes in any given rural area.

Such in brief is the genesis and growth of caste as a social institution in India—the child of the Indian or Hindu primitive efforts to run "society" on an orderly and systematized basis—if we can stretch our sociological imagination to the dim beginnings when "society" itself was being born in India.

Evolution of Caste and Class in Modern India. Caste in modern India is an example of social institutions that have developed almost to be the opposite of what they were intended to be. There is a tremendous gulf between what caste was planned to be and what it actually has grown to be. There is no doubt that caste in Hindu society was created to meet a definite need, although sociologists may disagree on what the precise need was. As originally conceived and practiced, it had no class consciousness and economic implications. It never pursued values in terms of rupees and annas. It recognized no snobbery of worldly power or prestige, for the first caste was composed of priests who were never wealthy. Nor was the vocational division strict and rigid originally, for *Brahmins* have been warriors and doctors from earliest times. The Maurya emperors were *Sudras,* and several princes in India today trace their genealogical line to *Sudra* ancestors. In fact, we read in the *Puranas* stories of individuals and of families who changed from lower to higher castes and vice versa. Manu, the Hindu lawgiver, admits the possibility of ascent and descent. The *Mahabharata,* one of the great Hindu epics, points out:

> There has been so much mixture in marriages that no test of *jati* (or caste) or birth is good. The governing consideration should be *sila* (or character) and the first Manu has declared that there is no point in distinctions of caste if character is not considered.

According to the great Hindu scripture *Bhagavad Gita* (The Song of the Lord), "The fourfold division of caste was created by me (the Creator) according to the apportionment of qualities and duties. . . . Not birth, not sacrament, not learning makes one high caste but righteous conduct alone causes it."

The creation of caste, as we have seen, had nothing to do with the Hindu religion. Once the system came to stay, however, the Hindu religionists seized it, rationalized it, and gave it a metaphysical and theological color, which to begin with did not distort the real purpose of caste. Despite all the religious injunctions that behavior and not birth and belief should be the hallmark of caste, the lapse of centuries blinded Hindu society to the true nature and significance of caste. It became stratified and

rigid and fell a victim to the abuses against which it had been warned. Moreover, a philosophy was created and a rationale was evolved by the group consciousness of the first caste to support the myth that caste was divinely ordained. They spread the insidious dogma that the status of every individual in this life and the caste in which one was born were determined by *Karma,* or one's actions in previous birth. With interpolations such as these, the abused caste system was foisted upon an otherwise innocent Hinduism. Hinduism cannot be blamed for this social fragmentation any more than Christianity can be blamed for the present international anarchy. The masses were misled, and the gullible millions resigned themselves to a system that fettered them to myriad disabilities. And as wave after wave of foreign invasions reduced India to a life of constant peril and insecurity, these fissures in Hindu society became deeper and resulted in the fragmented shambles of the meaningless social structure that caste is today.

Caste and Color. Some European and American scholars have tried to explain the Hindu caste system as the outcome of the contact and conflict between the alien *light-skinned* Indo-Aryans and the Dravidians, the original and older inhabitants of India. This might have been expected from Nazi Indologists who can explain institutions they do not comprehend only in the light of race problems based on color. Is there any basis for this comfortable myth of certain Western scholars whose preoccupation in this regard seems to be the pigment of the skin? Since the *Vedas* mention *Arya Varna* and *Dasa Varna,* and the four supposed high castes as *Chatur Varna,* and since *Varna* means, among other things, color, these observers have readily jumped to the conclusion that the caste Hindu was white and the outcaste Hindu was black. Were we to accept this, the logical absurdity must follow that the second caste must be red and the third yellow and so on. These scholars would like to think naturally of white *Brahmins,* red *Kshatriyas,* yellow *Vaisyas,* brown *Sudras,* and black *Panchamas.* But there was never a "race" problem in India in the scientific anthropological sense. The castes in India, therefore, do not represent 4,000 or 5,000 different biotypes with as many shades of color beginning with black and ending with almost blond. *Caste is not physical and is not and was never based on color.*

The problem of the "untouchables" has often erroneously been treated as a separate problem by several writers. Caste and untouchability are not two different social institutions but are merely the two sides of the same counterfeit coin. They are indivisible, for when we have caste we naturally come to have the outcaste too. The outcaste is the logical corollary of the caste and as such must be approached as an integral part of the caste system. The only and the best way to abolish untouchability is to abolish caste. We cannot destroy one phase of a sickly social institution and retain the whole. Gandhi once said that if Hinduism is to live, untouchability must die. And if the caste system lives untouchability cannot die. A quick and effective method for its abolition should and must be found.

Caste today. Caste has become something new. It is neither what it was intended to be nor even what it was a few decades ago. In theory the system and its restrictions do exist, but their sting has gone. Countless new, nation-building forces, both from within and from without, have set in. The resulting disintegration has reduced caste in several regions of India to a ghost of its

former self. Most vital of these forces have been Western education, modern democratic conceptions of equality, rapid means of transportation, increasing urbanization, and gradual industrialization of the country. The influence of Western education has led intelligent people to wonder about the need for caste today. Happily, many of these progressive forces attacking caste have come from within. All social reformers who have attacked caste, from Raja Rammohun Roy to Mahatma Gandhi, have belonged to the Hindu fold.

Roads, buses, railways and steamships, airplanes, telephones, the telegraph, and radios have begun to reduce India's geographical immensity to a tiny unit. Interprovincial migration, rural exodus, the growth of cities, emigration and foreign travel—all these are breaking down the barriers of caste. Industrialization is upsetting the traditional pattern of the occupational distribution of India's population.

To take up only one caste as an example, the Brahmins, the supposed *Herrenvolk* of Hindu society, can be found today in almost all occupations. Their ancient monopoly of priestcraft and piety has vanished. They follow liberal professions, such as law, medicine, teaching, and Government service — from clerks to ministers of the cabinet. Some Brahmins are landowners, or *mirasdars.* Some have become peasants. There are Brahmin nonpolitical convicts, and there is actually an all-Brahmin criminal tribe, like the *Tagris,* on the upper Jumna.

The same is true, for instance, with *Sudras,* the peasants who belong to the supposedly high-caste Hindu society. Today they are found not only in their traditional occupation of farming but in practically every other profession as well. Among them are doctors, lawyers, professors, presidents, clerks, civil servants, businessmen, bankers, cabinet ministers, Sanskrit scholars, and even priests.

The influence of caste on occupational distribution has definitely vanished. The inevitable pressure of modern economic insecurity has forced the people to forget *Manu's* injunctions and earn their living as best they can. No longer can it be strictly said that caste interferes with the economic life of the people. The driving force behind present-day occupational distribution is ability, opportunity, and qualifications, not socioreligious stratified gradations of a bygone era. Today we find priests and professors, doctors and lawyers, cooks and maids, beggars and vagrants among all castes. The caste system has become metamorphosed into a class system—the haves and the have-nots.

Yet in parts of rural India, in meals and marriages, caste restrictions do continue even today. Intercaste dining and intercaste marriages are increasing, but they have not become the order of the day. Nevertheless, the people are realizing that their caste prejudices are undemocratic and hostile to a healthy nationhood. Many a blow at the caste system has been struck, but as yet none has proved fatal. But caste must and will go. And the hastening of its end is the endeavor of young and progressive India.

OUR INDIGENOUS SHANGRI-LA

By WARD SHEPARD

In this century of revolution, the world will be swept by social changes as vast and far-reaching as the technological revolution of the past century. Fundamentally, the emerging social revolution—of which two world wars and a world depression were symptoms—revolves about the nature of man and of human society. Peoples are stirring to achieve a freedom, a dignity, and a way of life worthy of their status as human beings. Can these changes be scientifically guided as were the vast technological changes of the past century, or must they be left to the emotional clash of partisan politics and of pseudoscientific ideologies? Can we achieve a science of society, can we learn to diagnose the spiritual maladies of modern man, can we build life-giving institutions which will release the greatness that is potential in all men, can we define valid goals of collective social endeavor that correspond to the actual spiritual nature and needs of man? Or must social reconstruction continue to be based on rules of thumb, hunches, guesses, and pseudosciences?

The answer hinges on whether or not it is possible to create a social science which, in the unique realm of human life, achieves the essential practical function of all science—namely, prediction. It is the predictive power of science that gives men some prevision and some control over their destiny. Measured by this test, the social sciences have been highly deficient. Eddington has said that a physicist cannot but look with dismay on the disorganization of the sciences pertaining to human life. This disorganization grows out of the failure of the social sciences to achieve qualitative methods of assessing the values of human institutions. Historically, under the impact of the "natural law" doctrines of the Enlightenment, the great quest of the social sciences was for a system of "natural laws," modeled on the classic Newtonian concept, externally governing the origin and development of human societies. In its heyday, Comte and Spencer were the philosophical interpreters of this quest and, in its decline, Spengler. With the recession of the rainbow of "natural law," the social sciences turned to the useful, but not very illuminating, task of description, and at the same time cultivated a pose of moral neutrality toward the social phenomena they investigated. If, however, the social sciences are to yield the qualities of prediction, evaluation, and control, they must expand their horizons and create new scientific methods.

The social sciences are confronted with a unique subject matter. In essence, it is the problem of human values. Human societies are what men make of them. Theoretically and historically, their diversity, good and bad, is limitless. What is a good society and a bad society, a good institution and a bad institution? If the social sciences cannot evaluate social institutions, then in this age of unprecedented whirling change the fateful decisions of world social reconstruction must be left at best to benevolent amateurs and at worst to dangerous ideological demagogues.

In this day of Armageddon, the last place in the world one might expect to find the clue to a science of society would be in a study of the ancient Hopi Indian villages, perched high on their sun-drenched crags in northern Arizona. Yet a fascinating little book, *The Hopi Way,* published by the University of

Chicago Press, and written by Dr. Laura Thompson, anthropologist, and Dr. Alice Joseph, psychiatrist, boldly and successfully attacks this central human theme: What kind of values does a given human society foster, how does it foster them, and what human personalities emerge?

In this article I have set forth the main conclusions of the authors of *The Hopi Way* and have accepted them at face value. But the broader interpretation of the significance of the new methods, as well as the conclusions on the philosophical implications of the Hopi way of life for modern society, are my own.

Historically, the endless struggle for human freedom has been the instinctive quest of multitudes of men for personal fulfillment. The kind of people a society produces is the only fixed bench mark against which the validity of a society and its institutions and values can be judged. Neither historical perspective nor the classical "natural law" myth affords any such fixed point of reference. The judgment of social institutions, and more particularly the projection of valid new institutions, against this bench mark will be the future field of the social sciences. The social sciences cannot continue to take refuge in the neutral pose, the amoral attitude, of physics. For all human purposes, the cosmos is fatefully teleological and the ultimate task of a science of society is to evaluate human values.

The Hopi Way pioneers boldly on this new path. In essence, it disentangles the reciprocal flow of forces to and fro between the totality of a society and the personalities of its constituent members, in order to weigh and value the kind of people that society produces. Scientifically, it is of secondary importance that it happens to deal with the Hopi Indian community of northern Arizona, as part of a larger study of several changing Indian tribes.

I have had the privilege of reading in manuscript the second volume of this series, on the Navajo people, by Dr. Clyde Kluckhohn and Dr. Dorothea Leighton. This extraordinarily vivid, detailed portrait of Navajo society and Navajo personality structure—both at almost opposite poles from the Hopi—strongly verifies my main conclusion from the Hopi study: that in the Indian personality project a new and powerful tool of social and personal analysis and evaluation is being forged. There is no intrinsic reason why the striking methodology applied to this small primitive group cannot be developed for great modern societies. Indeed, I should like to suggest that it be applied to a thoroughgoing analysis of the historic and social origins of the fatal psychic breakdown of the German people, in order to afford a scientific foundation for the Allied occupation and administration and for the long-range moral regeneration of the German people.

There is, too, in this little book the fascination of discovery beyond scientific pioneering. For the Hopi community, subjected to an intense and manifold scientific scrutiny, turns out surprisingly to be an "ideal republic," a pure, achieved democracy, intensely nurturing an ancient spiritual culture, intensely nurturing and socializing its young. And furthermore the unusual wisdom and the beauty of the Hopi way of life contain a healing message to minds drenched in the terror and pity of world tragedy, oppressed by the specter of vast and unpredictable change. Even more, the Hopis, having long since mastered the fine art of cultivating the garden of human life, have much to tell us about the essential eternal values required for the sustenance of the human spirit.

In their forbidding and picturesque desert, the Hopis have lived for centuries, and continue to live, largely iso-

lated from the great world. Living in several compact villages, with a total population of about 3,500 souls, they afford ideal conditions for a study of almost laboratory precision. Their culture is self-contained, complete, and remarkably stable. It is colorful, rich, and spiritually powerful. It is highly complex, but its complexity is offset by the fact that its categories of order are precisely organized and articulated into a singularly balanced whole. Finally, it displays virtually none of the pathological symptoms that have disintegrated modern civilization. It is a society devoted to intensity of living and to a sophisticated faith in man's importance in the cosmos.

The Hopi culture has developed and survived for over a thousand years against a background of drastically severe and precarious economic conditions. Food and water must be wrested with infinite skill from a semiarid land, subject to severe droughts, crop failures, floods, and, in recent times, soil erosion. Famine, through the centuries, has been an ever-present threat, and certainly often a grim reality. Yet the Hopis met and mastered the challenge of the desert both on the physical and on the social and the spiritual level. On the physical side, the desert forced them to achieve a remarkably effective technology of dry farming. On the social level, it forced a democratic, cooperative social structure which tolerated no waste of human energy and no individual self-seeking, and yet achieved a high degree of human freedom and individual development. And on the spiritual side, this starkly demanding earth-environment, which permits no negligence and no mistakes, yet yields reluctantly to precision, insight, and cooperation, may well be the ultimate source of the central philosophical theme of the Hopi religion and world-view. The entire realm of nature, say the Hopis, in its farthest cosmic sweep and through human society which is a part of nature, constitutes a system of reciprocal and mutually supporting relations, in whose working out the decisions, actions, and even the thoughts of men play an important part. Thus, the "primitive" Hopis anticipated by several centuries some of the main philosophical doctrines of James and Whitehead. Verily, among the Hopis, as among the ancient Hebrews, the stark clarity of the desert is more provocative of cosmic insight than the steaming jungle of industrial civilization.

The most striking thing about the Hopi culture is the almost complete absence of political government—a void that is filled by an extraordinarily complex, integrated, and autonomous system of social and personality controls. There is, to be sure, a modern Tribal Council fostered by the Indian Service, but it has as yet assumed little or no tribal leadership. And there are village chiefs and priests, whose main functions, however, are concerned with the traditional religious ceremonies. There is no lawmaking body: the law is custom, and custom is the law. Looking at the world trend toward centralized bureaucratic government, one would expect in the almost literal "anarchy" of Hopiland a perpetual reign of chaos. On the contrary, quite the opposite is true. The principle of order is implicit in the organic structure of the total Hopi culture. This culture is based on the insight that freedom and order are indivisible and is devoted to the unfolding of personalities that know how to utilize, to fit into, and to sustain the subtly and intricately woven web of Hopi life. From birth to death the Hopi Indian is enmeshed in a complex system of interpersonal and intergroup relations, explicitly defined and fostered, yet yielding diversified, powerful, and stable personalities.

This burden of social ordering is mainly carried by two institutions—the

familial clans, which in turn are grouped together into larger systems or phratries, and the secret societies, mostly for the men and boys. The functions of these two institutions are diverse, though complementary. The clan is a widely ramified system of mutual and clearly defined interpersonal obligations and rights, drawing people together for mutual support through the powerful biological pull of kinship. It is within the family and the clan that children are rigorously trained from birth in the primal reciprocal human duties, rights, and skills—a habitual, conscious, defined ethical conditioning that leaves nothing to chance, yet ultimately relies on individual decision. The secret societies, on the other hand, initiate the successive generations of boys and young men into the higher ethical and intellectual preoccupations of the tribe. They are, among other things, the custodians and carriers of the ancient, elaborately beautiful, ritualistic ceremonies. These ceremonies embody the high esthetic, religious, and philosophical values of the tribe. Year after year, generation after generation, the Hopis devote an intense energy and a vast amount of time to the professional mastery and performance of these psychic dramas. Roughly speaking, the secret societies and the ceremonies are the institutions of higher education as contrasted with what might be called the primary education centered in the family and clan. And, by cutting across clanship lines, they counteract any divisive tendency among the clans.

Within this highly structured social framework the individual leads from birth a life that is oriented in minute detail toward a certain goal. That goal is the evocation of the individual's power and devotion for the benefit of the whole group and the conscious diffusion of his loyalties among the greatest number of people. At the heart of Hopi culture, therefore, is a thoroughly instrumented ethical principle which is almost diametrically opposite to the egocentricity deliberately cultivated by our acquisitive Western civilization. What kind of personality emerges from such a culture so oriented?

The method used by the authors in their study was to bring the various disciplines of anthropology, history, sociology, natural ecology, psychology, and psychiatry to bear simultaneously on determining how the total cultural complex, the nature-culture continuum, operates in shaping the individual personality. The result is a dynamic, integrated picture of the total Hopi culture from the economy to the total world-view. Treating the culture not as a static structure but as a dynamically balanced system of forces, the study then proceeds to its main quest: what kind of human being emerges from this particular system, and why?

A wide range of devices was used to get at the underlying personality structure of the Hopi people. There are detailed psychiatric case studies of many Hopi children, and a variety of psychological tests applied to large groups, up to the age of 18, designed to reveal intelligence, emotional responsiveness, moral ideology, and a detailed picture of the basic personalities of the children tested. The results of these studies are then correlated with, and interpreted from, the characteristic operating forces of the whole culture.

The individual case studies reveal two highly significant things. One is the wide diversity of personality among Hopi children—diversity of emotion, intellect, imagination, behavior. Indirectly, these results throw some light on the much-mooted question of the relative influence of environment and native endowment in shaping personality. Compared with ours, Hopi society is highly regimented; yet the stubborn inborn qualities of the unique individual per-

sist and come to fruition. A second finding is that personality maladjustments and warps arise in Hopi culture—and presumably in any culture—from the same types of environmental irregularities, from which we may infer that the primal demands of human nature are biologically embedded in the mind-body complex and are inescapable in any type of culture.

The various testing devices bring forth vividly the qualities of mind, imagination, and character of the Hopis, and above all the values by and for which they live. The intelligence of Hopi children up to 18 (the highest age level of the study) was found to be markedly higher than that of white children, as measured by identical tests. They especially excel in complex tests of the maze and puzzle variety. They reveal a capacity to weigh the significant details of complex situations and to deal with such situations as organized wholes. They have, in short, a highly developed capacity for "multidimensional" thinking. Unless one adopts the very improbable hypothesis of an innate intellectual superiority, one is forced, with the authors, to believe that the Hopi Indians have developed outstanding and whole minds because the entire fabric of their lives—from the precise fitting together of their rigorous agricultural techniques, through the intricately functioning clan and ceremonial structures, up to their cosmic philosophy of a reciprocally interacting universe—deals incessantly with complex but balanced organic systems.

The Hopi system of moral values is markedly different from our own. Personal ambition for the sake of the self, for prestige, is virtually unknown, and personal failure or inadequacy carries little or no weight. Individual motivations are directed outwardly toward the group, which probably accounts for the Hopi's extraordinary oversensitiveness to group criticism. Mere acquisition is not a Hopi incentive. Of all pleasure-giving activities, work with the group—in household or field or ceremonial—ranks among the highest. Aggression toward others is looked on with extreme aversion. The very name "Hopi" means "the peaceful people."

Yet the Hopi society is not a pure Utopia. The stress of the exacting physical environment and of the still more exacting social environment tells on the Hopi personality in a certain lack of spontaneity, often amounting to rigidity. A powerful, abstract, intellectual development has been gained at some loss of creative exuberance. Yet despite these stresses and strains, the Hopis are decidedly not a neurotic people. In fact, the Hopi personality is highly intelligent, balanced, rounded, stable, yet varied, richly developed, and frequently creative. It is also responsible, duty-bound, cooperative. It is overwhelmingly group-minded rather than ego-minded. The Hopi has freedom—but freedom within the law. What he has sacrificed in emotional spontaneity he has gained in security and group survival. It is something more than blind love of ancient ways that makes the shrewd and sophisticated Hopis resist the encroachment of white civilization. From their lofty sky-cities, they look down on us in more than the physical sense.

The Hopis live richly, intensely, yet peacefully, and the substance of their lives is spiritual, not material. Has such a culture anything to offer to the modern distraught world beyond fortifying the yearning of many for a primitive escapism? I think it has many things, of which I shall mention only two or three. The Hopi society is an example of extreme decentralization. Not only skills but ethical sanctions are internalized in the individual to the point where the society can virtually dispense with coercive authority. The society is

self-managing. Modern government and industry, on the other hand, have steadily abstracted powers from individuals and primary communities and lodged them in central bureaucratic hierarchies. In this "managed" society, the anonymous standardized individual becomes a cog, or a serial number, with an undeveloped mind and personality suitable to his insignificant role. Such a society, in its later degenerative phases, as fascism shows, can be held together only by machine guns—and not very long.

The Hopi society is organized to develop complete, rounded, stable personalities devoted to the community rather than to the self. The Hopi people live intensely. Theirs is a pure democracy whose chief concern is the attainment of human excellence. In material things the Hopis are poor, but not squalid. They are not oppressed by material luxury or the need of it. Their wealth is people, not goods. The simple richness of their lives, the power of their minds, grow out of their preoccupation with the essential nature of things, with cosmic beauty and order, with the beauty of the earth and of the changing seasons and above all with the beauty of people and of human relations. The teleology of the cosmos, Whitehead has said, is devoted to the unfolding of variety, intensity, and beauty. In its own way, the Hopi culture exhibits these qualities in an organic community timelessly rooted in and flowering out of the order of nature.

Since the Renaissance, Western man has been dominated, in all aspects of his life, by the concept of egocentric individualism, fortified in more recent times by the doctrines of survival of the fittest and of automatic linear progress. These dominant Western concepts have played an important historic role in the development of science, technology, and our important but only half-realized ideals of democracy. Nevertheless, in their final stages, they have been primarily responsible for the disintegration of Western civilization, since they ignore the supreme nurturing function of society vis-à-vis the individual personality. Paradoxically, the frantic quest for egocentric individualism ended by more and more diminishing the individuality, the freedom, and the very souls of Western men. More and more they have been subjugated to the machines, the owners of machines, and the powerful centralized state whose *raison d'être* is to keep the machines running.

Western civilization has been darkened by the fog of an unworkable materialism which does not nourish the basic needs of the human personality. Its materialistic goals are juvenile, recessive, atavistic; the "abundant life" we visualized is trivial and thin. We have been building a pyramid civilization that has everything but a soul. It is therefore not surprising that our civilization has become predominantly psychoneurotic, and that modern men are filled with anxiety, boredom, cynicism, and hopelessness. Nor is it surprising that an economy devoted to egocentric materialism and human exploitation almost ceased to function except for war production. The extraordinary paradox of modern civilization is that, as men have gained unprecedented scientific power, they have steadily lost self-respect, pride, and dignity. Probably at no stage in history has the human ego been so dangerously deflated. It is not only disillusioned about itself, but it even has the temerity to be disillusioned about the cosmic process. These are not the normal symptoms of a healthy biological organism, nor is there anything visible in the nature of the cosmos to doom man perpetually to such a pathological outlook. They are the product of frustration. Our civilization is suffering from acute spiritual anemia.

No one would be so naive as to sup-

pose that a modern technological culture can adopt the ancient, prescientific culture of the Hopi Indians. But it is possible for modern civilization to create a rich, selfless, intense manner of social living, to reorient itself to the production of full human personalities, who in turn devote themselves cooperatively to great social ideals of beauty and excellence. The modern analogue of the Hopi democracy is a completely cooperative democracy, which will not tolerate the subjugation of man by man and which, by subduing the economic system to an efficient but secondary role, will open the way for developing an intense, localized, diversified, essentially spiritual culture. All men are potentially great because all men share in the universal mind. The essential goods of civilization are its constituent personalities. Human history does not move toward some "far-off divine" event. Fulfillment of life is here and now: "Our road is also our goal." The burden as well as the privilege of life is that it must be lived fully and intensely, each creature unfolding its full potencies and fulfilling its own nature. The alternative is degeneration.

The fateful choice of our civilization is not between guns and butter, but between half men and whole men. The Hopis cannot give us the blueprint for a new civilization, but they can instruct us in the nature of society as the nurturing ground of whole men and in the essence of true democracy, in which the eternal and yet infinitely malleable substance of human nature is wrought out to its full beauty.

SAINT LOUIS

From December 7, 1941 to August 14, 1945, scientists were preoccupied with war. Free neither to write papers nor to talk about their work, they had no time to travel to scientific meetings, few facilities for travel, and still fewer meetings to travel to.

Meanwhile new knowledge has been acquired; new applications of old facts have been discovered; new techniques have been evolved; new equipment has been invented. The scientific atmosphere is charged almost with the potential of the journalistically overworked atomic bomb, and it is appropriate to channel the pent-up energy into a scientific meeting. This is what the Association and many of its affiliated societies propose to do at Saint Louis from Wednesday March 27 through Saturday March 30, 1946.

The meetings are predestined to be a success. Perhaps there will be few definitive papers dealing with the phenomenal scientific developments of the war; possibly the exhibitors will have trouble filling their booths in the Auditorium with the latest gadgets and inventions, which are not yet in production; probably the overcrowded trains and hotels will cause personal discomfort, and the dates of the meeting—the only ones available anywhere for a meeting of this size—will prove inconvenient. But for other and more basic reasons the Saint Louis meetings will be a success: Scientists throughout the nation have much to talk over—perhaps more in the lobbies than in the meeting rooms. The isolation, the specialization, the pressure of the war years demand release through coversation and discussion.

Actually, the scientific fare, from A. J. Carlson's address of March 27 down to the smallest exhibitions in the Auditorium, gives every promise of being unusually good. But what guarantees success at Saint Louis is the resumption of scientific fellowship, the personal exchange of greetings, opinions, and experiences following the long and unrelieved years of war.—H. A. M.

THE DEVELOPMENT OF THE CONCEPT OF HEAT—I

FROM THE FIRE PRINCIPLE OF HERACLITUS THROUGH THE CALORIC THEORY OF JOSEPH BLACK

By MARTIN K. BARNETT

THE development of modern investigations on the phenomena of heat is connected with two distinct notions as to the nature of heat, the one asserting the substantial ("fluid") nature of heat, the other maintaining that what we perceive as heat is essentially a rapid vibration of the particles of the body which feels hot. It will be of interest to trace the history of these ideas, the roots of which are to be found in antiquity.

The Greek Notion of Fire. The phenomena of combustion must have early excited wonder and speculation, but the first European to give the concept of "fire" a prominent position in a system of natural philosophy was the Greek philosopher Heraclitus (cir. 500 B.C.). However, as Windelband (1, p. 36) notes, when Heraclitus declared fire "to be the essence of all things, he understood by this not a material or substance which survived all its transformations, but just the transforming process itself in its ever-darting, vibrating activity, the soaring up and vanishing which corresponds to the becoming and passing away." Hence, it would be a superficial view, indeed, which regarded the Fire Theory of Heraclitus as the root of the phlogiston theory or the caloric hypothesis which flourished over a thousand years later. Rather, in his conviction that all things are in a continual flux, that permanence is illusion due to the strife of opposites, he is closer to the kinetic view of heat.

A much closer approach to the substantial view of heat is found in Empedocles' (450 B.C.) four-element theory. He postulated as elements earth, water, air, and fire and thought that all change must consist in the dividing, intermingling, and separation of these fundamental constituents, regarded as imperishable, homogeneous, and unchangeable. The four-element theory gained great influence through its adoption by Plato (427–347 B.C.) and Aristotle (384–322 B.C.), the latter, however, introducing an important modification, namely, the postulate that the elements may be transmuted into one another by modification of their properties.[1]

Ellis (2, p. 76) thinks that earth, water, air, and fire of the Greek four-element theory correspond to the modern terms, solid, liquid, gas, and energy. With respect to the first three of these, we may agree with Ellis: all solids, all liquids, all gases were no doubt referred to as earth, water, and air, respectively, the generic terms not yet having been formulated. The identification of the Greek fire-element with energy is, however, objectionable. In the burning process, a bright fluid appears to emerge and it was no doubt this which Empedocles, and Aristotle after him, called "fire." On the other hand, the recog-

[1] Aristotle's properties were hot, cold, dry, and wet. Of these, hot and dry, especially hot, were supposed to belong to fire; hot and wet, especially wet, to air; cold and wet, especially cold, to water; and cold and dry, especially dry, to earth. By alteration of properties the one basic kind of matter could assume the form of fire, air, earth, or water.

nition of the peculiar nature of this "element" is already evidenced by the fact that Heraclitus chose fire as symbolic of the principle of *change* and restless activity. Aristotle (1, p. 147) regarded fire as endowed with a peculiar *centrifugal force,* and Democritus (460–360 B.C.) considered the "fire atoms" to be the cause of life processes.

Thus we see that in the Greek notion of "fire," three ideas representing different aspects of the combustion process are confused and they remained confused until modern times, when they finally received clear formulation in the distinct concepts of "quantity of matter," "quantity of heat" (or, more generally, "quantity of energy"), and "available energy" or "motive power."[2]

Greek Atomism. In the meantime, there had developed another view of the world which was destined to become, many centuries later, the foundation of the kinetic theory of heat. That view was Greek atomism. We have already noted how Heraclitus, captivated by the manifold change and variety in nature, had asserted that the essence of reality was simply flux or continual change, symbolized by "fire." On the other hand, Parmenides (470 B.C.), founder of the Eleatic School, adopting as his definition of Being, that which fills space (corporeality), reasoned that, since non-Being can neither be conceived of nor exist, therefore there exists only one Being which fills all space. Further, Being, if it were to change, could only pass into non-Being; because this is inadmissible, since non-Being does not exist, it follows that Being is eternal and unchangeable, and that all change as well as qualitative distinctions, must be illusory.

Democritus, the great systematizer of the Atomistic School, confronted with the opposition between the doctrines of Heraclitus and Parmenides, attempted to effect a compromise. He adopted Parmenides' notion of Being, as a stuff whose sole properties are eternality and corporeality (space-filling). But to explain the world of changing phenomena so insistently emphasized by Heraclitus, he supposed this Eleatic Being divided into small bits, scattered throughout empty space, and endowed with random motion. The continual flux of natural process, observed by Heraclitus, is nothing more, according to atomism, than the unceasing coming together and disintegration of the atoms, which, to be sure, are assumed to differ in size and shape. "All becoming, or change, is in its essence motion of atoms in space" (1, p. 43).

However, it is only insofar as the kinetic theory of heat may be regarded as derived from the general notions of atomism that Democritus and his followers may be regarded as the true fathers of the modern kinetic notions regarding the nature of heat. For when we inquire how the Greek atomists regarded the phenomena of fire and heat, we find that these were attributed not to any particular state of motion or configuration of the atoms of a body but to the presence in it of a particular species of atoms, namely, the "fire atoms." The "fire atoms," says Democritus, constitute a particular kind of atoms, namely, "the finest, smoothest, and most mobile." This phrase "most mobile" may be interpreted to mean that the "fire atoms" were characterized by a greater velocity than the other atoms. But the modern notion that an ordinary atom, by acquir-

[2] As an element in the same category as earth, water, and air, "fire" is "matter"; as identified with perpetual process (Heraclitus), it is "quantity of energy"; and as endowed with a peculiar force (Aristotle), as the "principle of disturbance" (Empedocles), as endowed with unusual mobility (Democritus), or as the principle of restless activity (Heraclitus), it corresponds roughly to "available energy," or "motive power."

ing through impacts a greater velocity, can become a "fire atom" is implicitly denied by Lucretius, the great popularizer of Greek atomism, when he states that atoms have always moved and always will move with the same velocity (3, p. 139).

The manner in which the Greek atomists regarded the phenomena of fire and heat is typical of the manner in which they explained many of the properties of matter: these properties were regarded as due to intrinsic, unchangeable properties of the atoms in question rather than to the momentary condition of these atoms. Thus the peculiar fluid nature of liquids was attributed to the smoothness and roundness of their atoms. Things which were painful and harsh to the senses were supposed to consist of hooked, rough atoms.

Thus, with respect to the nature of heat, Greek atomism is seen to occupy an intermediate position, which, indeed, was revived in modern times, especially by continental Europeans (4, p. 31). Insofar as all things were regarded as constituted of atoms moving in space, Greek atomism foreshadowed the kinetic theory of heat. But in postulating that fire and hot bodies contain *particular kinds* of atoms (although, to be sure, they were not considered to be atoms of an imponderable fluid), it tended to agree with the Aristotelian postulate of a fire-matter, which was the forerunner, not only of phlogiston, but also of a much more fertile conception, namely, Black's caloric.[3]

The Alchemical "Principles." The Hellenistic-Roman and Medieval Periods contributed relatively little to the heat concept. The best European minds of these centuries were occupied with questions of logic, ethics, or theological metaphysics. The only advances in the knowledge of heat were those gleaned by the alchemists of Egypt and Arabia and later of Europe, in their practical, partly magical, attempts to prepare gold from the baser elements and to discover the elixir of life.

Throughout this period, the teachings of Aristotle functioned as the guiding light, especially among the Arabians, who introduced them into Europe through the invasion of Spain in the eighth century and also through contacts made during the Crusades.[4] Like Aristotle they regarded the one fundamental matter as capable of appearing in four different elementary forms (fire, earth, air, and water) which could be changed one into another by a proper variation of properties. However, in their practical work with the metals, they came to regard Aristotle's particular choice of properties as inadequate. Thus we find Geber, an Arabian alchemist of the ninth century, introducing the properties ("principles"), "mercury" and "sulfur," the first to account for the luster, volatility, fusibility, and malleability of metals, the second to account for their color, combustibility, affinity, and hardness. To these Valentine added the principle "salt" which was supposed to remain intact on chemical treatment and was regarded as serving as a sort of base for the union of the principles, mercury and sulfur. By variation of these three properties, the metals were thought to be capable of transmutation into one another.

It is not difficult to understand how

[3] This statement is not intended to divert attention from the fundamental opposition between Greek atomism and Aristotelianism. For Democritus the only primary, i.e., real, properties of matter are extension in space and motion. Aristotle, on the other hand, was quite opposed to any attempt to reduce phenomena to mechanical causes, e.g., "hotness" itself was, for him, a primary quality.

[4] Of Aristotle's works, only a portion of his logic was transmitted directly to European posterity by means of the Christian theology. The remainder arrived much later via Arabia (1, p. 268).

the alchemists came to associate sulfur with combustibility. A mixture of sulfur and nitre was used as a fusion mixture for the metals, so that sulfur was thought to have the property (intense heat) of fire in this respect. Further, its yellow color suggested the bright color of "fire-matter," which appears to emerge from a substance, leaving an uncombustible residue.

We must realize that the terms "sulfur," "mercury," and "salt" were used, at least in the earlier centuries of the period, in a manner which, from our point of view, was metaphorical.[5] They denoted properties rather than substances, although this distinction seems gradually to have been obliterated.

System of Paracelsus. Thus we find Paracelsus (1493–1541) attempting to reconcile the Aristotelian four-element theory with the three principles of the alchemists by assuming that sulfur is the product of the action of fire on air; mercury, that of the action of air on sulfur; and salt, that of the action of water on mercury. Paracelsus was a man of great influence, and his system, however imaginative, reveals an important fact, namely, that the three principles had come, in the minds of the alchemists, to acquire a substantial character so that they were placed in the same category as Aristotle's four elements.[6]

What had happened was, no doubt, that the three-element theory, because of its value in explaining the behavior of the metals, had come more and more into the foreground with the consequent neglect of the Aristotelian doctrine, and that, as a result, the former finally usurped the province of the latter, at least in the field of metallurgy, so that the three principles came to be regarded, not only as properties, but as substances as well. Then the system of Paracelsus may be regarded simply as an attempt to justify current alchemical ideas by referring them back to Aristotelian doctrine, regarded as fundamental.

That by the latter part of the seventeenth century, the sulfur principle, in particular, had come to acquire a definitely substantial character and, indeed, to be more or less identified with the substance which we now denote by that name, is evident from Becher's (1635–82) criticism of the alchemical combustion theory. Sulfur, he says, itself burns and therefore cannot be the principle of combustibility but must, like the metals, contain it. Becher definitely postulates that combustion is a process of decomposition, a view which seems to have been held by all of Aristotle's followers, and states that in the burning of a substance the combustible principle, which he calls *terra pinguis* (fatty earth), is evolved.

Advent of Phlogiston. Stahl (1630–1734) substituted the term "phlogiston" for Becher's *terra pinguis* and skillfully employed the hypothesis, under the name of the "phlogiston theory," to correlate the phenomena of calcination, combustion, reduction, and metallic displacement of hydrogen from acids.

Although the phlogiston theory came to exert a commanding influence on most of the chemists of the eighteenth century, it nevertheless shared the fundamental weakness of the sulfur theory which had preceded it, namely, the confusion between property and substance. Nor was this alleviated by Stahl's statement that phlogiston is "the material and principle of fire, not fire itself" (5).

The increase in weight of a substance, on burning, and the essential part played

[5] Wisely avoiding confusion, Brown (5) calls the alchemical principles "*philosophical* sulfur," "*philosophical* mercury," etc.

[6] This is particularly evident from Paracelsus' description of the burning process: "That which smokes and evaporates over the fire is mercury, that which flames and is burnt is sulfur, and all else is salt" (2, p. 161).

by the surrounding air were the main facts which finally overthrew the phlogiston theory of combustion. The advocates of the theory met the first objection in two ways. Some of them regarded phlogiston, not merely as an imponderable, but as actually endowed with negative gravity: they regarded its presence as functioning to decrease the effective weight of a body. This idea was advocated by numerous alchemists before Becher's time and indeed was shared by Aristotle who, as we have noted, endowed his fire-matter with a peculiar centrifugal force. Others maintained that the increase of weight was due to the addition of *ponderable* fire-matter during the combustion process: the fire in the flame was thought to penetrate the container to which it was applied and combine with the burning substance. The second objection, that the presence of air was necessary for combustion, was met by the assumption that it (the air) functioned as an absorbing agent for the phlogiston liberated.

By the latter part of the eighteenth century, the phlogiston theory had become extremely unwieldy; in fact, phlogiston had been variously identified as light, electricity, soot, and hydrogen, and indeed, in France, was still associated with sulfur (5). Hence the penetrating researches of Lavoisier (1775) on the weight relations involved in combustion were quite sufficient to cast out from the minds of most chemists the 2,000-year old notion that burning is a process of decomposition and to establish in its place the postulate that combustion consists in the union of the burning substance with the oxygen of the atmosphere.

However, as Merz (6, Chap. VIII) has noted, the phlogiston theory, insofar as it asserted that something was liberated in combustion, contained an element of truth which was destined to bear fruit. Lavoisier's great service showed that the mysterious phlogiston had no place in a description of the *weight* changes incurred by burning. But a more careful investigation of the changes occurring in the surroundings was to lead to a sort of reincarnation of the phlogiston theory. Our modern phraseology still bears witnes to that reincarnation, for we say that heat is liberated in burning.

Nor had the concept of an imponderable fluid, exemplified by phlogiston, reached the end of its scientific usefulness. Rather, this end had come only in the case of chemistry proper, which, following the example of Lavoisier, proceeded to concentrate on the study of the *weight* relationships in chemical change. In those branches of physics concerned with changes of state or condition, the concept of an imponderable fluid continued to be of service. We shall see that, as caloric, it rendered invaluable service in comprehending and classifying the phenomena of change of state, expansion, compression, and reciprocal changes of temperature. In the eighteenth century Watson (1746) and Franklin (1706–90) independently proposed fluid theories of electricity. Later, Coulomb (1785–1789) advocated the two-fluid theory to correspond to positive and negative electricity. He also proposed two magnetic fluids. These fluid theories of electricity and magnetism served as the basis for the mathematical formulation of the laws in these branches of physics.

Rise of the Kinetic Theory of Heat. In the meantime, epoch-making advances in astronomy and mechanics had brought the motion concept into great scientific prominence, and this fact, together with the revival of Greek atomism, led more imaginative minds to the formulation of the modern *kinetic* theory of heat.

It is of interest to note that in the cases of Bacon (1620) and Descartes (1640), motion theories of heat are advanced independently of atomism. This was

done, in the crudest form, by Francis Bacon (7, Chap. VIII) who, impressed by the motion of boiling liquids and fire, asserted heat to be the outward, upward, expansive motion of the small parts of a body. His doctrine is not a revival of atomism, for he states, specifically, that the motion is "not in the very minutest particles but in those of some tolerable dimensions," nor does he postulate a void. He does not explain how a liquid can be in violent motion without being hot or how, on the other hand, a red-hot iron may be in a state of rest. Descartes, though denying the existence of a void and postulating the infinite divisibility of matter, nevertheless states motion to be the cause of all change. In particular, he regards the sensation of heat as due to motion communicated to the nerves, an old idea of Democritus (1, p. 114).

Greek atomism was revived and popularized through the efforts of Magnenus (1646) and especially Gassendi (1647). The union of the motion and atomic concepts to account for thermal phenomena had not long to wait, for we find Boyle (1629–91) asserting, in quite the modern fashion, that heat is a molecular motion and Locke (1690) describing it as "a very brisk agitation of the insensible parts" of an object (8, p. 24). Hooke (1665) adopted a similar view toward the property of fluidity, attributing this, not to the shape of the atoms of the fluid, as the Epicureans had done, but to "a certain pulse or shake of heat."[7]

Newton, too, who as Merz (6, Chap. VI) notes, first made the kinetic view of nature scientifically possible through the publication of his *Principia* (1687), states that "heat consists in a minute vibratory motion of the particles of bodies" (9, p. 84). These views were strengthened when Bernoulli (1738) laid the foundation of the kinetic theory of gases.

Relation of Light and Heat. The tendency to associate heat with motion received a fresh impulse from the establishment, by experiment, of the close relationship existing between light and radiant heat. For whether light be viewed according to the corpuscular emission theory favored by Newton or the undulatory theory of Huygens (1690), it is, in either case, a motion (in the first case, a streaming motion of particles; in the second, a vibratory motion in the ether), so that radiant heat, so closely related to it, must also be of the nature of motion. We have just noted Newton's kinetic view of heat. Huygens also held such a view, for he not only states that light is a motion, but also that the solvent properties of heat and flame, the causes of light, indicate that they too are of the nature of motion (10, p. 211).

The close connection between light and heat had already been vividly exhibited by the properties of the burning-glass of Archimedes (287–212 B.C.), and in the thought of the medieval alchemists the two were pretty thoroughly confused. In modern times we find a renewed interest in these phenomena. Tschirnhausen (1699), concentrating the rays of the sun with two large lenses, succeeded in burning wood, boiling water, and melting lead and iron. By coating the metals with charcoal, he was able to volatilize them, from which he deduced the salient conclusion that black bodies absorb the most heat. At the same time he cast doubt on the legitimacy of generally identifying light and heat when he observed that the rays of moonlight could not be made to produce any noticeable heating effect.

Boyle, by noting that the burning-glass remains operative in a vacuum, had already demonstrated that the propaga-

[7] "For heat being nothing else but a very brisk and vehement agitation of the parts of a body," these "are thereby made so loose from one another, that they easily move any way and become fluid" (2, p. 96).

tion of heat and light is independent of the material medium, and Scheele (1777) verified this with the observation that heat felt ten feet from an oven is unaffected by an intervening stream of air. Scheele also made the important distinction between this sort of heat which he called "radiant heat" (*strahlende Wärme*), and the heat passing out through the chimney with the smoke (convected heat).

Lambert (1779), no doubt influenced by the emission theory of light, assumed that radiant heat consists of streams of "fire-particles," reminiscent of Democritus. He was also convinced that these "fire rays" obeyed the same laws as light, and proceeded to deduce the laws of the burning lens from the principles of optics. At the same time, he was aware that the rays of "dark heat" (infrared rays) were reflected in the same manner as light.

The close similarity of light and radiant heat was further demonstrated by Pictet (1790), who noted that a thermometer placed at the focus, or "burning point," of a concave mirror immediately registered a rise in temperature when a hot body was placed at the focus of a second mirror, coaxial with the first but at a long distance from it. From this he concluded that radiant heat must be propagated in straight lines at a very great velocity, perhaps at the velocity of light itself. He contrasted this with conducted heat which, unlike radiant heat, must proceed from particle to particle of the medium and consequently travels much more slowly. However, a complete distinction between radiant and conducted heat is not attained by Pictet, for, on failing to concentrate the heat from boiling water by means of a glass mirror, he thinks this might be done with a mirror which is a better conductor of heat, e.g., a metal mirror.

Hutton (1794) identifies light with radiant heat and postulates that dark bodies also emit radiation, even though undetectable by the eye. A hot body, he says, converts heat to light which, on absorption, becomes heat again. A step backwards appears to have been made by Herschel (1800) when he stated that every ray consists of a light ray and a heat ray.

Rumford (1805), famous for his experiments on frictional heat, regarded a body which radiates heat as analogous to a bell sending out vibrations (heat waves). The temperature of the hot body, he says, is determined by its period of vibration. A series of experiments led Rumford to further important conclusions: (1) all bodies radiate at every temperature; (2) the intensity of radiation is different at the same temperature for different bodies (substances); (3) bodies at the same temperature do not influence each other through radiation.

To Leslie (1804) is due the important observation that strongly reflecting surfaces are also poor emitters of radiation and that heat absorption and heat emission increase and decrease together. He also discovered that the intensity of radiation (from a unit area of surface) at a point outside a radiating body decreases with the angle made by the surface of the body with the line of propagation of radiation to the point. Leslie's speculations on the nature of heat are less fortunate. For, ignoring Boyle's discovery that radiant heat is propagated in a vacuum, he attributed radiant heat to air pulsations set up by the radiating (vibrating) body, thus confusing radiation with convection.

Prevost (1809), who clearly distinguished between the radiation and conduction of heat, adopted the emission theory of radiant heat (*calorique rayonnant*) and developed it along lines suggested by Bernoulli's kinetic theory of gases. He notes that glass retains "dark heat" but allows radiant heat to pass through and thinks this may be due to

the fact that there are several kinds of heat particles. Rumford had already postulated that all bodies radiate at every temperature,[8] and Prevost brought this principle into great prominence in his celebrated "Theory of Exchanges." A hot body is simply one which emits heat particles at a greater rate than colder bodies. The apparent state of rest characterized by equality of temperatures is really, says Prevost, a state of dynamic equilibrium in which the bodies emit the particles of "radiant caloric" at the same rate.

It will be unnecessary to describe here Fourier's (1817) mathematical systematization of the facts of radiation, nor shall we relate the long series of investigations, extending through the nineteenth century, which served to establish the agreement of radiant heat and light in all their properties, with the resultant triumph of the undulatory theory of Huyghens, the latter having been revived and developed by Young (1801) and Fresnel (1819). Rather, it will suffice, for our purposes, to point out just what significance is to be attached to that period of the history of the subject, ending in the early years of the nineteenth century.

In the first place, we cannot ignore the heuristic significance which these investigations had for the establishment of the First Law of Thermodynamics. For, entirely independent of the question of the precise nature of light and heat, the facts of their close association, conformity to the same laws, and their convertibility constituted undeniable evidence in support of that comprehensive, though vague, view that all of the "forces" of nature are "correlated." The precise manner in which this notion of "correlation of forces" contributed to the founding of the Energy Principle naturally belongs to a history of that principle.

Of more immediate concern is the effect of these radiation studies on current ideas concerning the nature of heat. We have already remarked that, insofar as radiant heat, like light, is regarded as a motion of something, the study of it tended to bolster up the kinetic theory of heat. If, nevertheless, the caloric or fluid theory continued to maintain its supremacy throughout the eighteenth century, this may be attributed, to a large extent, to the circumstance that Newton's emission theory, not Huyghens' wave theory, of light (hence of radiant heat) prevailed among most of the physicists of that period.[9] For if light is to be regarded as a rectilinear motion of very fine particles, radiant heat must also be so regarded. And, since the radiating body exhibits no loss in weight, the particles must be those of an imponderable fluid, naturally identified as heat fluid, or caloric, since the radiating body suffered a fall in temperature.[10]

[8] Hutton first noted the contradiction involved in assuming that only the hot body radiates: the same body, at the same temperature and in exactly the same state, is assumed to radiate, or not radiate, depending on whether it is the hotter or the colder of the two bodies.

[9] This we may attribute to the failure of Huyghens' theory to account for the nature of shadows and for rectilinear propagation, together with the fact that Newton's great authority opposed it. Young (1800) revived and expanded the wave theory in England, but his work was hardly noticed. It was Fresnel (1817), working in France, who finally convinced scientific authority of the superiority of the undulatory theory, but only after overcoming vigorous opposition. Thus his most important memoir on the subject, though submitted to the French Academy in 1821, was not published until 1827 (11, ii, p. 115).

[10] A curious dilemma must have presented itself to those scientists who opposed, on general principles, the assumption of an imponderable fluid. For if the existence of an imponderable heat fluid be denied, that of another, namely, Huyghens' ether, must be admitted to account for the propagation of radiation through a vacuum.

(*To be concluded*)

SCIENCE ON THE MARCH

THE SPECIFIC EFFECTS OF CERTAIN TEMPERATURES ON STORED FRUITS, VEGETABLES, AND FLOWER BULBS

For many years it has been the custom to hold certain fresh fruits and vegetables in cold storage for the purpose of preventing spoilage. It is the general consensus that the low temperatures in the storage rooms serve to arrest or retard the biological processes that bring about senescence and decay, and for the most part this belief is correct. However, there are certain physiological processes that not only are able to continue at these low temperatures but even appear to be accelerated. Sometimes a certain temperature or narrow range of temperatures appears very specific in producing certain effects on fruits, vegetables, tubers, and bulbs.

It often happens, for example, that when citrus fruits are held at 32° F. for four to six weeks there develops a physiological disorder known as "watery breakdown." Although it gives the fruits the appearance of having been frozen, carefully controlled experiments have demonstrated that this disorder may be produced by low temperatures that are still above the freezing point of the fruits. Pitting (pox, storage spot) is another low-temperature disorder of citrus fruits. It is characterized by shallow, pocklike depressions in the rind, which become discolored in the advanced stages. Temperatures most conducive to pitting are 32° for lemons, 36° to 40° for oranges, and 40° for grapefruit.

Still other, more or less superficial, rind blemishes of citrus fruits are "scald" of oranges and grapefruit and "peteca" and "red blotch" of lemons. "Membranous stain" of lemons, a darkening of the membranes between the segments, has been found much worse at 36° and 40° F. than at 32°, 50°, and 60°.

Tropical fruits are still more sensitive to low temperatures than are the subtropical citrus fruits. If papayas are held for only 5 days at 45° F. or lower, they become chilled, and the treatment so upsets their metabolic processes that they will not ripen properly when removed to higher temperatures. Experiments have shown that certain chemical processes that accompany normal ripening, such as inversion of sucrose, are arrested by exposure to low temperatures and are not resumed when the fruit is brought back to room temperatures. In like manner avocados, pineapples, and bananas experience a physiological breakdown when held too long at low temperatures. The result is usually a darkening of some of the tissues when the fruit is removed to higher temperatures. It is not necessary to drop very low in the temperature scale to produce deleterious effects in bananas. Green fruits, if held at temperatures below 56° for four or five days, will subsequently fail to ripen properly in the ripening rooms. Bananas that have been chilled after ripening will develop a dull-brown color when later exposed to higher temperatures, and are very susceptible to handling marks, the slightest bruising causing discoloration. Darkened bananas sometimes appear on the fruit stands after the merchant or the deliveryman has been caught unawares by sudden and unexpected cold weather, and the consumer has been known to hold bananas in the refrigerator long enough to have them darken when brought out into the room.

Tropical vegetables may suffer from low-temperature injuries just as much as tropical fruits. Tomatoes are not

ordinarily thought of as tropical because they are grown so extensively in temperate latitudes, yet their home is in the tropics. When wholesalers have attempted to hold too long at near 32° the mature green tomatoes, or "green wraps," as they are commonly called, they have experienced rather disastrous results. These chilled tomatoes, when removed to the ripening rooms, will ripen with a dull-yellow color, or they may decay before they ripen.

The average farm boy knows that the first frost is the signal to dig and cure sweet potatoes before they are injured by cold. Darkening and internal breakdown of the tissues occur in storage rooms also, when the temperature is below 40° F. The susceptibility of sweet potatoes to low-temperature injury is reduced by proper curing. It is interesting to note that some success in prestorage "curing" of grapefruit has been reported, the processes for the two products being alike in principle, though not in details.

Even flower bulbs are subject to specific physiological effects of low temperatures. Several years ago in experiments in the U. S. Department of Agriculture, King Alfred narcissus bulbs were held during three successive storage periods. Temperatures during the first and third periods were in the range of 60° to 90°, but the middle period came within the range of cold storage, that is, 32°, 40°, and 50°. When these bulbs were held at 40° during the middle period of storage there subsequently developed more "blind," or nonblooming, bulbs than when they were held at 32° or 50°.

Here is an instance of the specific effect of one temperature, or perhaps a narrow range of temperatures. Another example, previously mentioned, was that of 36° and 40° in their relation to the development of membranous stain in lemons. Other effects of specific temperatures might be cited. Continuous storage at 50° F. produces "pumpkin-yellow" grapefruit, a good red color in Haden mangoes, and considerably more "blood" spots in the so-called blood oranges. These effects are not produced by temperatures some degrees higher or lower, or at least not so rapidly.

Examples can be cited in which certain desired physiological changes have been produced in fruits, vegetables, or flower bulbs by employing the proper temperatures in storage. Early-season Bartlett pears from California arriving on the Eastern markets during July and August frequently fail to ripen with good color, flavor, and texture. Tests made by the U. S. Department of Agriculture showed that if these pears were ripened at 65° to 70° F. the flavor, color, and texture were satisfactory. Those ripened at 80–85° F., the temperatures prevailing in the stores at the time, were of poor quality. Fall and winter varieties of pears likewise are of best quality when ripened at temperatures between 60° and 70° F. Kieffer pears are usually considered to be of very low quality, and it is probable that this reputation has been based on the fact that they usually ripen during exposure to too high temperatures, at least in the South. Experiments have shown that when this variety of pear is ripened at 55–65° the quality is greatly improved, although it never becomes the equal of such a variety as Bartlett.

About 1928 or 1929 several restaurant operators and manufacturers of potato chips came to the U. S. Department of Agriculture with a problem. They stated that their French fried potatoes and potato chips were too dark for the customers and that the dark chips had an undesirable flavor. Now a dark color in a cooked product may be due to the caramelization of sugar, and the Government research men knew that there are certain storage temperatures at which sugar accumulates in Irish potatoes.

This had been worked out many years ago at the University of Maryland when it was reported that Irish potatoes become sweeter if held in storage at 40° F. or lower. A checkup revealed that the potatoes used for French "fries" and chips in this case had been previously stored at 32°. Subsequent tests by the U. S. Bureau of Plant Industry in cooperation with the Bureau of Home Economics of the U. S. Department of Agriculture revealed that if potatoes were stored at 50–60° F. for a while immediately before use they could be made into chips or French "fries" white in color and of good quality.

Examples may also be cited in which physiologists have utilized low temperatures for the purpose of stimulating seed production in certain vegetables. For instance, the production of flowers by sugar beet plants is objectionable when these plants are being grown for sugar, but when the object is the production of seed, the development of flowers is quite obviously necessary. The initiation of seedstalks and the flowering of biennial beets have been shown to be due to the cumulative effect of low-temperature exposure followed or accompanied by the effect of long photoperiods (duration of daylight). When research workers in the U. S. Department of Agriculture were breeding sugar beets and growing them in that part of the country where the length of day was insufficient to induce flowering, they overcame this by the use of cold storage. The tendency to "bolt," or produce seedstalks, was greatly stimulated by exposing the beets to low temperatures (33–38° F.) before planting in the field. The treatment could be applied to the "mother" beet roots or to the germinating seeds.

Similar work has been done on onion bulbs. Storing them at 53.5° F. for four months prior to planting has caused the plants to blossom and ripen earlier and to yield more seed per acre.

One more story of the specific effects of certain temperatures might be cited. Bulbs of the Creole type of Easter lily, when planted immediately after midsummer digging, bloom in the period between April 3 and April 12. This lily is in great demand at Easter. However, Easter is one of the few holidays that has no definite calendar date. It may come any time from late in March until late in April, and the disastrous results for the florist of having his lilies bloom too early or too late can easily be imagined. There is still another reason for desiring to change the blooming habits of the Creole lily. In some sections of the country the trade demands lilies at Christmas. To meet these varying demands for lilies, research workers attempted to modify the physiological response of lily bulbs by storing them at different temperatures following harvest. In these experiments the unstored lily bulbs bloomed 259 days after planting. However, if stored 10 weeks at 36°, 40°, 45°, 50°, 55°, and 59° F. after the bulbs had matured, blooming occurred from 111 to 135 days after planting. The temperature treatments that hastened the blooming of these bulbs also tended to reduce the number of blooms per plant, and this effect increased with increase in storage temperature. Because of this fact, 36° to 45° F. is usually recommended for storage of Creole lily bulbs intended for forcing of bloom.

The lay reader often objects to the manner in which scientific material is presented, pointing out that the impartial viewpoint usually leaves the reader with no definite answer to his questions. On occasions when this subject has been presented to groups or organizations, the speaker is invariably questioned as to the proper, rather than improper, temperatures at which various products should be stored. This information is available in publications of the State and Federal agricultural research agencies. For

some of the fruits and vegetables mentioned above the U. S. Department of Agriculture makes the following recommendations for temperature of storage: Bananas, ripening, 62° to 70° F., holding ripe fruit, 56° to 60°; grapefruit, 32° to 34° if grown in regions where decay is a serious factor, or 45° to 55° if from other regions; lemons, 55° to 58°; oranges, 34° to 38°; pineapples, mature green, 50° to 60°, ripe, 40° to 45°; potatoes, no lower than 40° for table or seed stock, unless the seed is to be kept longer than 3 to 5 months, in which case 36–38° F. is recommended; sweet potatoes, 55° to 60°; tomatoes, ripe, 40° to 50°, mature green, 55° to 70°.

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FACTORS CONTROLLING THE CONCENTRATION OF ERYTHROCYTES IN THE BLOOD*

THE existence of a sex difference in the erythrocyte (red cell) concentration of the blood has been observed in numerous animals, among which are the domestic fowl, pigeon, ring dove, mouse, rat, rabbit, cat, dog, sheep, horse, and man. In each of these species the erythrocyte count of the male is consistently higher than that of the female. For example, in the domestic fowl the average count for males is 3,260,000 red blood cells per cubic millimeter of blood, while that of females is 2,660,000. Similarly, in humans the average red cell count for a man is approximately 5,000,000 as compared with 4,500,000 for a woman.

It has been shown by observations on many castrated animals that this difference between the sexes is due, at least in part, to the reproductive glands: ovaries or testes. In a male such an operation is followed by a fall in the erythrocyte count, while in a completely ovariectomized female there is either no significant change or there is a slight rise in the number of red blood cells. Therefore, the castrated male and the ovariectomized female have approximately the same red cell concentration. Furthermore, if a testis is successfully grafted into such a castrated rooster, the red cell count will rise again to the normal male level.

In the domestic fowl, as well as in most other birds, there is only one functional ovary—the left one, for the right gonad remains undeveloped and persists throughout life as a nonfunctional rudiment. If the functional left ovary is removed from a young chick, the right rudimentary gonad will develop into a testis-like organ which has the endocrine characteristics of both an ovary and a testis. A bird possessing such a gonad frequently shows both male and female characters, such as the large comb of the male and the henny plumage of the female. It is also interesting to note that the erythrocyte counts of such individuals lie somewhere between the normal male and female levels but are usually closer to that of the male.

When male sex hormones (androgens) are injected or administered by means of pellets implanted beneath the skin, the number of red cells will increase in the castrated male or female bird, rat, dog, monkey, or human, while treatment with female sex hormones (estrogens) will induce anemia, or a decrease in the number of red corpuscles. By means of such androgen pellets the red cell concentration has been maintained at the male level in groups of castrated roosters, or capons, for periods of over two years. When pellets are almost or completely absorbed, the erythrocyte count drops, but after each subsequent implantation it will rise again to the male level.

* This investigation was aided by a grant from the Dr. Wallace C. and Clara A. Abbott Memorial Fund of The University of Chicago.

Since sinistrally ovariectomized poulards (hens from which only the left ovary has been removed) develop right testis-like gonads secreting both androgens and estrogens and usually show a red cell count intermediate between the male and female, it became of interest to see if such a condition could be produced experimentally in normal or castrated animals by simultaneous androgen and estrogen treatment. This problem has been approached in two ways:

(1) On the fourth day of incubation (the normal incubation time for the domestic fowl is twenty-one days) before the reproductive glands have developed in the embryo, a small amount of estrogen was injected into the egg, which was then allowed to continue incubation and to hatch. Genetically, male chicks hatched from such treated eggs are of an intersexual type when mature, possessing gonads which secrete both androgens and estrogens, as do those of sinistrally ovariectomized poulards. Such individuals also have erythrocyte counts intermediate between those of normal males and females.

(2) Capons were injected with, or received pellets of, both male and female sex hormones simultaneously. If the proportion of estrogen administered is low as compared with the androgen, the red cell counts rise to the male level. If, on the other hand, the estrogen dosage is relatively high, the counts remain unchanged or fall. Thus far a duplication of the situation found in sinistrally ovariectomized poulards and in intersexual males which received estrogen treatment during incubation—where the red blood cell count was intermediate between those of the male and female—has not been observed. This may be due to an improper balance between the concentrations of the two sex hormones administered, or it may be that the methods employed in administering the hormones do not duplicate, in fully grown birds, the condition prevailing in poulards and intersexual males, where the influence of mixed male and female hormones is not only exerted relatively early in developbent but is also regulated by the bird's own endocrine system.

Also of interest in connection with the problems arising from simultaneous androgen and estrogen administration are observations which have been made on laying hens. Although the domestic fowl is not a seasonal breeder, as are most wild birds, there is, nevertheless, in the average flock, an increase in egg production in the late spring and early summer. During the laying period there is evidence of an increase in both estrogen and androgen production by the hen's own ovary. In spite of the increased amount of androgen, the erythrocyte counts made during the spring laying periods are lower than at any other season of the year. This would seem to indicate one of three possible interpretations: (1) the increase in androgen is not of sufficient magnitude to raise the erythrocyte level; (2) the increase in estrogen is sufficient to lower the red cell count regardless of the simultaneously increased androgen; (3) the seasonal cycle in females is caused by factors other than male and female sex hormones.

Of particular interest at the present time are certain observations made on male and female human blood donors. There have been several recent reports indicating that red cell regeneration after blood donation is slower in women than in men. Similar observations have also been made on rats in which severe hemorrhages were induced. It was found that androgen injections stimulated, while estrogen treatment actually inhibited, the regeneration of erythrocytes. This would seem to indicate that in females regeneration of red cells is normally inhibited by the animal's own sex hormones, whereas in males regeneration is accelerated.

The red blood cells are produced in the marrow cavities of the long bones and are destroyed and removed from circulation largely by the liver. The increase in the number of red cells in androgen-treated animals, therefore, could be due either to a stimulation of their production or to a decrease in the rate of their destruction. Conversely, the lower count in animals treated with estrogens might be due to an inhibition of erythrocyte production or to an increase in their rate of destruction.

Studies on the bone marrow of pigeons, mice, and rats, made chiefly by Gordon and his co-workers at New York University, have produced evidence that the first suggestion may be correct. These observations have shown that estrogens cause the formation of bone within the marrow cavities, thereby interfering with the production of red blood cells, while androgens appear to stimulate the erythrocyte-producing (erythropoietic) tissue within the marrow to greater activity.

It must not be concluded that the male and female sex hormones are the only agents which affect the concentration of the circulating erythrocytes. For many years it has been known that oxygen deficiency stimulates the bone marrow to produce more red cells, resulting in higher counts in individuals living at high altitudes. Also, it has been known that in fevers, as well as after severe muscular exercise, there may be a marked increase in the number of red cells (polycythemia). Likewise, the administration of cobaltous salts in some animals causes a marked polycythemia, which, in extreme cases, is fatal. On the other hand, various poisons, such as lead, may injure or destroy the erythrocyte-forming tissues, and the lack of certain substances in the diet, such as an adequate amount of iron, will cause an anemia. Likewise, other endocrine glands, notably the thyroids, adrenals, and pituitary, have been found to play a part in the regulation of the red-cell concentration.

Thyroid insufficiency will bring about a drastic reduction in the number of red blood cells. This has been interpreted as due, not to the direct effect of inadequate thyroid hormone upon the bone marrow, but rather to a more general metabolic effect, causing a slowing down of most of the physiological activities of the body. An overabundance of thyroid hormone produces a high erythrocyte count by stimulating metabolic activity, thereby creating an oxygen deficiency which causes an increase in the production of red blood cells.

Adrenal insufficiency results in a spectacular rise in the number of red cells, while treatment with extracts from the adrenal cortex reduces the number within a few hours. This is not believed to be due to any actual increase or decrease in the number of erythrocytes in circulation, but rather to an increase or decrease in the fluid constituent of the blood, which in turn would affect the apparent number of red cells.

The pituitary gland itself operates indirectly as a regulating mechanism since it produces hormones which control the activity of the reproductive glands, thyroids, and adrenals. Some investigators have postulated a specific pituitary hormone which has a more direct control over the concentration of red cells, but this hypothesis has few adherents.

Since all of these regulating factors, with the exception of the ovarian and testicular hormones, are presumably operating similarly in both males and females, it would seem plausible to conclude that the sex hormones play the major role in bringing about the differences observed in the erythrocyte counts of males and females.

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BOOK REVIEWS

THEORETICAL ASPECTS OF HISTORY

The Philosophy of American History. Morris Zucker. 2 vols. Vol. I. *The Historical Field Theory,* 694 pp. Vol. II. *Periods in American History,* 1070 pp. 1945. Arnold-Howard Publishing Company, Inc., New York.

THE first volume of this work claims to lay the theoretical foundations for a science of history. The second volume purports to apply the "historical principles" developed in the first volume. The present review will concern itself mainly with the first volume, as the second is devoted entirely to the familiar events of American history. As history, Volume II should be left to historians to evaluate. Both volumes abound in diatribes against the injustices and the imbecilities of our times.

There is no question about these maladjustments or the vigor with which the author expounds them. Indeed, his criticism and ridicule of most of the historians and the "statesmen" of the day are by far the most effective and valid part of his work. I share most of the author's prejudices on these matters and consequently had an inordinately good time reading these volumes. As an argument for the application of science to social affairs and an exposé of the disasters resulting from not doing so, the two volumes are sound, witty, and highly readable. As a treatise of scientific method or an example of its application, the work unfortunately leaves much—nearly everything—to be desired.

In spite of all his lip service to science in general and to field theory in particular, the author finds it necessary to repudiate the post-Einsteinian developments of that theory, which he finds inapplicable to social science. Instead he finds it necessary to adopt a view of causality (p. 585), which has been pretty well abandoned in modern philosophy as well as in science. The section dealing with these matters is thoroughly out-of-date. A few examples must suffice.

In insisting that the social sciences must rely on a type of causality abandoned in the other sciences, the author betrays a basic misunderstanding regarding the nature of all scientific generalizations whatsoever. The "inherent fallacy of the statistical method of analyzing vital phenomena" is illustrated, he thinks, by the following example:

> It can be shown that the average life expectancy is today much greater than it was twenty, fifty and a hundred years ago in every age class. The reasons for this cannot be discerned by the minutest scrutiny of the actuarial tables. They will not show the causes, but merely record the results of a long process of development within society which manifested itself in the standards of living, general sanitation, purer food, the growth of public medicine and a myriad of other factors which no statistical table will disclose. These can be established only by the patient labor of the classical scientific method (p. 584).

One wonders, in the first place, how, except through statistical methods, the manifestations of improved standards of living, general sanitation, etc., is revealed at all. Whatever *scientific* knowledge we have of these phenomena, *as well as of the processes by which they have transpired,* has been achieved through the correlation of these data and "myriads of other factors" in the testing of hypotheses according to rules recognized in all science. Correlation of masses of data can be *reliably* carried out only through statistical methods. It is precisely here that scientific methods differ from those of Mr. Zucker and other historians. Historians also engage in correlation—informally, usually of cases selected to fit the theory with which they start, with no inconvenient questions of sampling or probability to mar the author's literary style. As such, their work provides valuable hypotheses for science. To regard these conclusions as more than

hypotheses is to confuse hypotheses and laws.

I invite any scientist to scrutinize the 1700 pages of this work to see if he can find a single proposition that he recognizes as a scientific law of the kind he credits in the other sciences. The reader may also meditate on Mr. Zucker's understanding of the nature and requirements of scientific generalizations as revealed in the following statement:

> There has never yet been a graph scientifically constructed which forecast exactly [*sic*] the number of deaths for a certain year. If guesses have been correct, they have been nothing more than lucky guesses (p. 585).

In short, we have in these volumes another attempted short cut to social science. The model and the method are those of Marx. Admirable as these may be in their own way they differ sharply from those of science. The author has himself inadvertently suggested one of the main differences in the following passage:

> In any event, a great deal depends upon whether we approach its [history's] investigation from the standpoint of causal or statistical law. If looked at from the statistical point of view, it is all a matter of chance, accident, probability, despite all we can do. If considered from the causal standpoint, then no doom is inevitable so long as man exercises some rational power over the unfoldment of the historical processes (pp. 586–587).

In science, and especially in field theory, the "rational power" of man *is itself part of the field and is as subject to the laws governing the field as any other element.* It is precisely this fact that many historians and some who pass for social scientists are not yet prepared to face. Until they are prepared to do so, they had better be modest about their scientific pretensions, especially so far as drawing on the theories of modern physics are concerned.

I am in profound agreement with the author's enthusiasm for the application of science to human affairs and in the applicability of field theory to social phenomena. Unfortunately, both can become realities only through some of the methods he discards. Philosophers of history, including Mr. Zucker, in their scientific aspirations are still in the Natural-History stage of scientific development. In the end they will find that history, as such, will play about the same role in social as it plays in physical science. As in the other sciences, authentic history will itself be inferred from the laws of social behavior established by the accredited methods of science from contemporaneous data, subject to observation and generalization by these methods. It should be said to the author's credit that in some places (e.g., Vol. I, p. 33), at least, he seems to recognize this eventuality. Incidentally, the author devotes considerable space to an attempt to refute John Dewey's argument against the possibility of history as a science and, in my opinion, fails in his refutation.

The work closes on a Marxian note, although here as elsewhere the author shows considerable independence as, for example, in deploring the Russian political system while approving of its economic order. Also, he ventures to differ with official Soviet interpreters of Marx (Vol. I, p. 617). There is in fact much admirable objective analysis in these books, especially of recent and current history. Except for the scientific pretensions of the work, a more favorable review would be justified.

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DEVELOPMENT OF MEDICAL SCHOOLS

Medical Education in the United States before the Civil War. William Frederick Norwood. 478 pp. 1944. $6.00. University of Pennsylvania Press.

"MEDICAL education in the United States, with all its ramification, in the century before the Civil War, constitutes a significant and unique chapter in the social history of the country." This statement, with which this book closes,

expresses a conclusion that is thoroughly sound. The careful and laborious study that has gone into the making of this book throws much light on the development of an important factor in the cultural and social structure of this country. It presents many and, at times, tedious details of the struggles, often tempestuous, through which medical education fought its way. Rivalry, individualism, personal liberty, free enterprise without restraint, and struggles for existence, created many a battle between professors, between faculties and trustees, between rival medical schools and, at times, between the medical profession and the public, when protests against human dissection took the form of riots.

Organized medical education in this country had a dignified and promising beginning in Philadelphia in 1765, and progressed until the end of the 18th century under the leadership of brilliant young men who had studied medicine in Paris, in London, and especially in Edinburgh. By the end of the century four medical schools had been founded in connection with established colleges. These schools have all survived to this day as the medical schools of the University of Pennsylvania (1765), of Columbia University (1768), of Harvard University (1783), and of Dartmouth College (1798). Anatomical teaching, lecture courses, and teaching hospitals were planned and developed.

Soon after the turn of the 19th century an American system of medical education began to develop and to spread widely as social and political order of a sort extended throughout the broad reaches of our land. The general pattern of three years of apprenticeship with a practitioner of medicine, and the attendance on two short annual sessions in a medical school, to attend the same lectures each year, became the accepted requirements for the degree of doctor of medicine. Medical schools sprang up not only in the larger cities, but even in country towns, wherever five or six doctors who had a desire to teach could hold themselves together to form a faculty. These schools usually formed some loose attachment with an established college, often at a distance, and, in one notable example, in another state.

A number of influential teachers were itinerants and traveled about to form new schools or to join faculties that invited them from a distance. Some of them had a strong and lasting influence on American medicine. However, as standards of graduation were generally ill-defined, as the support of the schools, and indeed of the professors, depended upon the number of students that could be attracted, and as no licensing examinations were in force, American medicine was largely dependent on the natural ability, sincerity, honesty, and energy of those who practiced it. Fortunately these traits were by no means uncommon, though many practitioners without these qualities were turned loose on the public. American medical education during the 19th century up to the Civil War, as described in much detail in this book, does not present a phase of our history in which we can take much pride.

To the student of medical history, it is of interest to see fundamental improvements being made in scattered places, especially in the West and South. A notable innovation occurred in 1850 when the newly organized University of Michigan put its medical professors on a salary basis, similar to that of other University professors, and relieved them of their dependence on student fees. In 1857 the New Orleans School of Medicine introduced a system of individual clinical instruction of its students and gave them the opportunity to study the patient at firsthand. In 1859 Dr. N. S. Davis, who had taken the leading part in forming the American Medical Association about ten years previously, or-

ganized a two-year graded curriculum in the Medical Department of Lind University, Chicago. Thus, two distinct years of training were begun, instead of having students repeat the same course in two successive years.

The outstanding characteristic of this book is the thorough and complete historical study which lies behind it, making it a source of information on the development of medical education in the United States. Although much of the book is taken up with details of defunct medical schools that are not likely to interest the general reader, or even those especially concerned with American history, it is a useful reference book with much permanent value.

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APOSTLE OF THE WILD

Son of the Wilderness: The Life of John Muir. Linnie Marsh Wolfe. 364 pp. Illus. 1945. $3.50. Alfred A. Knopf, New York.

There is no more beloved name in the annals of the American conservation movement than John Muir. He was the evangelist of that movement, a crusader against man's wanton destruction of natural resources and beauty. He was a great naturalist, a pioneer geologist, a botanist, a lover of mountains and trees, more at home among the haunts of the water ouzel and wild sheep or in some alpine glacier meadow than in the "false society of men." Yet he lived among men too, raised a family, made many undying friendships, and left the world a better place. But for no one did he ever compromise his principles or fail to fight relentlessly for what he knew was right. He was, as his friend Bailey Millard said, as "wild as a Modoc and as unafraid as a grizzly."

All these characterizations, and dozens more that might be listed, make the story of John Muir's life anything but dull. His story has been told before—much of it in his own writings, which have become classic (*The Story of My Boyhood and Youth, Travels in Alaska, My First Summer in the Sierra,* etc.). The two-volume "definitive" biography, *The Life and Letters of John Muir,* by William Frederic Badé, appeared in 1923–24. But it is good to have Mrs. Wolfe tell it afresh. This author's interest in Muir, fortunately, is not a fly-by-night affair. She has been a student of Muir for many years, and in 1937 she edited a delightful book called *John of the Mountains,* made up of some of Muir's until then unpublished journals. Furthermore, she has had access to much new material, chiefly notes and letters and other Muiriana turned over to her by the Muir family.

John Muir was a many-sided individual; he lived a full and strenuous life. Some phases of his career would themselves fill large books. Mrs. Wolfe has done a good job of condensation of material and has produced an unusually well-rounded picture, emphasizing those things that show John Muir the man and setting straight a few matters that she believes have been misrepresented. Here is her thumbnail sketch of Muir:

"Far from being an effeminate plaster saint, all sweetness and light, as some of his admirers have conceived him, he was in truth red-blooded and intensely masculine; a mystic, yet a realist with his feet on the ground; a lover of solitude, yet gregarious and often a prey to bitter loneliness; frugal in supplying his own needs, but lavishly generous to others; a man of puckish humor and of stern, dour moods. Infinitely gentle and understanding in his friendships, and towards the young, the old, and all defenseless creatures, he was blazingly intolerant of bigotry and every form of social callousness. Although mellowed in maturity and more humorous in his judgments, he was still a fighter, an archenemy of all encroachments in the name of 'progress' upon human or animal rights."

In many ways the book is an expansion of this paragraph, and John Muir emerges as a great man. No one, it seems to me, could read it without gaining the highest regard for his genius, a new appreciation for what he did to save Yosemite, Kings Canyon, and other wilderness areas of America for the people, and a deep and lasting admiration for him. To me, some of the most appealing episodes of his story are those pertaining to his friendships—his meetings with Ralph Waldo Emerson and John Burroughs, kindred but contrasting spirits; his camping out with Theodore Roosevelt; his relations with Edward Henry Harriman; his love for the wonderful dog Stickeen; and many others.

The vitalizing thing about John Muir was his boundless and (to his friends) sometimes embarrassing enthusiasm. This was so ingrained in his nature as to be entirely unselfconscious. It was the urge that drove him time and again back to the mountains, that sent him traveling to the far corners of the earth, that kept him fighting and writing to the end of his life. He was a man who could be perennially renewed in spirit by communion with the living wilderness, who found fresh springs of loveliness and wonder with every recontact with nature. To read the life of such a man—so out of this world and yet so dependent on it—is a memorable experience and one that few biographies render. The book should appeal to many classes of readers—scientists, naturalists, conservationists, nature writers and poets, and especially to a newer generation of citizens, who may remember John Muir only by name but who are now enjoying more than they know the fruits of his persistent struggle to save vast areas of incomparable American landscape from devastation.

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GROWTH FACTORS

Bioenergetics and Growth. Samuel Brody. 1023 pp. Illus. 1945. $8.50. Reinhold Publishing Corporation, New York.

THIS book presents in detail the results of the researches sponsored by the Herman Frasch Foundation for Research in Agricultural Chemistry at the Missouri Agricultural Experiment Station.

Very few technical books contain sufficient experimental data and scientific developments to warrant a useful life of more than a decade. In this respect Dr. Brody's book is an exception to the general run of books. It is seldom that a book is published that will appeal to as many different fields as this one will. I bought the book for the chapters on aging, but found the book so well-written and so full of biochemical data that I read the entire book. The first part of the book (17 chapters) consists of a detailed, accurate, and careful treatment of experimental biochemistry, nutrition, and the biodynamics of life processes related primarily to farm animals but also containing a large amount of data on human problems. The chapters on hormones, vitamins, and enzymes are very complete. Throughout the book the kinetic approach and utilization of data are stressed. Dr. Brody has emphasized the economics of his work and the relation to agriculture in all of the material covered. The application of thermodynamics, as well as kinetics, to the interpretation of biological systems is to be advocated. The careful analyses of energy relationships will be of interest to the layman as well as the scientist.

The two chapters on aging are of especial interest to the workers in that field because they illustrate very well the fact that aging is the end product of two competing chemical rate processes in the body. The wealth of data on aging rates and the variation of physiological functions with aging are worth the price of the book.

The last part of the book (6 chapters) covers fundamental economic aspects of animal husbandry. Throughout the book the efficiency of various processes is emphasized and analyzed from the standpoint of maximum production, long-term production, and cost relationships. I have never examined a book of this size before that contained a greater wealth of experimental data in the form of graphs and tables. Some of the statistical methods used are given in detail and the mathematical results are developed from an experimental basis in many cases. The impression is given that the reader is following the problems discussed along with Dr. Brody and his associates during the years of work. The book is well illustrated with pictures. The make-up of the book is good and it is well bound.

Only a few typographical errors were noticed. The ones found were: p. 113, chlorophyll differs from hemoglobin in having different groups on the pyrrole rings as well as in the central metallic atom; p. 127, the formula for choline is in the wrong order; p. 137, oxidation and reduction may be defined as the loss and gain of electrons, but as a chemist, I cannot agree to the statement ". . . or loss and gain of protons;" p. 138, 924, the statement that fat is converted to carbohydrate in the body is disputed (The reference is old (1920) and has not been verified;) p. 144, the formula for 2-thiouracil is not quite correct; p. 150, parallel is misspelled; p. 158, typographical error, *only ln;* p. 283, the first graph should be labeled 11.11 instead of 11.11a; p. 926, in note 69, amphetamine is misspelled. Other minor errors will probably be found in time.

In the summarizing chapter available sources of energy are discussed from the standpoint of fuels. Atomic energy is practically discounted. Although wartime secrecy was in force when the book was finished (Nov. 1944), sufficient information had been published by 1940 to indicate the early development of atomic energy. Instead of having enough U235 for bombs in 1945, we would probably have had only enough to run an experimental power plant, but its early utilization was expected by many scientists. The omission of this point by Dr. Brody is a matter of opinion and should not be considered as a serious criticism.

This book is recommended to graduate students, investigators, and field workers in the fields of animal husbandry, biochemistry, nutrition, and aging. It will be considered a standard reference book for many years to come. Dr. Brody and the Herman Frasch Foundation are to be highly commended upon an excellent job accomplished.

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COMMENTS AND CRITICISMS

MORE ON ETHICOGENESIS

SCIENTIFIC ETHICS

I make no apology for attempting a further contribution on this subject. The atomic bomb underlines the significance of the discussion which has recently taken place in the pages of THE SCIENTIFIC MONTHLY. Science has given to the world an instrument of stupendous power. Thereby comes a challenge. Will science provide us with the means to control that instrument in the interest of humanity or is it to become a Frankenstein monster to encompass the destruction of its creators? Are scientists concerned with ethical questions, or is this the preserve of the philosophers? Whatever the answer, scientists are also human beings and as such have a vital interest in the ethical question whether it is to be solved by scientific or philosophic method.

Right at the outset of our discussion I think we should try to break down the exclusiveness of science and philosophy. Dr. Leake contrasts the descriptive thinking of science with the normative approach of philosophy with its "pure reason." Dr. Romanell pertinently explains that this applies to aim rather than method, but I think there is more to it than this. Probably most people would agree that philosophy has tended unduly to armchair thinking inadequately checked at the descriptive level while science has ignored the very existence of certain fundamental problems. The basic distinction, however, is wideness of aim. We have had sciences in the plural but there can be only one philosophy. True there may be rival claimants for the honor but that is a different matter. The various sciences may be equally acceptable and mutually compatible. For the most part, however, it has been impossible to judge this compatibility owing to the exclusive nature of the fields. But of late years there has been a tendency for some of the sciences to overflow their boundaries and link up with others, as in the case of chemistry and physics. We may expect more and more of this with an increasing tendency towards the building up of one integrated science.

It is this movement towards integration which leads the scientist into the field of philosophy. What is to come of it? Will the new science supersede the old philosophy, or will it fall short of philosophy in respect of aim or method?

Let us consider method first. Essentially there is only one method to develop understanding. It consists in gaining data and seeing its interrelationships. We are continually getting data through our senses but sensations mean nothing until they are interpreted. When Kohler's ape broke a branch from a tree to use as a stick to draw bananas into his cage, it was the result not merely of his seeing the tree or the branch but seeing the relation of the piece of branch to the no-stick-to-drag-in-banana situation. Now the charge against the philosophers is surely that they have gone on weaving a web of these relationships until they have achieved a comprehensive theory which solves their philosophical problems but has not kept in touch with objective data en route. Scientists, on the other hand, while making a great show of meticulous attention to objective details have failed to notice that they have also made some sweeping assumptions as the basis of their interpretation.

The upshot of all this would seem to be that the time has come for scientists and philosophers to develop a greater degree of cooperation.

Secondly, we have to consider aim. Romanell opposes the existential of science to the normative of ethics and finds that philosophy is aiming at "wisdom" whereas science aims at "knowledge." Presumably no matter how wide may become the field of scientific knowledge it fails to equate itself to

the "wisdom" at which philosophy aims. This latter is concerned not with what is but with what ought to be.

Here seems to be the real crux of the problem. Here is the ground for asserting an essential difference between science and philosophy. Here we become faced again with the problem of what does constitute a basis for the laws of ethics.

This distinction as between the existential and the normative would seem to imply that ethical data is in some way less real than that of science. It does not really exist. It is just a sort of plan of what might and should exist. It is concerned with laws which derive their validity from something we know not what, whereas the laws of science are such in their own right!

It seems to me that the behavior of man through the centuries reveals a deep-seated resentment against this. He has strived in various ways to justify the fundamental claims of moral law. All this may be explicable in terms of psychoanalysis, or maybe psychoanalysis itself uses assumptions which equally need explaining. Any psychology must have some theory of motivation. With regard to human conduct we can justifiably ask "why?" in a sense unknown to the scientist as such. You may ask the scientist why it is raining but his answer is almost certain to be in terms of "how." He will trace for you a set of relationships in time and space, but when mother asks Tommy why he stole the jam, she is not satisfied with an explanation of this kind. Ultimately the answer to "why?" must be in subjective terms. A man doesn't really make love because it is conducive to the welfare of the species; he does it for the unique satisfaction which it gives him. He does it because the activity has value for him and value is something experienced only subjectively.

I would suggest that all our human behavior, or at least what we regard as the "higher" forms of behavior, are determined by a set of values of this kind. These values, I would hold, are just as real and fundamental as the qualities which we assign to matter. After all, matter is just the common-sense interpretation of our experience. Our evidence for value is of the same nature as our evidence for electricity, and perhaps even more direct.

If we can accept value in this sense, we shall not be called on to explain ethics as based upon some attenuated theory of reasoning. No one can be motivated by pure logic; it must always strike home to some value complex if it is to influence behavior. We all feel the truth of that in some way but difficulties arise because we think of human desires as essentially selfish and often at variance with what we regard as good and right. We are thus led to look for an ethical standard which transcends human desires. The biologist naturally tends to think of this higher standard in terms of racial welfare, evolutionary progress, increasing survival value. He fails to notice that, if matter is subject to evolution, value may be subject to the same process. More especially he fails to notice that this concept of value, far from emphasizing the selfishness of human beings, provides the bridge by which individual action may lead to group welfare. The behavior which value motivates is not necessarily that which will further individual welfare. It may dictate the sacrifice of the individual for the benefit of others.

Scientists such as Leake make the fundamental mistake of thinking that individual welfare is all-important to the individual in its own right. Not mere living is the important thing but the experiencing of values, and we might have the paradoxical state of affairs where dying provides the greater experience of value. But notice that individuals so constituted would hardly live long enough to reproduce their kind. Evolving values must be, on the whole, conducive to the survival of the individual and the race so that values do subserve racial continuance, and thus Leake's criterion seems to be justified. We have just introduced a further term. But have we? Might it not be more logical to assert that we continue life in order to experience value than to say that we experience value in order

to continue life? For we cannot legitimately talk in terms of purpose and desirability until value exists, and the fact that value promotes the continuance of life cannot be used to prove that life itself is of greater value than the value which informs it.

All this may seem at first rather confusing and, apart from the paradox, might be expressed more clearly, but once the nature of value is understood all the rest follows.

Finally let us examine in more detail the problem of the individual acting in the interests of the group at the cost of his own welfare. This is the rock on which so many ethical systems have foundered. Some systems have established a satisfactory goal in terms of group welfare but have been quite unable to provide any adequate motive for its pursuit; others have found adequate motivation but have been unable to direct it beyond enlightened selfishness. Morality is surely something more than the latter. The concept of value as I have propounded it here allows us to exalt ethical principles to a place of honor without any need for mystical derivation of the authority for moral law. Moral law, like physical law, is implicit in the evolving universe and needs to be derived neither from the personal commands of a deity nor the logic of the scientists and philosophers. Logic may indeed be used in its discovery, but this is rather different from invention, which is more nearly the role that many scientists would seem to think necessary.

All life processes are necessarily associated with value. That which conduces to the survival of the individual, by a process of evolution rather than of logic, be it noted, tends to be linked with value, so that what individuals wish to do is usually in their own interests. But that which tends to race survival is necessarily even more certainly linked with value, so that in case of value conflict, which is very frequent in some form, the racial values will tend to predominate over the individual values. So individuals are able to transcend what we might term their inherent selfishness. We are capable of a divine nobility.

I should like to pursue my theme into some of its many ramifications but I will content myself with one more effort to drive home my fundamental point. I wish to emphasize that value as I have spoken of it here exists in its own right. It is one of the fundamental givens. Certain laws of thought can be denied only if we assume their truth in the argument by which we deny them! Value would seem to be in a similar position but in a more subtle way. Within the realm of conduct we take value for granted whether we recognize it explicitly or not. But if we do recognize it explicitly as a fundamental it provides us with a key to unlock our ethical puzzles. We can proceed to study the evolution and integration of values and thereby explain the normative problems in terms of a wider existential basis.

C. J. ADCOCK

A SCIENTIFIC VERSUS A METAPHYSICAL APPROACH TO ETHICS

The response to my provisional attempt at "ethicogenesis" in the April SCIENTIFIC MONTHLY has been startling and stimulating. It ranges from querulous versifying to Professor Patrick Romanell's patient and helpful philosophical analysis in the October SCIENTIFIC MONTHLY. Most gratifying has been the impression that it may be possible to promote a cooperative endeavor between interested philosophers and scientists in exploring a proposed scientific approach to ethics.

While many professional philosophers are frankly skeptical of approaching ethics in a scientific manner, there are apparently some who are interested, especially in attempting a study of the consequences of the evolution of interhuman adaptations. On the other hand, many seem to insist on retaining an *a priori* metaphysics (often supernaturalistic) as a basis for a theory of ethics. Even while admitting that the principles of ethical theorizing may be as scientifically developed as in physical theorizing, they claim that the subject matter to be dealt with is not of the

same sort. The "primitive facts" for ethics are held to be individual judgments such as, "This act is wrong" or "This man is evil" or "This ought to be done." It is the scientist's contention, however, that the primitive facts for ethics are the same as those with which scientists usually deal; namely, the elucidation of the consequences of action (or energy exchanges) on the basis of our objectively verifiable and voluntarily agreed upon knowledge of ourselves and our environment.

The resolution of the difficulty may occur through careful consideration by philosophers of what demonstrable knowledge we now possess of the mechanisms of brain activity. Instead of continuing to keep their heads ostrich-like in the barren sands of centuries of speculation on the semantic mysteries of "mind," some of our philosophers might profit from a careful reading of current neurophysiology.

While there is fair general appreciation of the work of Pavlov, Head, Sherrington, Cannon, and Herrick, there is little evidence of significance of the work of these men in the writings of the majority of current philosophers. One would think that philosophers would base their speculative ideas on voluntarily agreed upon and objectively demonstrable facts. It may be too much to ask philosophers to keep abreast of current work in neurophysiology, but at least one might expect them to become acquainted with the classics in the field. It should not be expecting too much to ask them to familiarize themselves with recent summaries, such as those of Tilney, Krieg, and Fulton. Philosophers might also derive much interesting but inconclusive information regarding the chemistry of the brain from Page's book by that name.

As study of the brain proceeds, scientists expect the disappearance of the notion of "mind" as something supernatural and thus different from the possible mechanisms of matter. Now that the expenditure of two billions of dollars has resulted in the awful control of the release of atomic energy, with the possibility of a controlled peace, it might be wise for us to devote at least an equal sum to the study of energy transformations in the brain. We might thus learn what limitations there may be to the validity of our thought.

With reference to Professor Romanell's "reply," let it be clear at once that it is generally agreed that philosophers have the responsibility of developing our rapidly growing scientific knowledge into such "faiths" or "syntheses" or "wisdom" as may be satisfying from time to time to our yearning to understand "what it's all about." In undertaking this important responsibility, however, it seems necessary that philosophers keep within the limits of what our growing scientific knowledge shows to be possible or probable.

In a tender discussion of "Mathematics as an Intercultural Bridge," Arnold Dresden ably refutes the common notion as expressed by James Darmesteter that "science equips man, but does not guide him . . . it is invincible, but indifferent, neutral, unmoral." Scientists may strive to be neutral, but never indifferent nor unmoral. Dresden shows that the basic mathematical principles of inversion, of the order in which operations are performed, and of existence theorems are of profound moral significance, under conditions of reasonable association. Similarly other scientific endeavors in physics, chemistry, and especially in biology may be shown to have significant moral implications under the same conditions.

Spencer indicated these contributions of science to ethics, as did Leslie Stephens, W. R. Sorley, A. E. Taylor, and George Gore in a previous generation, and as Dewey and his pupils, R. B. Perry, Conklin, Herrick, Holmes, Cannon, Waddington, Julian Huxley, and others are doing now. It seems prerequisite to a discussion of what constitutes right or wrong conduct to elucidate, on the principle of inversion, all possible consequences of action, whether particular or general. Further, for such a discussion it appears necessary, on the basis of an existence theorem, to determine what conditions are essential or sufficient for a desired end or

purpose. These prerequisites can be met in part by the efforts of scientists. The synthesis or coordination of agreed upon scientific data, with reference to a general problem or proposition, is the business of philosophers.

The Greeks appear to have recognized the trinitarian relations of logic, aesthetics, and ethics. They emphasized the necessity of knowing the "truth" about a matter before purposes can be chosen, or techniques used to apply knowledge to the accomplishment of purposes. The job of scientists is to obtain "truth"; of philosophers and statesmen, to choose appropriate and possible purposes, and of artists and engineers, to apply knowledge to the accomplishment of purposes.

Professor Romanell pays me a great compliment in so carefully analyzing my effort to induce a naturally operating ethical principle. His analysis reveals many points at which I failed to make myself clear. Thus, I had hoped that my definition of the general procedure of scientists would have indicated the importance of the experimental endeavor as well as of the descriptive approach. My emphasis on the latter was made in contrast to the normative approach characteristic of classical ethics. My purpose was to suggest the possible importance of a scientific approach in ethics, particularly from the standpoint of elucidating the consequences of actions and the conditions necessary or sufficient for the accomplishment of purposes.

It is not clear that ethics by definition is essentially a normative pursuit. Ethics is concerned with the problem of goodness, which is a matter of conduct, and thus of relations between people. Sociology, anthropology, psychiatry, psychology, and thus in a broad way, physiology and biology, become significant scientific disciplines for ethics. Nor are the raw data for ethics merely the judgments of different persons as to what is right or wrong, or what ought or ought not to be done, as Ducasse implies. The various factors conditioning these normative concepts are perhaps even more important as data for ethics. Such factors are elucidated by scientific effort.

Professor Romanell says that a normative inquiry is concerned mainly with studying objects as they ought to be. Is it not pertinent first to explore, as in an existence theorem, what is possible for these objects of study to be? Such an exploration may proceed with greater surety by scientific methods rather than by speculative. If "ethics is an examination of the *good* life," then it must also be an examination of the factors in life which are conducive to the good. Such an examination would surely profit from a scientific approach, both descriptive and experimental.

There seems to be some confusion in Professor Romanell's logic regarding existential and normative inquiries. If a normative inquiry is concerned with what ought to be, then it certainly involves a method of choosing what ought to be. If this method is speculative or metaphysical, as in classical ethics, then it may fall to pieces by failing to recognize the impossible. If the method is scientific, one proceeds, first, by knowing what is, and then, second, by knowing the consequences of what is. When the consequences are known, an adaptive or rational choice may then properly be made with respect to the purposes intended. Rationality in choice involves appropriateness of adaptation to the end desired. The normative inquiry involves, as Romanell implies, choice of purpose. However, in order to have any validity, that choice must be based on what we know is possible. It seems, therefore, that there is a normative method in ethics, which, if not properly replaceable via scientific method, might at least well be supplemented by one. To this, I think, Professor Romanell may agree.

To the scientist it is irrelevant or meaningless to discuss in general how people ought or ought not to act. The scientist proceeds by finding, through verifiable experiment, the particular conditions necessary to be met in order to achieve some particular end. Our effort to generalize this proposition results in the tentative formulation of an operative natural principle, inducible from the plethora of common experience, to the effect that, if

the end to be reached is a lasting, satisfying relationship between individuals or groups of individuals, the condition necessary to be met is that the relationship must become mutually satisfying. It is perhaps significant that none of the many critics of my essay on ethicogenesis make a systematic attack on the attempted formulation of a naturally operative ethical principle.

Professor Romanell is also confusing in his discussion of the relation of the real to the ideal. If the ideal is distinguished from the real, then its unreality is admitted, and its validity becomes questionable. Is it not possible to place the concept of the ideal in appropriate scientific reference by considering it to be an asymptotic limit, never to be reached, and therefore valid merely as an asymptote?

It is regrettable that I should have implied to Professor Romanell that philosophy is dead, when I suggested that perhaps metaphysics has no further meaning in view of our present knowledge of ourselves and our environment. It would seem, in the light of current scientific developments, that philosophy might advance more rapidly and effectively without metaphysical impedimenta by deriving justifiable generalizations from the demonstrable scientific knowledge we now possess of ourselves and our environment and by elucidating the consequences thereof.

Professor Romanell might be under much strain if called upon to justify claims for an inverse relation between certainty and depth of knowledge, or between detail and significance of knowledge. If he was thinking of the Heisenberg uncertainty principle when he thus dogmatized, he was unfortunately led into a false and dangerous analogy.

Many philosophers make much of the notion that an organism is more than the sum of its parts. This is perhaps not a fair way to state the proposition. There is the assumption that an organism appears to be more than the full sum of all of its parts and their respective potentialities. Some of the available data on biological levels of organization have been summarized by A. E. Emerson and R. W. Gerard. The factor of time in the organization of living things may also be explored scientifically as du Nouy showed, with more than a hint of metaphysical skill.

There is nothing occult, metaphysical, or supernatural about the concept implied by the word "wisdom." Neurologists indicate that richness of sense experience may be readily reflected in richness of neuron association. Similarly "vision" and "understanding" are symbols referring to the ease and richness of neuron association in the brain in bringing together appropriate neuron association pathways built from the richness of sense perception and experience. By reference indeed to the degree of universality of neuron association pathways in different individuals as manifested by similarity of reaction to similar sense experiences, one may apply the canons of science to philosophical propositions. Those which may be refuted by this method may properly be termed metaphysical irrelevance.

Professor Romanell apparently does not differentiate between philosophy and metaphysics or between metaphysics and science. To the scientist, however, it seems that metaphysics has no validity because it has no basis in the objectively verifiable or demonstrable. This is to say that it has no basis in scientific knowledge, and hence no present reality. Philosophy has a valid and significant function in coordinating scientific knowledge and elucidating the consequences thereof. An attempt was made to define scientific endeavor in the article discussed by Professor Romanell. By metaphysics is generally meant that which is beyond the physical. The scientist can find nothing there, and hence can't believe in it. In my discussion here I am using "metaphysics" as a symbol for all supernatural explanations or speculations about phenomena, and as a general symbol for such presumed nonphysical or nonmaterial, but allegedly real, concepts as "mind" or "soul."

It is from this position that the scientist rejects metaphysical irrelevance in attempting an approach to ethics. As Professor Romanell proceeds in his discussion of the

interrelations of science and philosophy it would seem that scientists generally would be in sympathetic agreement with him. The scientist holds, however, that the "truth" can best be obtained by the scientific method and that metaphysical considerations introduce unnecessary nonverifiable speculations and semantically confusing concepts.

In his critique of my Darwinian argument in connection with a naturally operative ethical principle, Professor Romanell reveals again my failure to make myself clear. I attempted to emphasize the significance of adaptation in the Darwinian exposition of evolution. Darwin realized his error in referring to a "struggle for existence." It was this phrase which Nietzsche exploited and against which Samuel Butler, Edmund Montgomery, Henri Bergson, Alfred Noyes, and Jacques Barzun so vigorously reacted. It was Thomas Huxley who unfortunately popularized it and himself revolted from it. But Charles Darwin did not stress the struggle for existence nor "nature red with tooth and claw," nor the "gladiatorial theory of existence." Darwin did emphasize adaptation to environmental circumstances as a necessary condition for survival. It is this aspect of Darwinian evolutionary theory which is considered to be significant ethically, and which I think he so regarded.

An attempt was made previously to be explicit regarding the implications of Darwinian factors in survival, but apparently Professor Romanell preferred to follow the usual but rather erroneous ideas associated with Darwin's thesis. His strictures against my point of view are therefore slightly irrelevant, but they are helpful in proposing a clarification of the matter. It is quite unfair to Darwin to imply that Nietzsche's iron rule is part of Darwinian ethics. Darwin's contribution to ethics was essentially to offer biological evidence in support of the Aristotelian harmony theory of ethics. This, as has been later developed, appears to be a mater of adaptation, adjustment, and compromise toward what may become mutually satisfying.

My very minor contribution is simply to offer expression of the conditions under which relations between human individuals or groups may be expected to survive. This expression gives a scientific basis for choice of conduct, and thus for ethics. From the plethora of universal experience, this general principle may be induced: Behavior patterns between individual persons or groups of people tend to become adjusted (by trial and miss) toward those which yield the greatest mutual satisfaction. It seems that the concept of "good" arises from developing experience with the more satisfying behavior pattern.

The principle may perhaps be more scientifically expressed in a manner explicitly stating the conditions under which relationships between people may be expected to survive: The probability of survival of a relationship between individual humans or groups of humans increases with the extent to which that relationship is mutually satisfying. As a practical matter, this principle may be worth much attention and appropriate application in dealing with our Russian ally. The Russians are perhaps most likely of all peoples to appreciate the natural operation of the principle.

To Professor Romanell's inspirational closing it may be asked, If all good people sacrifice themselves, what happens then? We can't all be Jesus Christs, John Husses, Giordano Brunos, Miguel Servetuses, or even Marines. Some of us have to go on living ordinary, commonplace, everyday lives. Perhaps it even takes more heroism to face the dull grind of routine, humdrum daily life with equanimity and dignity than to ride the winds of fancy snatching at what seems to be fire in hope of being another Prometheus.

Let it be hoped that this discussion may be extended to a serious effort on the part of philosophers and scientists to understand each other more thoroughly. Scientists certainly have much to learn from philosophers, and it seems that philosophers might profit greatly by becoming more fully acquainted with current scientific developments, par-

ticularly in biology, neurophysiology, psychology, psychiatry, anthropology, and sociology. The efforts of scientists in these fields may supply significant and important data not only for a sound ethic, but also for a satisfying general philosophy.

CHAUNCEY D. LEAKE

CRACKING THE CONCEPT

The wish to live in a more decent world is felt by man without the least effort on his part. The preference for better conditions in the presence of inferior ones is inborn in man. This is what John Dewey has in mind when he remarks in his *Human Nature and Conduct:* ". . . the ideal is itself the product of discontent with conditions." But this wished-for better world will not become a reality without our effort—a long, sustained, concerted effort.

However, in order that man may be persuaded to put forth the intense effort required to change chaos into order, he *must* feel that he has the necessary stature for the assignment, at least the potentialities.

The man Christian philosophy looked at had the required stature for the task. But the Christian concept of man has disappeared from academic circles.

The naked statement that the Christian concept of man has vanished from Academia may be shocking to some. But this is what Mortimer Adler is trying to tell us when he writes a whole book on what man has done to man: when he writes on what psychoanalysts, psychologists, psychiatrists have done to man's concept of himself.

Let us see how much the modern educated man has wandered away from the Christian concept of himself. According to the Christian philosophy, man's reason—"the soul's summit," as Thomas Aquinas called it—has a higher hierarchy than the rest of man's hungers. The rest of his hungers are those which he has in common with the beasts. This is so definitely true that the entire Christian concept of freedom rests squarely upon the Christian concept of reason.

Hierarchy, however, is a concept alien to the so-called scientific approach. Let us look at this reality through Professor Stace's book, *The Destiny of Western Man.* I choose this book because it received the sole award of $2,500 in a contest intended primarily for educators, and because the committee which made the award was composed of prominent men. One of them was Carl Van Doren, who called it "a book of world-wide significance, sure to clarify and fortify contemporary opinion and to leave its mark on years to come."

Says Professor Stace on page 93 of his book: "The Greeks, therefore, had in general no right to their belief that man is superior to the other animals. . . . And therefore, we cannot admit the validity of that argument in favor of the *primacy of reason* which bases itself upon man's superiority to the rest of creation." (Italics mine.)

However, unless the concept of the primacy of reason is recovered, even John Dewey is meaningless in education: "The ideal aim of education," Dewey says, "is the creation of power of self-control." Self-control, we are tempted to ask Dewey's pupils, is control of what over what? What is self identified with? Hierarchy is implied in the term self-control. Or else the term is meaningless.

That the term self-control is meaningless at present is revealed by the unsubstantial, makeshift character of our education. Quoting "someone," Professor Frederick G. Nichols of Harvard wrote in 1942:

> About 1900 in the University of Iowa a teacher took a hen into the class and, while this was a good deal of an innovation, it was simply a hen. About 1910 this hen had become a "problem." About 1915 it had become a "project." About 1919 this hen was a "unit of work." Around 1925 it was an "activity." In 1930 it became the basis of an "integrated program." And lo! In 1936 this poor hen has become a "frame of reference."

In our age two philosophers—especially two—have taken the warpath to recapture the Christian concept of man: Jacques Maritain and Mortimer Adler. They are trying to recapture the concept via Saint Thomas

Aquinas. They have taken the silent, solemn road untrodden by modern specialists. I, for one, would like to see them come out victorious in their daring adventure, but I do not even dare hope for their victory. Borrowing Korzybski's terminology, one would say that our age has become so "neuro-semantically conditioned" by education as to have become "constitutionally incapable" of understanding such men as Aquinas or Kant. On page 140 of his *Destiny of Western Man* the Princeton Professor says: "And this outward aping of goodness was what poor Kant thought to be the essence of virtue." After reading such a statement where aping of goodness and Kant are uttered in the same breath, and after taking into consideration the authority conferred upon the statement by the award, one feels that both Maritain and Adler had better take notice of the closed-road signs along the solitary path which they have been traveling.

It may appear absurd to philosophers, but in our age of specialization it is not only man's concept of matter which must come from science, but also man's concept of himself.

The much-discussed release of nuclear energy might afford the opportunity for scientists to step forward with a concept of man which will make integration of knowledge possible. Knowledge is disintegrated simply because the concept of man is out of joint. What disintegration of knowledge really means is that it is not culturally mandatory to make good use of knowledge. However, if the scientific concept of man coincided with the Christian concept of man, it would be culturally mandatory for man to make good use of his knowledge. Once man is put together everything else falls into place. And now that the scientists have released nuclear energy, the layman feels that science is responsible for the giant's behavior. And science would be equal to the task of controlling the monster if it had evolved by now a descriptive ethics of its own. Men of science talk and talk about the necessity of a descriptive approach to the subject of morality. Yet they have not given us that descriptive ethics which should replace the normative variety which has been so bantered and discredited.

A descriptive ethics must of course describe man, not his doings. This last would be history. And it cannot take the easy and superficial course of describing man as a "domestic animal." It has to describe the *specifically human* nature of man, not the part which man has in common with the beast. It must take account of this fact in man's nature which was pointed out by Kant: "Everything in nature works according to laws. Rational beings alone have the faculty of acting according to the conception of laws, that is, according to principles. . . ."

At the stage of specialization of our knowledge, to determine what is specifically human in man requires a veritable cracking of the concept of man. This cracking, in its turn, requires a concerted effort of specialists; as much as was required for the atomic bomb.

Let us glance at the following three fields of specialization to realize why the effort must come from many fields:

Psychoanalysis. Here is a school which denies rationality and puts rationalization in the place which rationality leaves empty. If the psychoanalyst is analytic enough, however, he soon discovers that his school cures by the very opposite principle which it proclaims. No psychoanalyst cures, for instance, by providing suitable mates; he cures by providing explanations. And a person who can be cured by explanations must be a highly rational being. An individual for whom to understand is to heal must have a diffuse rationality—one which penetrates and overpowers libido.

While the psychoanalyst is curing by explanations he could jot down for his descriptive ethics: Once reason understands it creates order below. Reason, therefore, must have an immanent power to govern the psychophysical hungers of the self.

Hypnotism. Of course a specialist could remain at the toying stage with hypnotism. But a specialist might heed Hegel's words: "I only know of an object insofar as in it I also learn of myself." If he tries to

learn of himself, of man, in his object or subject he may learn a lot. His experience has shown him, for instance, that under hypnotic or posthypnotic suggestion a man will dispossess himself of all his worldly goods and bequeath them to a stranger. Should the reader be skeptical let him consult Hugo Münsterberg's authoritative *On the Witness Stand.* And in some cases a man under posthypnotic suggestion will even accuse himself of a crime which he has never committed, and thereby land in the electric chair. But a man under hypnotic influence will not violate what to him is a moral principle.

When this something which is stronger than hypnotic suggestion comes to the surface of man's being, the specialist could write down for his descriptive ethics: Man's affinity for a moral principle is stronger than his instinct for self-preservation.

Industrial Psychology. The long experiment conducted at the Western Electric Hawthorne Plant reveals that man as a physical organism does better under bad conditions than under unjust conditions. Only a highly rational being would be affected by unjust conditions which are physically good. And only if rationality has an immanent power over the rest of man's reactions would the above characteristic reveal itself under experimental conditions.

The descriptive ethics which could come from cross-specialization must establish these two facts: (1) Man's rationality, differing not only in degree but in kind from animal intelligence. (2) Reason's immanent power to govern the rest of man's personality.

These two facts must be proved scientifically if democracy is to be differentiated from nazism by something more than military victory. Democracy is a product of a theory of human personality; nazism is also a product of a theory of human personality. What is the personality behind the democratic theory? Well, democracy is a faith that man can live with self-imposed oughts. Only a rational being, of course, could live with self-imposed oughts, and only so if his reason has an immanent power to govern his desires and impulses. Otherwise oughts would have to be externally imposed.

If man's affinity for correct solutions were not potentially stronger than his affinity for the things which satisfy his cravings, democracy would be a dream. Democracy would stand distant and empty—a meaningless Greco-Christian myth.

In writing their descriptive ethics, specialists must not forget that man could have been so constituted that he had an affinity for wrong solutions, or at least that between a right and a wrong solution he would be indifferent. Had that been man's condition, science, philosophy, and democracy would be unknown in the world. Democracy would also be impossible if man did not have a wider scope for perceiving as right the right solution than for discovering it.

Science has steadily rejected normative ethics. But in this hour of crisis, in which science is about to lose its freedom, would not scientists gallantly step forward and give us a truly descriptive human ethics—an ethics that could not possibly be thrown off the saddle by scientific progress?

To match the cracking of the atom there must be a parallel cracking of the concept of man. Each specialist would have to start bombardment within his own field. If he goes at it hard enough he will discover, crouching within his own field of specialization, a man of much larger stature than the one his specialty stands for.

If the bombardment is started from all quarters and the concept is cracked, the release of spiritual energy will be voluminous enough to make physical nuclear energy behave. It might be powerful enough to light the lamps of peace and keep them burning.

ANA MARÍA O'NEILL

Prof. Sumner's Wise Reflections

Permit a mere journalist, retired after fifty years of activity, and now facing extreme old age, to congratulate you on the publication of Prof. Sumner's profound and philosophical article on old age and death. War almost invariably leads to the revival of what some people choose to call religion, but what is in reality crude superstition and naive beliefs supported by no evidence worthy of the name. These efforts are futile and vain, of course, but while they last, they do some harm. Voltaire did not say that those who believe in absurdities are prone to commit atrocities, but he might have said it, for it is true.

The dread of death, as Prof. Sumner—who, by the way, was a fellow La Jollan and almost a neighbor of mine—points out, is either a dread of hell-fire, which is an absurdity, or else a dread of total extinction, and this kind of dread is normal enough. Huxley had it, as he confessed. But we are intelligent beings, for the most part, if we get any education and introduction to the wisdom of the ages, and we learn that extinction is the common lot, except the lot of matter in its simplest form. Human life is tragic, because life abhors the idea of extinction. But life, on the whole, is good, not bad, and it is better to live the average human span than not to live at all.

As for individual or personal immortality, it is what Spencer called a "pseud-idea." No one can imagine the existence of a disembodied soul or spirit. No one can really believe that after death something indescribable and inconceivable leaves the body and continues to float somewhere in space in some other shape or form—doing what, serving what purpose, pray? No one knows. No one *can* know, apparently. Can we take seriously those who tell us, solemnly, that in heaven there are cocktails sans alcohol and tobacco sans nicotine? (Sir Oliver Lodge's "Raymond" for instance?) Prof. Sumner's reflections on this question are criticism-proof.

In recent magazine papers, some writers have taken the odd position that immortality, which cannot be proved, is at least "probable." Why? Because, forsooth, nature cannot afford to waste and lose human personality, the most precious thing there is in the universe. The conservation of matter, or energy, suggests that personality, too, is being conserved. This argument is not even plausible. What becomes of personality in senility, in loss of memory as the result of certain diseases—as in the case of Henrik Ibsen, who had to relearn the alphabet after seventy? What happens to personality in the case of the young who die *before* maturity and full development of their powers? Finally, the best in personality does survive death, not in the shape of a disembodied soul, but in the form of books, pictures, music, buildings. Bach, Beethoven, Dante, Milton, Shakespeare, Kant, are not dead—they live and mold our lives, ennoble and enrich our personalities.

It is to be hoped that Prof. Sumner's brave and candid paper will encourage other men of science to publish their ripe reflections on life, senescence, death, and reasonable ideas of immortality. Men of science are leaving their ivory towers, or their laboratories, and manifesting concern about our pressing and difficult problems, moral, economic, and social. It is their duty and privilege to help build a better world, a world free from superstition, barbarism, and injustice.

VICTOR S. YARROS

Professor Sumner's Unwise Reflections

In the August 1945 SCIENTIFIC MONTHLY the essayist of "A Biologist Reflects Upon Old Age and Death" has certainly ventured far afield from his entitled theme. He has virtually tried to offer a new concept on suicide, and one on the status of religion in the realm of "thinking persons."

With a view to our own laws and moral codes, (which we observe and cherish as American) suicide is an illegal act, is immoral, foolhardy, and cowardly. Incidentally, we certainly should not and cannot evaluate nor judge the Japanese with their national propensity for suicide, by our own laws and moral codes. But the author records, "Suicide, far from being 'cowardly,' as is so often pretended, is for most persons an act of supreme courage. Is it not really cowardice which has prevented most lives from being ended prematurely at moments when conditions seemed intolerable?"

It is disconcerting to observe a "scientist," one who searches for the truth, expounding a self-styled doctrine which, when carefully and impartially analyzed, is neither scientific nor popular.

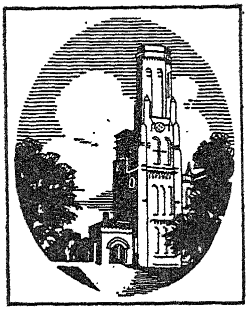
The author's subtle thrust upon religion is the equivalent of an explanation for his neat compilation on suicide.

Otherwise, the orderly expositions on lack of driving power, becoming less impressionable, and the evidence of a failing memory seem to be more germane to the implications of the title of his interesting paper.

The author's inference, as expressed in his footnote, "that scientists as a class are 'tough minded,'" is indeed presumptuous. Any limited circle of friends or acquaintances should not be used as an indiscriminate "yardstick" by anyone.

HARRY MAETH, D.D.S.

THE BROWNSTONE TOWER



ALEXANDER WETMORE, Secretary of the Smithsonian Institution and a distinguished ornithologist, was kind enough to comment on our Christmas story of the owls of the Brownstone Tower. He wrote as follows: "The barn owl of the Smithsonian towers is an old friend of mine, as I have known this bird, and his predecessors in tenancy, since 1912. Actually, the bird has become almost a legend, since Pierre Louis Jouy, who worked at the Institution for a time, records this owl in his list of District species published in 1877, and Coues and Prentiss in 1883 remarked that it 'is known to have nested in the Smithsonian towers'."

In a later conversation Dr. Wetmore told me that the owls nest in the small tower at the northwest corner of our building. From my chair I can see one of the owls' doorways —an open quatrefoil above a pair of narrow windows. One time the quatrefoils were screened without Dr. Wetmore's knowledge. Upon learning that the owls had been dispossessed, Dr. Wetmore ordered the screens removed. The birds soon returned to their rent-free home and, we suppose, have lived there happily ever since.

Another friend who enjoyed the owls added to his compliments: "Wish you would write more about your general philosophy of things." It is a temptation that I shall resist as long as possible, for the SM already contains the wisdom of wiser men than I. If I should attempt to preach from the Tower, I fear that I would be taking unfair advantage of those who do not have such a pulpit, and I might even come to regard myself as an oracle. Therefore I would rather risk the faults of triviality than those of profundity. Let me leave the latter to the owls.

Those birds are not the only fauna of the Tower. I could tell the story of the recumbent roofers, but perhaps I should confine my remarks to the so-called lower forms of life. Take the starlings, for instance (and you're welcome to them). Fortunately, only a few of these birds flutter about our windows, though myriads roost on the ledges of the Archives Building across the Mall. Could it be that our owls are protecting us from these desecraters of monumental buildings? Dr. Wetmore says that starlings are their principal prey in winter.

We have no "Archie" to criticize us. He and a few of his fellow-cockroaches live in the basement of the Smithsonian, where he has assisted Dr. Earl S. Johnston as a subject for tests of a sensitive method for the determination of minute quantities of carbon dioxide. After taking many deep breaths for Dr. Johnston, Archie has returned to his friends and relations, none the worse, we hope, for his exhausting experiences.

In referring so familiarly to insects I hope that I have not offended one of our readers, an engineer, who wrote that he wanted the SM, provided we would not publish any more pictures of bugs. An entomologist tends to forget that cockroaches are almost as repellent as snakes to many people. Perhaps it is safe to write about an insect not so well-known.

One night this winter, while my wife was reading, a hemipterous bug about a centimeter in length walked slowly across her book. This insect was garbed in scarlet and black, the colors of my first college, Haverford. It seemed bewildered on the printed page, so I gently brushed it aside, explaining that it was harmless—only a box-elder bug seeking shelter for the winter. For weeks we saw one bug every night and liked to think of it as the same individual, whom we called Elmer. He was a friendly fellow but finally he took advantage of our tolerance, for he called in "his sisters and his cousins, whom he reckons by the dozens," and we had to take steps to curb the invasion arising from the cellar.

F. L. CAMPBELL

THE SCIENTIFIC MONTHLY

MARCH 1946

JAMES BRYANT CONANT

PRESIDENT OF THE A.A.A.S. FOR 1946

By VANNEVAR BUSH

THE election of James Bryant Conant brings to the presidency of the American Association for the Advancement of Science a leader whose career is convincing demonstration of the effectiveness of scholarship in action. It is no disparagement of Dr. Conant's notable attainments in chemistry and his acumen as a researcher to say that in calling him from his laboratory in 1933 to become its twenty-third president, though it demanded of him great personal sacrifice and though it took from the scientific investigation of photosynthesis a sadly missed pioneering mind, Harvard University did education and the country a great service. For the opportunities and the responsibilities which then became his, he was ready; and as problems in fields far from scholarship and education arose to require his keen analysis and solution, he had already grown to meet them. His university and the nation as a whole are, by consequence, continuing beneficiaries.

Dr. Conant's measure as a statesman of the intellect had been well foreshadowed a number of years before his recent and present preoccupation with the world-relations of the United States as an aftermath of the war. His annual reports as President of Harvard epitomize his driving belief in democratic opportunity in education and his common-sense approach to ways of opening and stabilizing it. Americans are a report-writing people, and the annual output of such documents is therefore staggering in more ways than one. Without exception, Conant's reports have been oases, broad in conception, lofty in aspiration, sound and practical in application. The philosophy of education which they express came to him as part of his New England birthright. It has been matured and consolidated both by experience and by earnest thinking, and in it as thus developed are to be found the basis for Conant's leadership as a citizen and the explanation of the unselfishness with which he has devoted himself to public service.

His recent activity as adviser to the Secretary of State in negotiations looking toward international control of atomic energy illustrates Conant's scholarship in action for the establishment and stabilization of peace among nations. The convictions of the necessity for world concord which have impelled his work for peace are the same convictions which made him a militant interventionist as long as seven years ago and brought from him the first American demand for "unconditional surrender," only two months after the day of Pearl Harbor.

But both in his firm insistence that the United States must carry determined war to an unrelenting conclusion against

Photograph by Bachrach

JAMES BRYANT CONANT

the Axis and in his unstinted collaboration in the endeavor to prevent the recurrence of such a grim necessity, Conant is no summer soldier or sunshine patriot. He knows the harsh bargain that war drives with those who slacken their grip on peace and therefore have to buy it back with blood and treasure. Conant learned this lesson in the first World War, when as a twenty-five-year-old major in the Chemical Warfare Service he directed top-secret work on gas warfare. It fortified him for the heavy demands which the second World War was to impose upon him.

Conant's active concern over the war was voiced as early as 1939, and with the formation of the National Defense Research Committee in June 1940, he went to work in dead earnest, taking responsibility for NDRC's development of his World War I subject—chemical warfare. As the country's war research expanded, the Office of Scientific Research and Development came into being in 1941, and Conant took on the chairmanship of NDRC, which became the central agency in OSRD military research and development, involving the expenditure of many millions of public funds. His contribution to the success of American arms through this work is itself incalculable.

Through most of this period, however, Conant was at the heart of the great program of research, engineering, and administration that brought nuclear fission to practical utility. His manifold other accomplishments through his post in NDRC are equaled in significance by what he did in co-ordinating and spurring the project which reached achievement in New Mexico last July and which a month later put an end to the war. The Smyth report gives in stark outline the history of his participation in this work—the successive reorganizations from the original Uranium Committee through a series of units to the OSRD S–1 Executive Committee under his chairmanship, as well as his contributions to the formation of policy through the so-called Top Policy Group formed in 1941 and the Military Policy Committee which came into being after the prosecution of the work had reached such a stage that the Manhattan District was created and the Army began to assume operating responsibility for the development. Unreported but vivid in the memories of his colleagues—military and civilian—through these years is the record of his rare combination of hard-headed analysis and tactful collaboration in problems that were ever new, ever changing, and charged with vital importance to the country and to men everywhere.

In Conant's personal history up to the war, Harvard is the dominant note. For all but sixteen of his fifty-two years—he was born in Dorchester, Mass., March 26, 1893—as student, teacher, or administrator, he has been in and of it. Prepared at the Roxbury Latin School, he was graduated as a Bachelor of Arts in 1913, and received the Ph.D. and became an instructor in chemistry in 1916. He advanced through the grades to become Sheldon Emery Professor of Organic Chemistry in 1929 and chairman of the department in 1931, holding these posts at the time of his election to the presidency of the University on June 21, 1933. As a chemist, he has worked on the quantitative study of organic reactions, on hemoglobin, on free radicals and superacid solutions, and on chlorophyll. In 1932, Columbia awarded him its Chandler Medal for achievement in chemical science, the subject of his paper being "Equilibria and Rates of Some Organic Reactions." In the same year, the William H. Nichols Medal of the New York Section of the American Chemical Society came to him for his

work on chlorophyll. The gold medal of the American Institute of Chemists, for "noteworthy and outstanding service to the science of chemistry or the profession of chemistry in America" was awarded him in 1934.

Married in 1916 to Grace Thayer Richards, Conant has two sons. His personal interests are in simple things: He is a good fisherman, and used to go skiing on occasion—in fact, he once cracked up a shoulder on a tricky slope, to the dismay of the Harvard Corporation. Thanks to the fact that one of his grandmothers was born in 1800, he is a member of the executive committee of the extremely select Association of Grandchildren of the Eighteenth Century. Good talk is one of his great delights, and he is adept both at drawing out the other fellow and at keeping up his end of a conversation.

He comes to the presidency of the Association at a high point in a distinguished career. With war responsibilities drawing to a close, he will bring to the problems of scholarship and education the vigor of his basic belief in democratic opportunity and the added strength of convictions matured by reflection and observation during this period of stress. It is notable that, with all the pressure of recent years, Conant has made time for thinking and writing on the essential questions of the relation of the scholar and the state, and the ends and means of learning. Moreover, it was he who in the midst of unprecedented and taxing demands, set in motion and followed in operation the fundamental study of educational philosophy and policy which is embodied in the report of the Harvard Committee, *General Education in a Free Society.* The advance of the Association toward its objectives will be spurred by his influence, as other efforts at resolving the perplexities of bettering the minds and therefore the lives of the people have gained by his guidance.

THE SUN MAKES THE WEATHER

I. MEASURING SOLAR VARIATION

By C. G. ABBOT
RESEARCH ASSOCIATE, SMITHSONIAN INSTITUTION

After his term as Speaker of the House of Representatives, "Uncle Joe" Cannon of Illinois came back to the Appropriations Committee, of which he had been a distinguished member. Representative Fitzgerald of Brooklyn was the new chairman of the Committee.

In the midst of the Smithsonian hearing the chairman said: "The next item is the Astrophysical Observatory. What is it? What does it do? What good is it?"

"Mr. Abbot will speak to that, Mr. Chairman," said Secretary Walcott.

I had just begun to tell of our work when the chairman was called from the room. Said Representative Sherley of Kentucky: "This is rather interesting, but I think Fitz would have his troubles if he tried to explain it on the floor of the House."

Old Uncle Joe was walking to and fro, with his cigar tilted high, as the cartoonists liked to draw him. He stopped opposite Sherley and said: "No, Sherley. I recollect when old Professor Langley came to me and said, 'Mr. Cannon, I need $4,000 for the Astrophysical Observatory in order to investigate the infrared spectrum of the sun with the bolometer.' 'My God, Professor,' said I, 'Can't you abolish it?' But no, Sherley, one may forget about the stars that are so far away it takes light a thousand years to come from 'em, and if they were all abolished tonight our great-grandchildren would never know the difference—we can forget the stars, but everything hangs on the sun, Sherley, and it ought to be investigated, and I think this appropriation is all right."

We got the appropriation.

For 25 years we have worked on three problems. How much does the sun's heat that warms the earth change from time to time? How much do these heat changes affect the weather? Can sun-produced weather changes be predicted?

To solve these problems my colleagues of the Smithsonian Institution and I have observed the heat of the sun's rays at 12 widely separated stations ranging from sea level to 14,500 feet in altitude, and with automatic apparatus we have even measured sun heat from balloons at 15 miles' elevation. Many months of daily observation have been spent at each of the 12 stations, except Mount Whitney, and 3 of them, Mount Wilson and Table Mountain in California, and Montezuma in Chile, have been occupied for a score or more of years.

Our daily observations, aimed to re-

CHARLES GREELEY ABBOT
BY C. K. BERRYMAN, WELL-KNOWN CARTOONIST.

cord the variation of the sun, begin at the stations soon after sunrise and last till about 10 A.M. After that the plates must be developed and measured. Then come the computations, which often last far into the night. The best of our stations are on mountaintops in desert lands, thousands of miles from home, where there is neither bird nor beast, plant nor insect, nor any human neighbors within many miles, and where rain rarely falls. At Montezuma, Chile, all food, and even water, is hauled from 12 miles away by the observers themselves.

As a result of this long and strenuous investigation, I believe I know the answers to our three questions.

When I speak of the sun's heat that warms the earth I mean the heat equivalent of those rays which fly through space from the sun to us in 8 minutes, moving 186,000 miles each second. Some of them we can see and we call them white light. Sir Isaac Newton proved that white light is really a combination of the colors of the rainbow. Beyond the violet lie rays invisible to us, which we call ultraviolet; beyond the red are invisible rays which we call infrared. All of these, when they enter substances, such as clouds, water, or soil, produce heat. Thus the sun's rays keep our earth warm enough to live on, and if indeed they alter, the weather should alter too. Radio rays are similar to infrared sunrays except that their wave lengths are much greater. So far as we know, they are not contained in sunrays.

When I ask if the sun's heat that warms the earth changes from time to time, I mean before it reaches our atmosphere, while yet altogether in free space in the form of waves. In other words, is the sun a variable star, as many other stars are? The answer is yes.

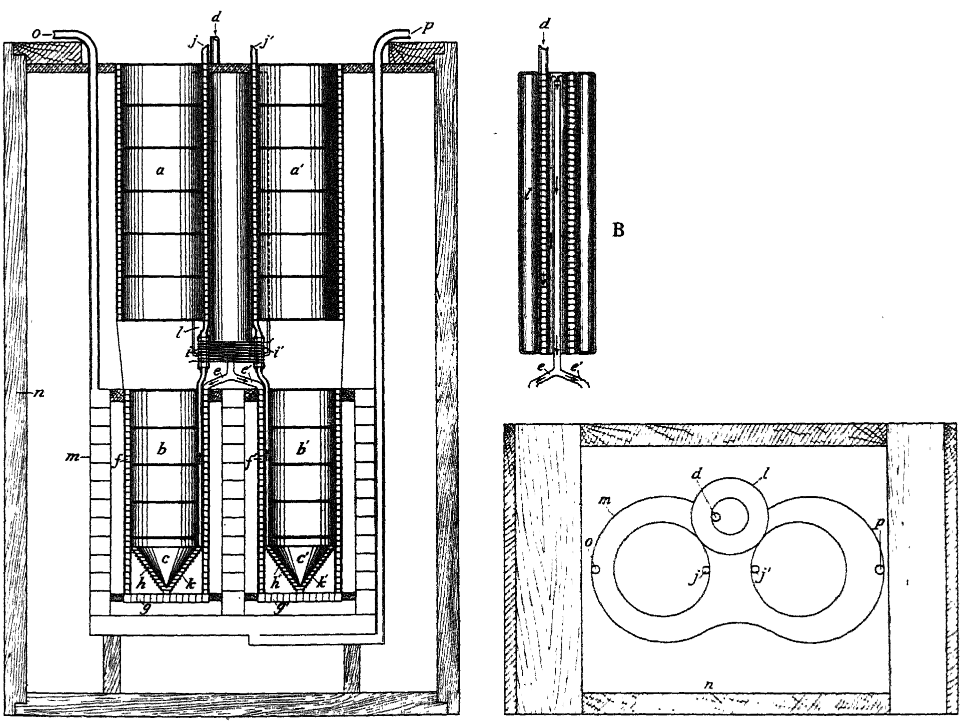
WHEN we began these studies about 40 years ago, scientists did not know within wide limits how much heat the sun sends the earth. The best textbook of meteorology published about 1900 gave values ranging through 250 percent without offering a preference. Both in theory and in practice the subject was chaotic. It is now almost universally admitted that the Smithsonian observations and publications have established that outside the atmosphere the sun's heat is known within 1 percent. We call it the "solar constant of radiation." If there could be a cube of water 1 centimeter (.4 inch) on edge, so black that it would fully absorb sunrays, situated on the moon, where there is no atmosphere, in March when the sun is at its mean distance, and exposed with its surface at right angles to the sunbeam, then its temperature would rise 1.94° C. (3.49° F.) in 1 minute of time.

Though this is called "the solar constant," many thousands of our measurements of it show that it is subject to small variations, seldom exceeding 1 percent, but on rare occasions reaching a range of 2 or even perhaps 3 percent. It varies from day to day because the sun rotates on its axis in about 27 days and is not equally bright all over. Thus unequally bright areas face us from time to time. It also varies over the months and years, but we are not yet sure that we know why. Some of our critics doubt if our measurements of the solar constant are accurate enough to show variations as small as 1 percent or less. They argue that even if our ground measurements have sufficient accuracy, still our observing stations on the earth's surface have above them an ocean of air, charged with ozone, water vapor, carbon dioxide, dust, and clouds, tending to throw the estimates into error. Can such a formidable obstacle be overcome?

First of all, we have to measure the sun's heat at ground level. For this purpose I perfected the "silver-disk pyrheliometer" about 30 years ago. Compared to a sewing machine or an

automobile it is a very simple instrument —merely a disk of silver about the diameter of a half dollar and a quarter of an inch thick. It lies, flat side up, at the bottom of a brass tube 15 inches long, through which the sunrays pass. The exposed front of the silver disk is painted dead black to absorb the sunrays. A thermometer bulb is inserted radially in a hole in the disk. The observer merely measures how fast the thermometer rises because of the sun's heating the blackened disk. Nearly 100 of these silver-disk pyrheliometers have been built and standardized at the Smithsonian Institution under my direction and have been furnished at cost to observatories and experimenters on all continents. Published accounts by Australian and Argentinian observers, as well as our own experience, show that these instruments can measure to an accuracy of .25 per cent, and that they have retained their accuracy unimpaired for over a quarter of a century.

To express our results in accepted heat units, however, we had to devise a primary standard pyrheliometer, with which to compare these silver-disk field instruments. For this purpose I invented the "water-flow pyrheliometer." Everyone has noticed how absolutely dark an unlighted chamber seems, and how all of its objects near the back wall fade into invisibility when viewed from outside through a small opening. In fact, such a dark chamber is a complete absorber, better than the blackest paint. With this in mind I used a dead-black tube to receive and absorb the sun's rays. Its hollow walls have a spiral channel from end to end within their thickness.



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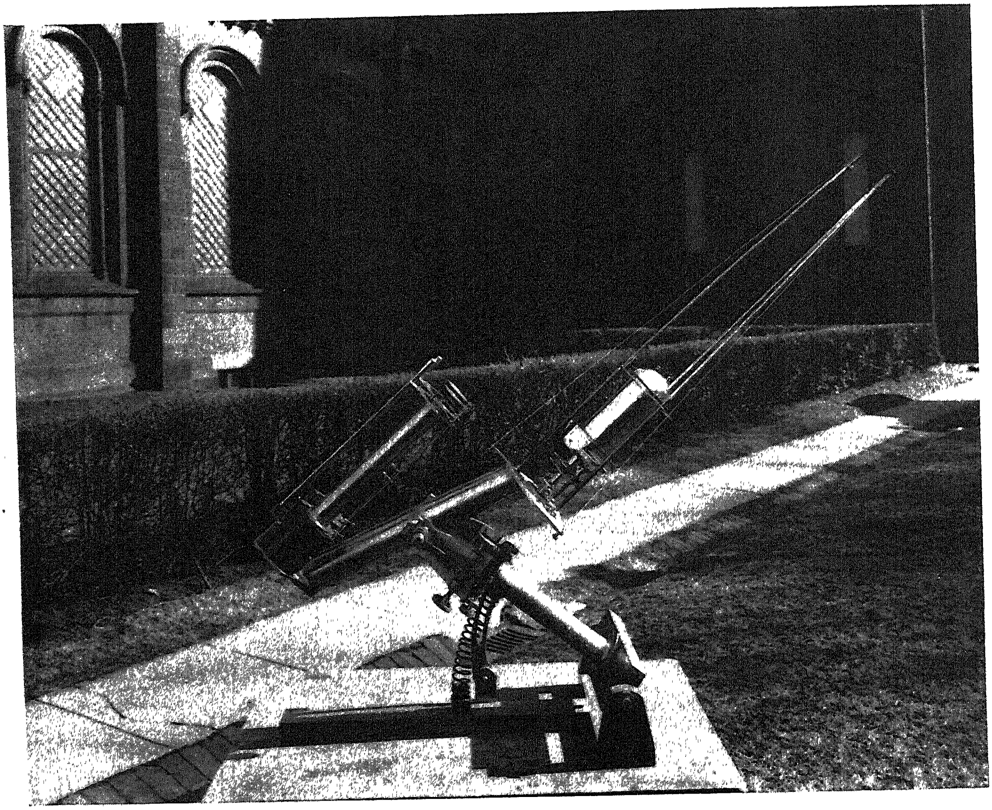
THE SMITHSONIAN'S WATER-FLOW PYRHELIOMETER

USED TO STANDARDIZE INSTRUMENTS FOR MEASURING THE INTENSITY OF THE RADIATION OF THE SUN.



TWO PYRHELIOMETERS AND A PYRANOMETER

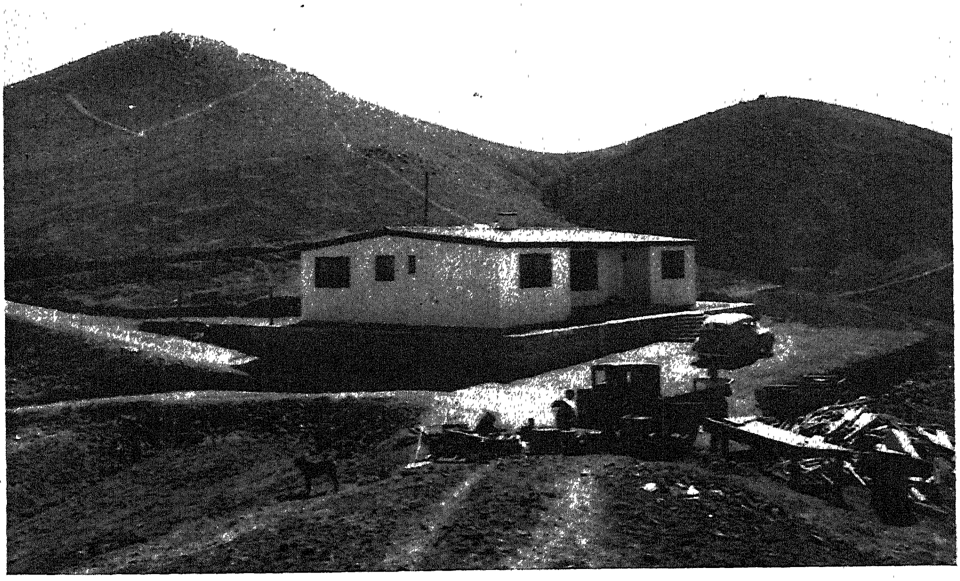
Through this channel a steady current of water flows at a measured rate to carry off the sun's heat as fast as received. By a delicate electrical thermometer we can measure the rise of temperature of the water caused by the sun's heat. The area of the front orifice through which sunrays pass into the instrument is accurately known. Thus we can measure how much a given weight of water is heated by the sunrays received in one minute through a known aperture, and fully absorbed. To make sure we are right, we have a coil of wire within the chamber near its back end, through which a measured current of electricity may be passed. This will produce a known amount of heat, which may be measured by the flowing water method as if it were sun's heat. Our experiments show none of the electrical heat escapes measurement. Hence we infer that is so with solar heat also. Such is our standard water-flow pyrheliometer. At a European conference, many years ago, Dr. Gustav Hellmann, then chief of the German Meteorological Service, said publicly: "There is but one standard pyrheliometer in the world. It is located at the Astrophysical Observatory of the Smithsonian Institution."

Dr. Langley once said in substance: "The difficulty of measuring sunrays accurately at the ground is indeed very great, but the difficulty of measuring their loss in the atmosphere is perhaps insuperable." So, though we had mastered the first difficulty, we had this discouraging outlook to face as we turned to the second branch of our problem.

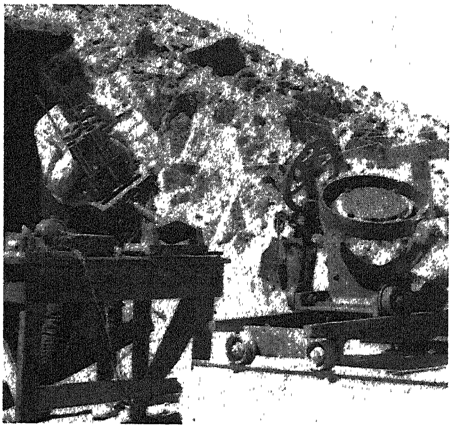
I shall not try to explain the theory and practice of estimating the transparency of the atmosphere for sunrays. Readers who wish to study it may consult Volumes 2 and 6 of the *Annals of the Astrophysical Observatory*. It will suffice here to say that we are required to locate our observing stations on mountains in cloudless deserts; that we take advantage of the fact that when the sun advances from morning towards noon the path of his rays in our atmosphere steadily diminishes; that we necessarily measure separately at about forty different places in the color spectrum, from far beyond the violet to far beyond the red; and that we use the Langley bolometer, which is an electrical thermometer so sensitive that it measures changes of 0.000,001 degree in temperature.

It is easy to believe that when we use an electrical thermometer sensitive to a millionth of a degree, we have to do it in very steady surroundings. The late Edgar Moore, of Los Angeles, suggested

MONTEZUMA STATION, CHILE
THE ORIGINAL DWELLING, ABOVE, WAS LATER REPLACED BY A REINFORCED CONCRETE STRUCTURE. THE INSTRUMENTS ARE ON THE MOUNTAINTOP.



THE NEW DWELLING AT MONTEZUMA, CHILE
THIS BUILDING WAS CONSTRUCTED TO WITHSTAND EARTHQUAKES. ITS HEAVY REINFORCED CONCRETE FOUNDATION RESTS MOSTLY UPON SOLID ROCK AND ITS BRICK WALLS CONTAIN THICK WIRES.



MOUNT MONTEZUMA, CHILE
APPARATUS AND OBSERVING TUNNEL USED EVERY DAY FOR MEASUREMENT OF THE SUN'S RADIATION.

to me many years ago that we could insurê a much more constant temperature around our instruments by mounting them within a tunnel, dug horizontally far back into the mountain. The sunbeam to be measured is reflected into the tunnel and made stationary in a north-south direction by controlling the mirrors with a suitable mechanism. We read in the Bible that Joshua made the sun stand still in the heavens. We cannot do that, but we do make its rays stand still in our measuring tunnels. Moore's scheme has gone far to insure to our results their high degree of accuracy.

You may be sure that I was proud when the late Professor Turner of Oxford in the year 1908, reviewing Volume 2 of the *Annals of the Astrophysical Observatory,* wrote: "Mr. Abbot has shown that he is measuring something definite, for he has detected an annual diminution of 3.5 percent from August to October, due to our greater distance from the sun." But the results published 32 years later in Volume 6 of the *Annals* disclose and measure changes in the sun's heat five times smaller than those for which Turner praised us.

On every promising day, for over 20 years at our 3 mountain observatories, we have made these studies of the sun's heat as it is outside our atmosphere. These stations are thousands of miles apart, one in the Southern, two in the Northern Hemisphere. Their results

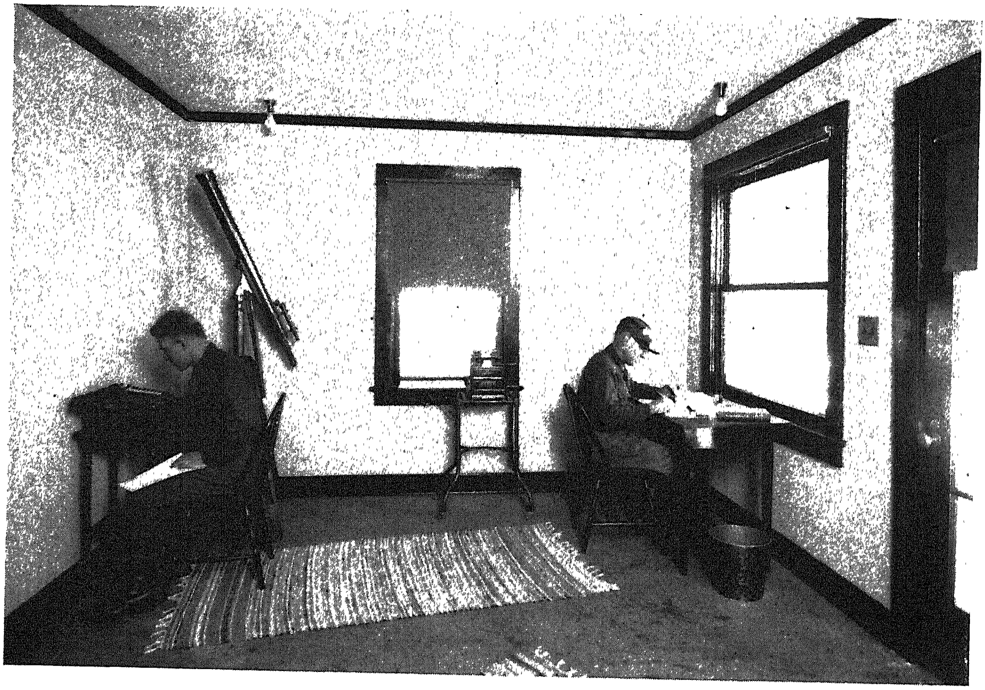
VIEW FROM THE NEW DWELLING AT MONTEZUMA

LABORATORY, SMITHSONIAN OBSERVATORY, TABLE MOUNTAIN, CALIFORNIA

agree beautifully, and all tell the same story, that the sun is a variable star.

This program is still in progress, and thus far has involved making about 20,000 measurements of the solar constant. Its results, from 1920 to 1939, are contained in very long tables of figures, illustrated by diagrams, occupying the last half of Volume 6 of the *Annals of the Astrophysical Observatory*. They have required great skill and devotion on the part of our observers in the field. Not only do they live like hermits, work like laborers, and observe like top-notch laboratory men, but they spend many hours every day measuring plates and computing the preliminary results. These they send to Washington, where four highly-skilled people go over every observation meticulously, compare the results, detect and cure errors, and apply little corrections only possible to determine from making use of thousands of days viewed as a whole. The preparation of Volume 6 of the *Annals* required almost four years of solid work by the workers at Washington, besides what was done in 20 years by observers in the field.

Some years ago we asked permission of the U. S. Civil Service Commission to appoint a certain young college graduate, whom we had specially trained for two years, to be in charge of the station at Montezuma. We described the place, the duties, the isolation. We told of the use of instruments to measure the sun's heat to 0.000,001 degree, and the necessity of being able to rebuild them on the spot if destroyed by earthquake. Such repairs involve the hanging of a carefully adjusted magnetic device, hardly heavier than a hair, upon a thread of



COMPUTING RESULTS AT TABLE MOUNTAIN

quartz crystal too fine to be seen with the naked eye. We mentioned the long and intricate calculations to be made each day, the early rising and long hours, the personal hauling of supplies—even of water—and the necessity for tact to carry on without friction, both in the observatory family and among the people of the country. We suggested that unless the Commission felt it indispensable to hold a public examination for the place, we be permitted to appoint the young man whose qualifications had been proved by two years' experience under our immediate supervision. The Commission replied, in effect, that as angels from heaven were unlikely to apply, they felt it futile to insist on a public examination.

Two types of solar variation are disclosed in Volume 6 of the *Annals*, short-interval and long-period. No less than 500 well-marked cases were discovered up to the end of 1939 in which the sun's heat increased, or, on the other hand, decreased in periods of 3 or 4 days each, by amounts ranging from .5 percent up to 2 percent. These short-interval solar variations are probably caused, as I have suggested, by the sun's rotation bringing unequally bright areas of the sun's surface to face the earth. This type of solar variation is very unequally distributed in the colors of the spectrum. It amounts to almost nothing for red and infrared rays, but increases steadily towards the green, blue, violet, and ultraviolet. The sun's variation in ultraviolet rays is at least six times as great in percentage as the variation of the solar constant itself, which of course comprises all kinds of solar radiation.

As is customary in many kinds of long-continued programs of observation, we give in Volume 6 of the *Annals* the *average* solar constant values for each 10 days, for each month, and for each year.

In this way the day-to-day fluctuations are smoothed away. But as we scan the whole 20 years of observation we see in these smoothed values many cases of rise and fall in the sun's output of radiation. The fluctuations are seldom as great as 1 percent in their range. Yet depending, as each of these average values does, on many days of observation, even these small fluctuations are significant. Indeed they are not all so small. There is one notable case in the years 1922 to 1924 when the sun's heat fell off rapidly by 2 percent, and then gradually recovered itself. I shall show later that this extraordinary case was accompanied by unusual weather in those years, and I shall point out the probability that we are now in the throes of a similar condition.

To the casual glance, the sun's variation, indicated by monthly averages from 1920 to 1939, seems altogether irregular, haphazard, and not subject to rule at all. But a prolonged and careful analysis has shown that it is really a complex of as many as 16 regular, simple, periodic terms. We have all watched the great waves sweep in toward the ocean beaches, bearing on their surfaces many little waves. The whole agitation of the water is a complex of big and little waves advancing simultaneously. Somewhat similar is the long-interval variation of the sun with its complex structure of simultaneously active long and short periods. A still more exact parallel is found in the sounds of a violin or trumpet. If one of these instruments sounds the tone *E* above middle *C*, its tone, if analyzed, is found to consist not only of the fundamental *E*, but of its octave, and of many higher tones called harmonics. These harmonics are all closely related mathematically in their periods of vibration to the time of vibration of fundamental *E*. The difference between the sound of the violin and that of the trumpet depends on which of these harmonics are present when fundamental *E* is sounded.

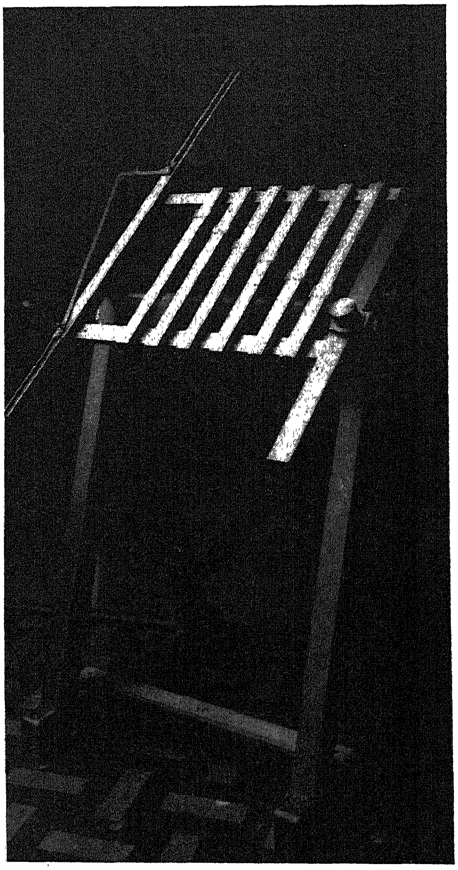
The sun's long-interval variation is similarly made up of different regular waves, or periods of change. All of them are almost exactly integral fractions of 273 months, or $22\frac{3}{4}$ years, which, in our musical analogy, we can regard as the fundamental. Our best knowledge indicates, however, that there are slight deviations from exact integral relationship with the "harmonics," as shown in Table 1.

TABLE 1. RELATIONSHIPS OF 14 PERIODS OF SOLAR RADIATION

Solar periods observed	273	91	68	54	45.25	39.5	34	30.33	25.33	21	11.87	11.29	9.79	8.12
Fractions of 273 months	1/1	1/3	1/4	1/5	1/6	1/7	1/8	1/9	1/11	1/13	1/23	1/24	1/28	1/34
Computed periods	273	91	68.2	54.6	45.5	39	34.1	30.33	24.8	21	11.87	11.37	9.75	8.03

If you ask why the particular periodic variations of Table 1 occur simultaneously, which added together make up almost completely the fluctuations in the sun's output of radiation, I cannot certainly tell. The most likely explanation, however, involves sunspots.

When Galileo made his famous telescope in the year 1610, besides the 4 moons of Jupiter he saw that the sun's surface had black spots upon it, which marched along and crossed the visible solar disk in about 14 days. This shows that the sun rotates in about 27 days, the spots being half the time hidden behind the sun. Since his time the sunspots have been studied. They have been



A SLIDE RULE COMPUTER

proved to be whirlpools in the gaseous substance of the sun. Their numbers range from almost none, as in 1943, up to many, as will be the case about a year hence. Their numbers, in fact, wax and wane in a cycle of about $11\frac{1}{3}$ years, which is called the "sunspot cycle." For at least the past 150 years these cycles have been alternately strong and weak. So there is really a master sunspot double cycle of nearly 23 years which is also the master cycle in the fluctuation of the sun's heat. My 14 solar heat periods are thus all very close to being integral fractions of the double sunspot cycle. They must, almost surely, be related to sunspots in some very deep-seated way.

But what causes sunspots themselves and their $11\frac{1}{3}$-year cycle of frequency? A great deal of study has been given by many astronomers and others to the possibility that sunspots are caused by the tidal forces of the planets, several of which are sometimes nearly in the same direction, as viewed from the sun. H. H. Clayton has recently published such a study. He finds that these planetary periods almost coincide with periods of variation in the sun's heat, as published in Volume 6 of the *Annals*.

But I will not pursue this clue further. I will add only that weathermen have found that the $11\frac{1}{3}$-year sunspot cycle produces corresponding periods in temperature and rainfall. Curiously enough, however, I did not find it a period of fluctuation in the sun's heat, as reported in Volume 6 of the *Annals*, but L. B. Aldrich has recently found a curious evidence there of its presence. It is well-known that sunspots are like machine guns, in that they bombard space, including, of course, the earth, with electric ions. This bombardment is very active at times of maximum numbers of sunspots. It is also well-known that electric ions, which in our atmosphere, besides reflecting radio waves around the earth so that we get programs from great distances, in addition act as centers of condensation for the water vapor of the atmosphere and so promote cloudiness, and doubtless also rain. Clouds, of course, also alter temperatures. So in this way, the $11\frac{1}{3}$-year sunspot cycle becomes a weather cycle and must be added to our group of fourteen. There are thus at least 15 solar periods, likely to be of some importance for weather.

(*To be concluded*)

SUNSHINE AND THE ATOMIC BOMB

By WILLIAM T. SKILLING

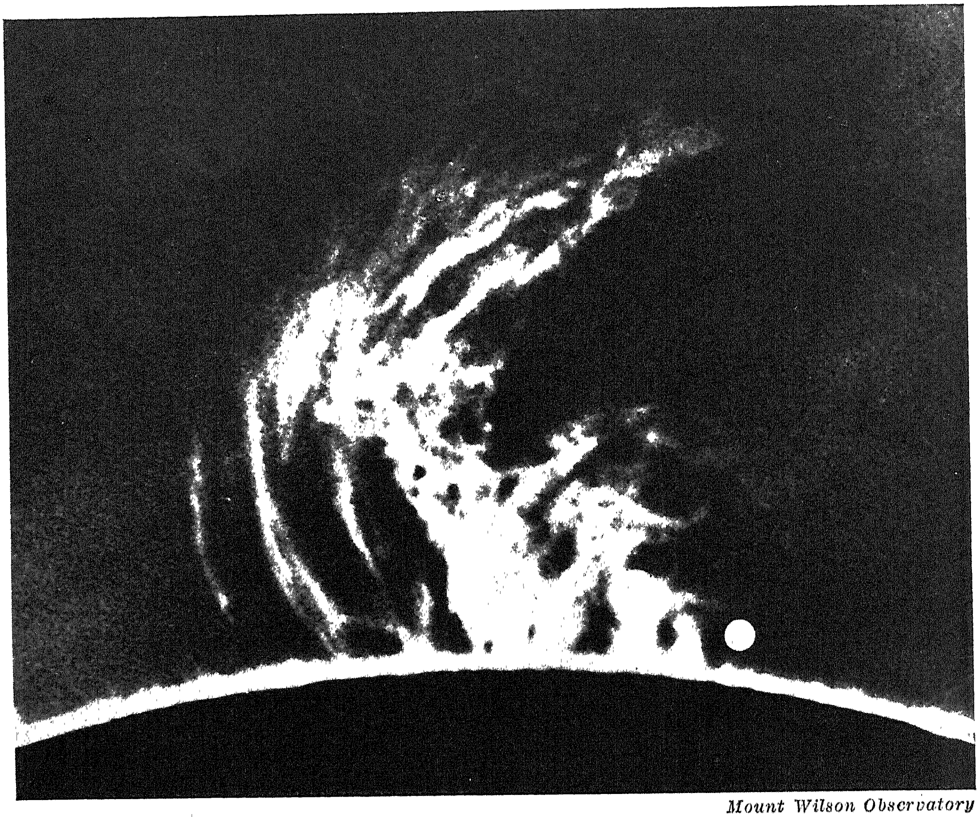
To COMPARE the atomic bomb with anything so gentle as sunshine *seems* absurd. Why not use a thunderstorm as an illustration if we are seeking one in nature?

But there are several arguments in favor of our title. In the first place, the actual amount of energy set free in the most violent lightning flash is small compared with the sun's heat energy falling on the earth, the equivalent of 1.5 horsepower per square yard of surface. If all the heat of sunshine could be converted into work, the total work would be greater than could be done by all the horses that would have room to stand comfortably on the earth's surface. A bolt of lightning may be very destructive instantaneously over a small area. Its duration is so brief, however, that when its force is spent, and its energy has been measured as well as scientists can measure so sudden a phenomenon, it is found that the total work that can be done by this much electricity is no more than could be done by the warmth falling on a square yard of the earth's surface in a day of bright summer sunshine.

Back of the power of the great expanding wave of heated air that seared and blasted to bits the city of Hiroshima there was an amount of energy let loose within the bomb greater than had ever been released on earth before from so concentrated a source. The reason for this lies in the fact that the detonator of the bomb opened a tap that turned on a flood of nuclear energy always before held back by the "binding force" of the nucleus. Out of the explosive material in the bomb came power equal to all that could have been generated by burning nearly 3,000,000 times its weight of coal. And even in this explosion only 1/1,000 part of the total nuclear energy of the atom was released!

We have seen photographs of the vast cloud that shot up to a height of 40,000 feet at the explosion. But this would seem like a toy demonstration of force if compared with similar outbursts on the surface of the sun (Fig. 1). The tremendous inner energy of the sun keeps its surface constantly boiling up with great bubbles of white-hot gas called "rice grains," or "granules" (Fig. 2), each as large as any of the states in the Union. Sometimes there are ejected immense cloudlike masses of gas called "prominences." These are often as extensive at their base as the size of the earth, and usually rise to a height of some 30,000 miles, or may go on up to 500,000 miles, with velocities ranging from a few miles to more than 100 miles a *second*. The energy back of all such activities on the sun is from a source similar to that from which comes the power of the atomic bomb.

The fundamental quality of the bomb that links it with the sun rather than with a lightning flash or an explosion of TNT is that its energy comes from changes in the deep interior of the atom, not from its surface. Electrical energy, including lightning, comes from the loose, outer electrons of the atom. The sun's heat and the force that explodes the bomb come from the dense inner core of the atom, called the nucleus, which occupies no more than 1/10,000 part of the atom's size. Lightning and fire both result from a reshuffling of the electrons that circulate around the nucleus as planets go around the sun. The electrons in no case constitute more than 1/1,800 of the atom's weight, and their



Mount Wilson Observatory

FIG. 1. A SOLAR PROMINENCE, 140,000 MILES HIGH
THE RELATIVE SIZES OF SUN AND EARTH ARE INDICATED BY THE SUN'S CURVED SURFACE AND THE WHITE DISK, REPRESENTING THE EARTH, WHICH APPEARS AT THE RIGHT OF THE SOLAR PROMINENCE.

ability to produce energy is a far smaller proportion than that in comparison with the energy that might come out of the nucleus with which they are associated.

Nuclear physics is a new science. The earliest successful attempt to make any change in the nucleus dates back only to 1919, when Sir Ernest Rutherford, of England, bombarded nitrogen atoms with particles flying naturally from radium, and changed some of the nitrogen atoms into those of oxygen. Since then artificial means have gradually been developed for speeding up the atomic particles to such velocities that nuclei struck by these flying missiles are broken. Electricity and magnetism are employed to give speed to the particles and to guide them against their target. These methods have reached their highest point of success in the atom-smashing cyclotron of Ernest Lawrence, at the University of California.

With the aid of an instrument called a mass spectrograph, experimenters found that when the nucleus of one atom was changed into that of another the weight of the newly formed atom was sometimes less than the total weight of the parts from which it was made. Two and two did not make four, but something less than four. The long-standing law of conservation of mass went out of date. In its place a new law emerged, namely, that if matter thus seems to be destroyed, enough energy is

produced in the process exactly to make up for the matter lost.

Long before this could be demonstrated experimentally Einstein had, in 1905, announced on theoretical grounds that if a mass of matter could be converted into energy, the energy produced must be equal to the mass destroyed times the square of the velocity of light, or $E = mc^2$, where E is the energy set free, m the mass of matter lost, and c the velocity of light. (Ergs of energy, grams of mass, and centimeters per second are the units employed in the equation.) This equation forms the basis for all work involving atomic changes, such as work having to do with the atomic bomb.

Although it was not until the end of 1942 that scientists learned how to handle the atom in such a way as to liberate more energy than they had to put into the process of generating it, they had long before settled definitely upon the theory that atomic changes, accompanied with loss of weight, must be the source of the sun's heat. Thus the faint hope was held out to them that they might sometime devise a terrestrial method of imitating solar efficiency.

During the twenties and thirties there waged a lively discussion as to whether in the sun the whole of matter is being annihilated as such, and energy substituted for it, or whether certain atomic changes may be proceeding steadily, induced by such a temperature as the sun is known to have, which would *reduce* the weight of the reacting substances enough to furnish sufficient heat.

Sir Arthur Eddington leaned toward the annihilation theory for a time; it seemed reasonable in view of the supposition that matter is essentially electrical in nature. Eddington reasoned that if a proton, the nucleus of the hydrogen atom, is nothing but a charge of *positive* electricity, and if an electron is an equal *negative* charge, the two chancing to

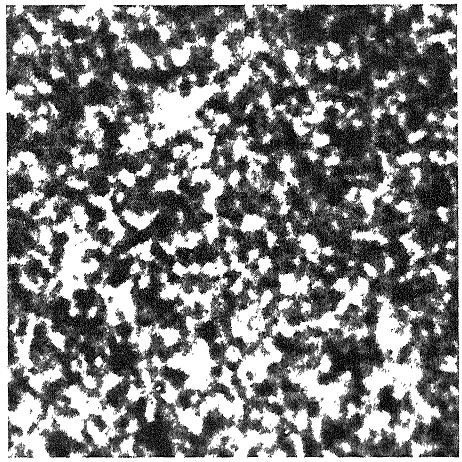


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FIG. 2. SURFACE OF THE SUN[1]
MAGNIFIED 700 TIMES TO SHOW THE GRANULES.

meet in a head-on collision might come into such close contact as to neutralize each other. As far as matter is concerned, this would be annihilation; but in place of the matter there would appear radiant energy, $E = mc^2$, which would be in the form of exceedingly short and energetic electromagnetic waves, very much shorter and more penetrating than X-rays.

The fly in the ointment of this theory was that physicists had never observed any cases of annihilation even on a small scale in the laboratory. In support of the alternate theory of partial loss of mass, there had been observed atomic changes in which the newly formed products weighed less than the sum of the separate parts.

Since the element helium is composed of atoms that weigh *almost* exactly 4 times as much as an atom of hydrogen, it could be very naturally assumed that an atom of helium is, indeed, 4 atoms of hydrogen that have in some unknown way been united. For a decade or so the sun's heat has been attributed to

[1] From *Sun, Moon and Stars* by W. T. Skilling and R. S. Richardson, Whittlesey House (McGraw-Hill), 1946.

some such change as this taking place. The atom of helium weighs less than 4 atoms of hydrogen by an amount equal to 1/140 of the hydrogen supposedly forming it, and this lost mass must become energy. The facilities of the nuclear physicist, including the mass spectrograph for accurately weighing atomic particles, have shown that many atomic changes do occur involving loss in mass, but in most cases much less loss than the hypothetical one mentioned above.

Regardless of whether complete or partial destruction of matter is the source of the sun's heat, the amount of such "fuel" needed to replenish the sun's constant loss of heat seems at first a little disturbing. Basing estimates on the fact that at the earth's distance from the sun each square centimeter of cross-section receives 2 gram calories of heat per minute, it is easy to compute how much matter would have to be destroyed to furnish this much radiation in all directions from the sun constantly. The staggering result is that it would take 4,200,000 metric tons (4,620,000 of our ordinary tons) per *second.* But what seems on the face of it so alarming vanishes as a menace, for the mass of the sun is so great that even at this rate of consumption it would be 150,000,000,000 years before 1 percent of its mass would disappear.

By 1939 so much information had been accumulated as a result of laboratory experiment that it became possible to say with a good deal of assurance that quite a number of atomic changes should be possible at the center of the sun, under the influence of its temperature of 20,000,000 C°. Velocities up to 500 miles a second would be induced in atomic particles, and their mutual bombardment would be sufficient to change some kinds of atoms, though not all. In that year H. A. Bethe, of Cornell University, announced a series of possible atomic changes, the sum total of which would be exactly equal to the very improbable chance of uniting 4 atoms of hydrogen to form 1 of helium. His series, known as the "carbon cycle," is represented in Figure 3. The cycle begins and ends with a carbon atom—hence the word "cycle"—and consists of collisions of 4 hydrogen nuclei with the carbon nucleus and its successive products, and, in addition, 2 products changing radioactively without the necessity of collision, these being unstable particles.

At each of the six steps there is a slight, but differing, loss of mass and a corresponding production of energy. The final product is helium and an atom of carbon like the one with which the cycle began. The net loss of mass is the difference between the weight of 1 helium atom produced and the weight of 4 atoms of hydrogen used up. The total production of energy may be computed step by step; or, for convenience in computing, the six steps may be united in one, going directly from hydrogen to helium—it makes no difference.

Knowing that atomic energy of some sort must be the source of the sun's amazing output of heat has been an incentive and an encouragement for scientists to make every effort to release some of this boundless store for terrestrial use. Simple computations based on Einstein's relativity equation $E = mc^2$ show that if all the energy of a pound of any kind of matter could be released it would be equivalent to a fabulous total that can be expressed as 15,270,000,000 horsepower hours, or 11,400,000,000 kilowatt-hours, or 21,500,000,000,000 gram calories, whichever kind of unit seems preferable to the reader.

The sun, according to Bethe's carbon cycle, uses 1/140 of this total amount of mass in changing hydrogen into helium. The atomic bomb makes use of only 1/1,000 part of the total in changing uranium into the lighter elements, but even this available proportion produces

an amazing output, as may be seen by removing three ciphers from each of the above total quantities. Perhaps the most comprehensible illustration of the amount of energy resulting from the fission of a single pound of uranium 235 or of plutonium is to say that it is equal to the heat produced by the burning of 2,700,000 pounds of coal!

The five years of experimentation that led to the atomic bomb dealt with entirely different kinds of atoms from those the sun uses, but in the bomb as well as in the sun the process is one of making changes of one atom into another with an accompanying loss of mass and a corresponding liberation of energy. Instead of using the atom of hydrogen, the lightest atom, as the sun appears to do, on the "Manhattan Project," as the great cooperative experiment on releasing atomic energy was, for secrecy,

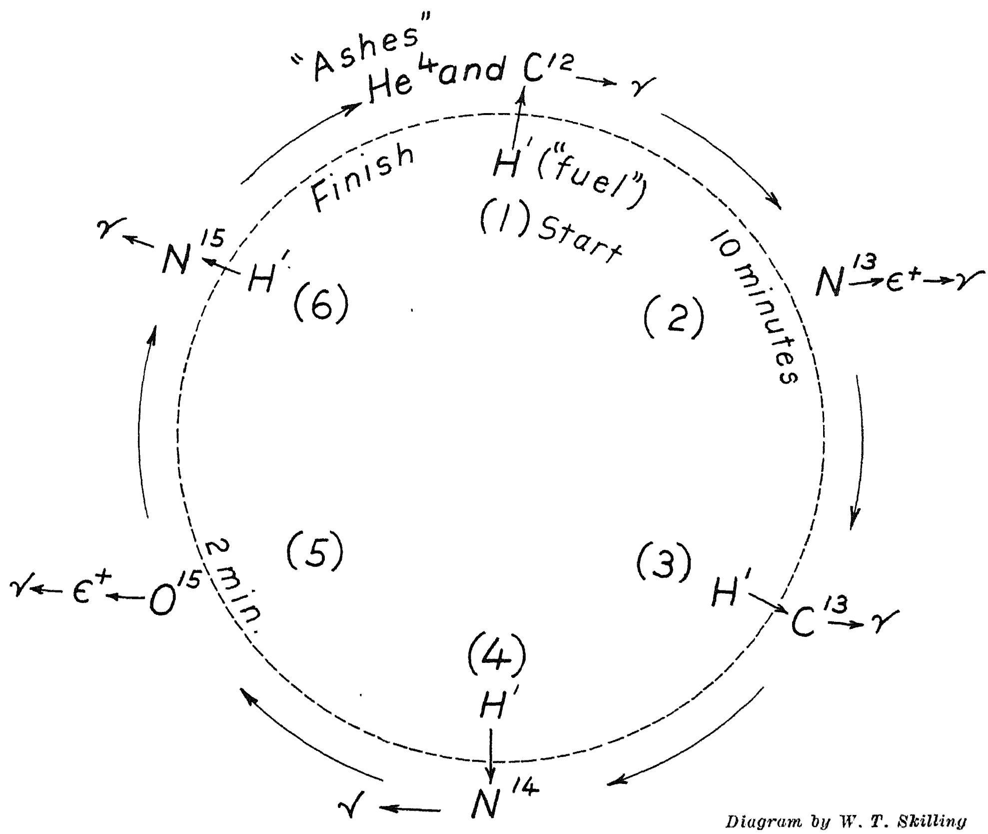


FIG. 3. THE "CARBON CYCLE" OF CHANGES WITHIN THE SUN

ACCORDING TO BETHE, AN ATOM OF HYDROGEN COLLIDES WITH ONE OF CARBON CHANGING IT SUCCESSIVELY INTO VARIOUS ISOTOPES OF NITROGEN, CARBON, AND OXYGEN, AND FINALLY BACK AGAIN INTO ORDINARY CARBON AND AN ATOM OF HELIUM. AT EACH OF THE SIX STEPS OF THE CYCLE ENERGY IN THE FORM OF SHORTER-THAN-X-RAY RADIATION (GAMMA RADIATION) IS EVOLVED. THESE SHORT WAVES OF ENERGY ARE CHANGED TO THE LONGER WAVES OF HEAT AND LIGHT BEFORE REACHING THE SURFACE AND ARE RADIATED OUT FROM THE SUN. THE *amount* OF ENERGY (HEAT) IS FOUND FROM THE EQUATION $E=mc^2$. THE MASS LOST, m, IS THE DIFFERENCE BETWEEN THE WEIGHT OF THE "FUEL" (HYDROGEN) AND THE "ASHES" (HELIUM). TWO OF THE STEPS (2 AND 5) ARE AUTOMATIC, N^{13} AND O^{15} BEING NATURALLY RADIOACTIVE. THE POSITIVE ELECTRON, ε^+, UNITES IMMEDIATELY WITH A NEGATIVE ELECTRON AND BOTH ARE "DESTROYED"; THAT IS, CONVERTED INTO ENERGY.

called, the workers used the heaviest of all atoms, that of uranium. They selected this because it is naturally so unstable that it constantly changes without any assistance at all into a succession of other atoms until the final product is lead. They chose it also because recent experiments had shown that in uranium there is a small admixture of especially explosive material.

An important incident that helped to touch off the greatest scientific adventure of all time was a visit, in 1939, of Niels Bohr, the father of the modern theory of the atom, to Albert Einstein at Princeton. From Europe he brought word of a remarkable new development in atomic science. During the previous ten years the behavior of atoms had been receiving close attention in various countries by physicists, represented by such well-known persons as Rutherford and Chadwick in England, Enrico Fermi in Italy, Joliot and Irène Curie in France, Otto Hahn in Germany, and Arthur Compton and Ernest Lawrence in the United States. Bohr, on his arrival, reported to his scientific friends that Otto R. Frisch and Lise Meitner, Jewish refugees from Germany, had told him that Frisch had split the atom of uranium by bombarding it with the very penetrating atomic particles called neutrons, and that Otto Hahn, of the physical laboratory in Berlin, had identified one of the products of the "fission" as barium atoms, which weigh somewhat less than half as much as the atom of uranium. (Always before the products of atomic change had been atoms not very different in weight from the atoms from which they were made.) Miss Meitner, of the Berlin laboratory, had computed the enormous energy of such a fission, and suspicion was at once aroused among all scientists who knew of the matter that in this direction lay the looked-for road to the liberation of useful quantities of atomic energy.

Many scientists were now convinced of the possibility of finding a way to secure atomic energy. They took the initiative in awakening an interest in Washington and in military circles. President Roosevelt was quickly responsive and secured a small appropriation to have the work started. Dr. Vannevar Bush, then Chairman of the National Defense Research Committee, led in the organization of the scientific work, and Brigadier General L. R. Groves was later placed in charge of all army activities in connection with the project.

A compelling motive for haste was the knowledge that Germany had at least an even start with us and was trying to add atomic bombs to her other "secret weapons." The work began in 1940 and culminated in 1945, resulting in the building of two new cities, one of 60,000 inhabitants on the Columbia River in Washington, the other in Tennessee. An experimental laboratory, said to have been the best-equipped in the United States, was also established in the desert wilds of New Mexico. The cost of the project amounted to $2,000,000,000.

The atomic particle called a neutron requires special attention in the discussion of atomic energy, for it has played a most important role in this whole five-year project. Although the neutron is one of the three fundamental particles that form all nature, neutrons were not known as particles until 1932, when they were proved to be such by Chadwick, the English scientist. They had been observed first in Germany, and then experimented with in France, but they had been thought to be "rays," not particles. They are similar in weight to the proton, the nucleus of an atom of hydrogen, but have no electric charge as the proton and the electron have.

This neutral quality gives the neutron power to insinuate itself into the heart of the atom. If charged particles are used for bombarding atoms they are re-

pelled by other similarly charged particles that are their target. But the neutron's lack of charge makes it hard to control; it cannot be speeded up by electric force nor guided by a magnet. To slow it down it must be passed through some light substance—graphite was used in work leading to the bomb. The neutron goes straight ahead until it collides with the nucleus of an atom.

Uranium was made the target for such bombardment by neutrons. Its atoms are the heaviest and most complex, and are naturally radioactive. Uranium, like nearly all elements, is made up of more than one kind of atom; the kind in greatest abundance in uranium has an atomic weight of 238, but one atom in about 140 is a lighter one weighing 235. It is this slightly lighter variety (isotope) of uranium that is "fissionable," that can be exploded. If the U–235 could be separated from the U–238 it would be ideal for use in the bomb, but since these isotopes are so nearly of the same weight, separation on a large scale is almost impossible. Chemical means cannot be used to separate them for, like all isotopes, they are chemically identical.

Fortunately, experiments with uranium both before and after the bomb project was begun led to the knowledge that it is possible to convert U–238 into a different element, as explosive as U–235. This was named plutonium. Being different chemically from uranium, plutonium can be readily separated from the unchanged metal. The making of plutonium is a major part of bomb construction. Great ovens, or "piles," made of graphite bricks, were used to change the huge slugs of uranium metal into plutonium.

Neutrons exist in the nuclei of atoms and are secured for use and given their motion by knocking them out of these nuclei with bombardment of some sort. On a small experimental scale, the most common way is to mix radium with the metal beryllium and let the particles coming off from the radium drive neutrons at high speed out of the beryllium. The neutrons become the bombarding particles to split uranium 235 and liberate energy.

For large-scale production of atomic energy what is called a "chain reaction" is set up, and uranium itself serves both as a source of neutrons and as a target to be bombarded by its own neutrons.

If uranium were made entirely of U–235 any large quantity of it would explode spontaneously, but in the small concentration of only 1 atom to 140 atoms of the nonexplodable kind, it is only split atom by atom. To get any large quantity of it to undergo fission, the action in the mass must be self-perpetuating. It must be made to go on as a fire does when once lighted; the fire does not go out when the match is removed. Such an action that, once started, is independent of any outside influence is called a "chain reaction."

Two conditions are necessary to set up a self-perpetuating chain reaction in uranium: the high-speed neutrons must be slowed down to a lower rate, called "thermal velocity"; and there must be a large enough mass of uranium so that the flying neutrons will be so well-surrounded with it that not many of the neutrons will escape without hitting a nucleus. Neutrons of moderate speed will make a larger proportion of hits; passing through graphite before coming to uranium slugs slows the neutrons.

The first chain reaction of this sort ever set up was in a graphite pile containing 6 tons of uranium on the campus at the University of Chicago, December 2, 1942. This first feeble "atomic fire" threw off energy at the slow rate of only .5 watt; and, though this rate of action was in a few days raised to 200 watts, this pile would have had to be kept in operation for 70,000 years to yield

enough plutonium to make one bomb. But this experimental pile was soon succeeded by the mammoth plutonium plant on the Columbia River.

The graphite piles are built with openings running through them. In these openings are placed the slugs of uranium, sealed in aluminum cans to keep them dry, as water runs through the pile to prevent overheating. Neutrons strike the uranium atom and are absorbed, thus increasing the weight of the atom, but it is still uranium, for the charge on its nucleus has not been changed. Then the nucleus gives off automatically 2 negative electrons and this does change its charge, raising it by 2, because the loss of negative charge is equivalent to an increase of positive charge. It is now a different element, Number 94, never existing before, and is given the new name, plutonium. Occasionally the uranium, now containing a little plutonium, is removed automatically from the pile and dissolved. Then the new element can be separated chemically from the uranium.

The piles must not work so fast as to explode. Strips of cadmium or steel containing boron are pushed into slots in the pile if action is dangerously fast, for these absorb some of the neutrons, which act like sparks in spreading the "atomic fire." If action is too slow, the strips are drawn out so that more of the neutrons will act upon the uranium. These strips serve a purpose similar to that of the thermostat in an oven. All such operations are either automatic or are performed at a distant instrument board, where the men are well-protected from the harmful rays and particles that are always thrown off in atomic changes.

Naturally the details in regard to the construction of the bomb have been withheld for reasons of security in the report authorized by the War Department and prepared by Dr. Henry D. Smyth, one of the scientists on the project. One difference between plutonium production and bomb production is that slow-speed neutrons are used in the former and high-speed ones in the latter. This is so that all particles in the bomb will be acted upon before they are blown too far apart for complete explosion to take place.

It is believed that within the bomb the explosive is stored in separate compartments, each portion so small as to be below the "critical mass" necessary for a chain reaction to be set up. Then to detonate the bomb it would be necessary only to bring these separate masses very suddenly together so that the mass will be above the critical quantity required to touch off the chain reaction. The igniting neutron sparks could be furnished by the presence of a little radium and beryllium, or the few neutrons always present in uranium. Even cosmic rays could serve as the igniting spark.

What can be said at this time of the future use of atomic power? Since that day in December 1942, when the first chain reaction was set up among the atoms, it has become evident to scientists that some future use of nature's greatest storehouse of energy is inevitable. When it was found that only a *part* of the liberated energy was necessary to make the process continuous and automatic, it could be easily seen that the *rest* of the power might be harnessed to do some useful work. Though this fraction of the atom's total potential energy that is over and above the fraction necessary for the automatic continuation of the process be ever so small, it becomes a matter of mere technique to increase the available fraction, as has been so abundantly shown since that first feeble display in 1942.

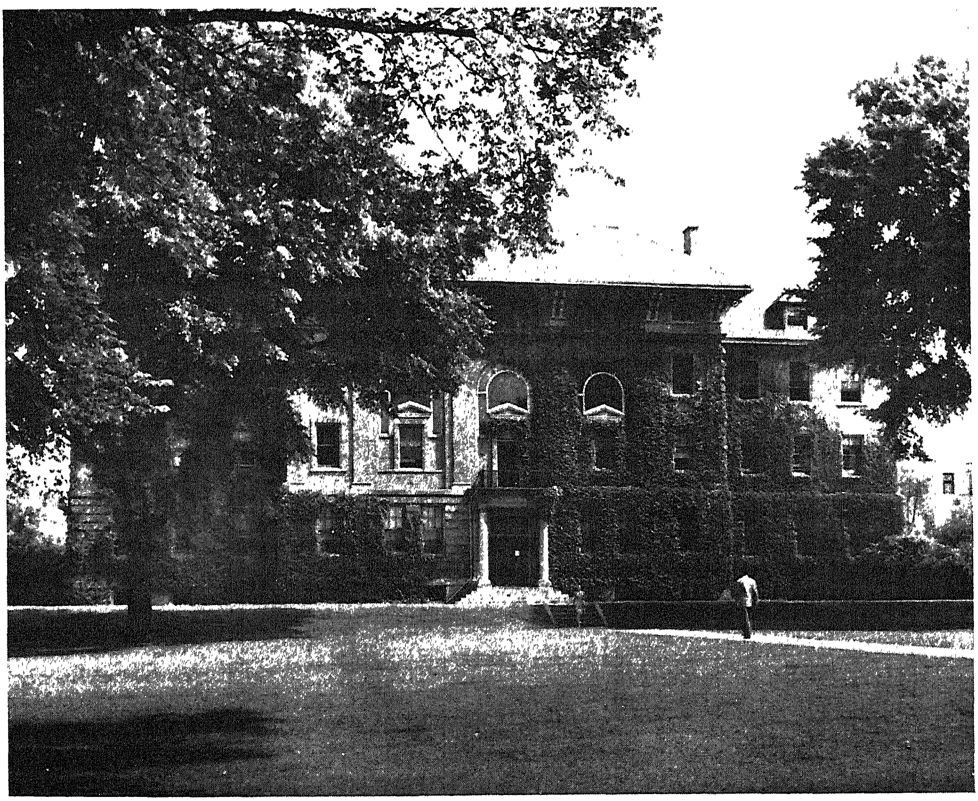
THE LENGTHENED SHADOW OF A MAN AND HIS WIFE—II*

By JAMES G. NEEDHAM
PROFESSOR EMERITUS OF ENTOMOLOGY AND LIMNOLOGY, CORNELL UNIVERSITY

WHILE Professor Comstock, as hereinbefore noted, in addition to teaching large classes, was doing many things for the promotion of interest in entomology, Mrs. Comstock was also doing things on the side. She had married before the completion of her college course, and now she wanted to finish it. She managed to take a few courses in her spare time and to graduate from Cornell in 1885. She entered into the social life of the faculty, where she was a great favorite. She attended many social functions, taking her husband along when she could get him to go, and understandingly leaving him behind when she couldn't.

She was keeping a diary, which became much more than a record of events. It told a story of high-brow social life in the nineties with a university background. Condensed here and expanded there, it was published as a novel in 1906 under the title *Confessions to a Heathen Idol*, by Marian Lee. A pseu-

* Continued from page 150 of preceding issue.



COMSTOCK HALL, HOME OF CORNELL'S DEPARTMENT OF ENTOMOLOGY

donym was used on the first printing, she said, because it would by some be considered "scandalous" that she should write a novel, and ruinous to her scientific standing. Comstock had read the manuscript, but had offered no assurance of success beyond the qualified endorsement: "For people who want this sort of thing, it is just what they want." A second printing was called for, however, by the public, and that one was made under her own name. No one can read *Confessions* without finding in its kindly philosophy and gentle humor a new understanding of the reason for Mrs. Comstock's social influence.

Mrs. Comstock meanwhile began the study of wood engraving. She was preparing to make suitable illustrations for her husband's projected textbook. For several years, while he was piling up manuscript, she was practicing with her engraving tools, gaining skill with practice and getting what guidance she could in Ithaca. Then she went down to New York City and to Cooper Union for six weeks of special training under the master-artist, wood engraver John P. Davis. Her work there won warm approval, and she was later elected to membership in the American Society of Wood Engravers.

From studying with Davis, she returned to the extended task of preparing the choice engravings that distinguish Comstock's *Manual for the Study of Insects.*

In the year 1889–90 Comstock got his first full-time assistant, Mark Vernon Slingerland. He was a cousin of Mrs. Comstock and came from her home county. He had entered the college as a Freshman two years before, knowing nothing whatever about entomology. Out of curiosity to see what his cousin's bug-chasing husband was like, he attended one of Comstock's lectures. The subject chanced to be the life history of a butterfly. Slingerland had not known before that butterflies come from caterpillars, or that they have an intermediate pupal stage in their life history. He found the subject of absorbing interest and then and there decided that he wanted to study entomology; and he studied with such zeal and success that he won appointment to an assistantship in that subject while he was still an undergraduate.

Slingerland's interest in insects ran to the applied side. He was quick to see that insect control depends first of all on knowledge of their life histories and habits. He soon made a name for himself by the work that he did in that field. He took over the undergraduate course in economic entomology and a large share of the work on insects in the Agricultural Experiment Station. He was made an instructor in 1890 and an assistant professor in 1907.

Slingerland took to insect photography like a duck to water. When I knew him he almost lived in the insectary with that big, old long-bellows camera. He went to the new insectary with it, stayed with it, all but slept with it; and it became the instrument of his chief contributions to economic entomology.

I shared an office with him in the head house of the insectary in the summer of 1897 and saw him at work. He was out in the greenhouse taking pictures, or out in the field after subjects for more pictures, most of the time. He loved to photograph insects: whole insects; insect eggs; larvae; pupae; insects on their food plants, in their burrows, in their cocoons; singly, in pairs, and in swarms. And he wrote his bulletins around his superb photographs. I never knew another man so wholly devoted to one pursuit.

He set a new standard in entomology for fine photographic illustrations. He built up a collection of lantern slides that was the best in his day, and that will long continue in service.

The next addition to the teaching staff of the Department was made in 1896

when Alexander Dyer MacGillivray was made an instructor.

MacGillivray was a mild-mannered man of very youthful appearance, blue-eyed, slightly stooped, quiet, industrious, and able. He took charge of the laboratory in Comstock's introductory course in general entomology. He was very kind and helpful with students, but also very rigorous in demanding full compliance with the requirements established for that course. Comstock called him "an excellent drillmaster."

While Slingerland was building up a great collection of negatives and lantern slides for the Department, MacGillivray was building up its insect collection. He was a good systematic entomologist; knew the insects of all the orders; collected diligently at every opportunity; pinned, labeled, named, and arranged his specimens with great care. He had a wonderful eye for species and could remember their characteristics and name them at a glance.

Some of his best collecting was done from the freshly gathered material that students brought in from their field trips. He had to supervise the work of students in determining their catch, and whenever an insect appeared that was of a species not represented in the Cornell collection, he spotted it immediately, and begged or traded with the student for it (*he always got it*) and added it to the departmental collection. Thus the collection grew apace.

Comstock chafed a bit under the necessity of teaching entomology during the long winter season, while unable to hold classes in the summertime when the major phenomena of insect life are available for study; so, as soon as he could, he made a shift in his own time schedule. He established a summer term in entomology and took his own vacation during the winter term. His summer course then stood alone. It invited full-time registrants. It soon began to attract teachers from other schools and colleges. They came to spend their summer vacations devoting full time to the study of entomology under Professor Comstock. Thus the enrollment came to have a considerable admixture of more advanced students and of graduates who were specializing in this field.

The first winter that was freed from teaching by this arrangement gave the Comstocks a chance to go abroad. They spent the winter of 1888 at the University of Leipzig in Germany.

For the next ten winters they were destined to be back in the teaching harness again. Stanford University was born. President David Starr Jordan persuaded the Comstocks to come out to Palo Alto and organize a department of entomology there, where in a milder climate than that of Ithaca insects could be studied in action all year round. It seemed like a good way to spend a vacation. Dr. Jordan was a dear friend, and there were former colleagues from Cornell now members of the Stanford faculty. It was a new and hopeful educational enterprise. A rich and little known insect fauna was waiting to be studied there. So they went to California; and kept on going for ten years.

The second winter they took Dr. Vernon R. Kellogg along as assistant in entomology, and left him there to carry on through the year and later to become head of the very successful department.

The few entomologists who were there had a good time together. Dr. R. W. Doane, who was then a graduate student, once told me some of their doings. He especially treasured the memory of the Sundays when he and Kellogg and Comstock took to their bicycles and pedaled out into the West Hills. They knew a secluded little valley with a running stream, where spring came earlier than elsewhere, where many rare insects were to be found, and where a good farm housewife furnished a hot luncheon, with fried chicken and all the trimmings.

Comstock's load of work was growing heavier. With classes to meet all the year round at Palo Alto and at Ithaca, with supervision of the research work of graduate students, with correspondence, and with frequent faculty duties that he never neglected, he found little time for writing. Each day was too full, and he himself at the end too tired to write effectively, so he decided to write in the morning when his mind was rested. He began the practice of getting up at 4 A.M. each day and going to bed (except when duty otherwise demanded) at 8 P.M. It goes without saying that, on a university campus, few would come into his office between 4 A.M. and breakfast time to interrupt his train of thought.

After the completion of the *Manual for the Study of Insects,* Comstock went to work on a similar treatise on spiders. For his needs far too little was then known about the spiders of the states on our southern border. So he spent a winter in the South, collecting materials for this book. There he had colleagues in the field who helped: noteworthy among them were William Morton Wheeler at Austin, Tex., Harcourt A. Morgan at Baton Rouge, La., and Glenn W. Herrick at State College, Miss.

The work of spiders in spinning their webs fascinated him. He desired that it should be adequately illustrated. He felt that its marvelous detail would be beyond the power of any artist's pencil to portray. So he took to his camera to show it. He kept living spiders and arranged suitable "looms" for their use in spinning in order to get freshly spun and uninjured webs to photograph; he called them "made-to-order" webs. And I can testify from personal knowledge that a prodigious lot of time, painstaking preparation, and patience went into the making of his superb pictures of them. The *Spider Book* was finally published in 1912, and took its place as a standard text and reference book for students of arachnids everywhere.

During the winter of 1907–8 the Comstocks took their first and only sabbatical leave. They spent most of the winter in Italy, Greece, and Egypt. In Europe they visited foreign entomological colleagues: Berlese in Rome; Silvestri in Portici; Simon (arachnidologist) in Paris; Poulton and Hampson in London, and others. In Belgium Comstock was elected to honorary membership in the Société Entomologie de Belgique.

Mrs. Comstock's greatest work still lay ahead. In the agricultural depression of the nineties, when farming had become unprofitable and country youths were flocking to the cities in alarming numbers, wise men were seeking some means of making life on the farm more attractive; educational means, as well as economic. In the hope of interesting future farmers in things of value and of beauty in their rural environment, the New York State Legislature made a first appropriation of $8,000 for the teaching of nature study in the rural schools, and handed it over to the College of Agriculture to administer.

This was a new kind of agricultural extension. Liberty Hyde Bailey was put in charge of it. He furthered it mightily by writing and speaking, as well as by efficient administration. A series of *Nature Study Leaflets* for use in rural schools was begun at once, and to that series Mrs. Comstock and members of the faculty of the College of Agriculture contributed numbers. "Uncle John" Spencer was drafted from his farm to deal directly with the children in schools. He promoted Junior Naturalist Clubs in the rural schools of the state until he had enrolled at one time more than 30,000 members. But it was Anna Botsford Comstock on whom fell the main task of bringing nature study to the teachers in the schools. To her, more than to any other person, the continuing success of the undertaking is due.

She gave up wood engraving to meet a greater need. She wrote many nature

study leaflets and made the drawings for their illustration. She wrote notebooks for children's use on birds, trees, and familiar plants, and got competent persons to write them on other groups. She discussed nature study at teachers' meetings all over the state, and lectured on it at Cornell and at other universities; at Stanford and Columbia repeatedly; at the University of California in 1906; at the University of Virginia in 1920; and at other educational institutions of various grades.

She organized courses in the teaching of nature study especially for rural schoolteachers at Cornell University. In 1908 she was awarded an assistant professorship by the trustees of Cornell—the first woman to attain that academic standing.

Fine as was her work with a graver, what she did with her pen was even more remarkable. She wrote books; first two small ones that were strictly entomological: *Ways of the Six-Footed* in 1903, and *How to Keep Bees* in 1905. Then she surprised her friends by publishing the hereinbefore mentioned novel, *Confessions to a Heathen Idol,* 1906; then the big two-volume *Handbook of Nature-Study* in 1911, and finally, *The Pet Book* in 1914, and *Trees at Leisure* in 1916.

If asked to name a single one of these for which she would like to be remembered I think she would have chosen the *Handbook of Nature-Study.* That book slowly evolved out of her work on the *Leaflets,* out of her writing for the *Nature Study Review,* which she edited for years, and out of her experience in training teachers of nature study. She consulted her husband, so she once told me, about the desirability of preparing the *Handbook,* and he encouraged her to do it; said he would give her any help he could; said it was much needed, but that she must not expect any financial returns from it, for it probably would never pay printing costs. So it was done as a labor of love and as a public service. Happily, he was mistaken. Its success was soon assured. It became, and still is, the one most essential reference book of nature study, and it is used around the world wherever nature study is taught in the English language. It is now (1945) in the third printing of the 24th edition, and going strong. Such is Mrs. Comstock's record as a writer.

She was raised in rank to a full professorship of nature study in Cornell University in 1920. She was given an honorary degree, Doctor of Humane Letters, by Hobart College in 1930. She became a member of the board of trustees of William Smith College and of Hobart. She was designated by a poll of the members of the National League of Women Voters, 1923, as one of the "twelve living women who have contributed most in their respective fields to the betterment of the world."

When I first came to Cornell as a graduate student late in the summer of 1896, the summer term had ended, but a few students lingered at the laboratory in White Hall, and Professor Comstock was there with them. He greeted me pleasantly, took me into his office, and proceeded at once to ask me what I had done in my first year's graduate work at Johns Hopkins University. Then he asked about the work in which I had participated earlier that summer at the floating laboratory of the Illinois State Natural History Survey. My first impression was of his alertness to what was going on in fields bordering on his own.

He showed me about the Department. In his office two east windows looked out upon the green of the main quadrangle. Before one window stood his own plain, orderly desk; before the other stood Mrs. Comstock's desk, equipped with light controls and tools for making wood engravings. On a table were prints and proofs; also a typewriter; on the wall, a hand-cranked telephone.

The Department was up-to-date with Welsbach-mantled gas lights, projection-lantern arc light, and a darkroom equipped for making photomicrographs. In the lecture room were long shop-made benches, with backs so aslant that they caused discomfort, but with a writing arm for each student's notebook.

There was but one laboratory for students. It extended across the entire north end of the building. It was equipped with carpenter-shop-made two-drawer tables. There were no desks for students as yet. Graduates and undergraduates shared the laboratory and even some of the tables, for there were more students than there were places to seat them apart and things had to be shared. An unabridged dictionary stood in a corner on a stand of its own. Comstock recommended its constant use as an aid to scholarship.

At that time Comstock, Slingerland, and MacGillivray were the entire staff of the Department. Comstock was giving the lectures in entomology, and in a course in general invertebrate zoology as well. MacGillivray cared for the routine of the laboratory, saw that students were supplied with specimens to be studied, and that they did the work assigned. He spent all his spare time in building up the departmental insect collection. Slingerland was taking over a large share of the experiment station work on insects.

Comstock was deep in his study of the venation of the wings of insects, but the only time he could claim for research was in the early morning hours, before the chimes began to call others to their tasks. By day the door between his office and the laboratory was nearly always open. Undergraduates were not neglected by him; he took a personal interest in the work of every one of them.

There were then but five graduate students in the Department. They sat with the undergraduates in the laboratory, where Professor Comstock came betimes to see how each student's work was progressing, and to drop hints of excellent things to be learned beyond the bounds of the present laboratory assignment. He did not do any student's work for him, nor did he leave him floundering alone when a mere suggestion of method or material would save his time and temper.

Graduates were expected to take the lectures in the beginning course in entomology along with undergraduates, but even those who had studied entomology elsewhere found that no hardship: it provided further knowledge. Comstock's handling of the subject was a lesson in balanced organization of subject matter, in clearness of presentation, and in simplicity of language. It was said of his lectures that "his winning personal attitude made every student a sympathetic listener."

As my work progressed it was stimulating to have him come in to the laboratory and sit down beside me and say, "Now show me what you have been doing," and to have him express pleasure in every little discovery that seemed to have any significance. It made me eager to go on. It was a treat to go afield with him to see the delight he took in the living world, especially in its insect inhabitants. He loved everything out of doors.

Early in my first year of study with him, after he had invited me to join him in a special study of the developing wings of insects, he said to me one day, "We need to know the tracheation of the wings of the cicada. I planted some 17-year cicada eggs on the campus here sixteen years ago. If they thrived the nymphs hatched from those eggs should now be well-grown. How would you like to help me dig for some of them?" Of course I agreed. So we got pick and shovels and went out to dig together. It was a Saturday afternoon, and the quadrangle was deserted.

There was then a lone hickory tree standing near the south side of the main

quadrangle in front of Boardman Hall. Sixteen years before Comstock had collected twigs from other trees, well-studded with fresh-laid cicada eggs. He had placed the twigs in the boughs of that hickory, where nymphs on hatching from the eggs would fall to the ground. He thought they would burrow down and feed upon the tree's roots. The long developmental period of this species was well-known by reason of the regular seventeen-year recurrence of local broods, but in the field of entomology Comstock liked to be able to speak from personal experience.

We dug and dug. We dug for two hours straight. We dug well down among the hickory roots in several places without finding a single cicada nymph. Finally, as he began to fill holes and to replace sods, he said: "Well, we've had some good exercise. The cicadas didn't like my choice of a home for them. Something in the environment was wrong." As we went back to the laboratory he added: "Some of our laboratory cultures also will fail, but that should not keep us from trying again."

Professor Comstock was not a one-college entomologist. In the middle of my second year of study with him, he made arrangements for my study *in absentia*. For the completion of my doctor's thesis I needed to study Odonata (dragonflies) in a larger collection than Cornell then possessed. He sent me to study in the Hagen collection at the Museum of Comparative Zoology. Besides the advantages to be derived from the use of that wonderful collection, I lived while at Cambridge in happy association with the zoologists at Harvard University: Edward L. Mark, George H. Parker, Charles B. Davenport; paleontologist Tracy Jackson; and the members of the Cambridge Entomological Club. The club in that day met with Dr. Samuel H. Scudder in his private museum. I appreciated those meetings.

Comstock desired that his students should know something of the men and the methods in other institutions as well as in his own. He considered these men as colleagues rather than as competitors. He entertained no feuds over technicalities; cherished no grievances; was always ready to cooperate. He advised us who studied with him to respect the work and the opinions of others; to stick to facts and not to engage in argumentation. When a piece of research had reached the point of preparation for publication, he required strict attention to the correctness of every statement, saying, "Be sure you are right, and then look again."

Aid and comfort and sound advice he gave me on many occasions. I cannot refrain from mentioning a few additional items of a very personal nature. When I returned to Ithaca to join the Cornell faculty and was looking for a place to live he said to me, "Find a location where your family will have congenial neighbors with like interests."

A little later he said, "If you like golf, the local country club would welcome you to membership. I joined to help a new local enterprise get under way, but I don't play golf often. I find trips afield after insects more interesting. The last time I figured up accounts I found that my golf was costing me $18 a hole."

When I had been selected by the New York State Museum officials to set up a field station for the study of aquatic insects in the Adirondack Mountains, he said to me, "How fine it is to get paid for doing what you would be doing without pay for fun and by preference."

After receiving my doctor's degree from Cornell University in 1898, I went to Lake Forest College as Professor of Biology, and remained there for eight and a half years. During that time there was steady growth in the Department of Entomology at Cornell, and improvement in its equipment. There were four members of the teaching staff. William A. Riley had come on a fellowship in

1898 and had been made an instructor in 1901. A new course had been established in insect morphology, and in that course he had a large share. The Department was just leaving its outgrown quarters in White Hall for new and larger ones in Roberts Hall on the adjoining Agricultural College quadrangle.

In 1907 I returned to join the staff of the Department as Assistant Professor of Limnology. It was given to me to initiate a new course of university instruction, and to break ground in a little-developed field. I count myself as very fortunate in having had Professor Comstock for my sponsor. He prepared the way for me. He persuaded Jared T. Newman, an honored and farsighted trustee, to give the University land at the head of Cayuga Lake for a biological field station. Liberty Hyde Bailey, then Dean of the College of Agriculture, provided a station building. Delavan Smith, of Lake Forest, Ill., a public-spirited friend, provided initial equipment and support. The need of both teaching and research in the undeveloped resources of our inland waters was recognized without argument by these men.

My own special field of limnological research was to be the biology of fresh water insects, and that justified my placement in a department of entomology. The wet land and its open waters, being a part of the land in whose animal population aquatic insects play a very large role, made desirable the alignment with a college of agriculture and an agricultural experiment station.

On the northwest corner of the second floor in Roberts Hall there was an excellent classroom that I was assigned for work in limnology. My first class numbered six students, four of whom later became my colleagues on the Cornell faculty: Hugh Daniel Reed, Albert Hazen Wright, Arthur Augustus Allen, and John Thomas Lloyd. Lloyd became my assistant. Later, as Instructor in Limnology, he joined me in authorship of a textbook for the course, *The Life of Inland Waters* (1916). Professor Comstock gave our work in limnology wholehearted support.

His department was run with economy and true efficiency. In all his relations with his helpers, from janitors to assistant professors, Comstock's method was to assign the work to be done, arrange fit conditions for doing it, and then keep out of the way. He didn't ask for reports at stated intervals; he asked only for reasonable accomplishment.

He was a bit short-tempered with any who shirked. Dr. Robert Matheson relates that one morning he entered White Hall with Professor Comstock, who, on going into his office, found his wastebasket full of paper scraps left over from the work of the day before. The janitor had neglected to empty it. Comstock seized the basket, carried it out into the hall and dumped the contents down the stair well, scattering them all the way down to the basement. Then he remarked casually that he had reminded the janitor often enough—perhaps he would now remember the wastebaskets.

Retirement from teaching service meant for Professor Comstock a golden opportunity to put the results of his lifelong studies into final form. He kept his office in Roberts Hall, and went in and out daily, setting a good example of productive scholarship to all the Department. First he completed a book that was a summary of work in his own special research field, *The Wings of Insects* (1918). A leading British entomologist said that Comstock's work in this field was the greatest contribution to knowledge of entomology in half a century. Then he put together another larger book that embodied the subject matter of his basic course, *An Introduction to Entomology* published in 1920.

IT HAS been said that many an institution is "the lengthened shadow of a man": the Department of Entomology

at Cornell University is the lengthened shadow of a man and his wife. During the first half-century of the University they were colaborers in the best sense of the word. Their place in its history is secure.

They were a complemental pair. He was short in stature, quick-spoken, alert, even fidgety sometimes, and always masculine. She was tall (fully his equal in height), slow-spoken, tactful, and gracious. They were alike in their aims and interests, in their spirit of helpfulness toward students, in their loyalty to the University and to every good cause that needed their support. Though they had no children of their own, their house became a second home to children of others, and a place of happy social intercourse to hundreds of students. Their lives were devoted to sound learning and sane living.

All around the world today there are many for whom the choicest memories of their college years are the evenings spent in the home of the Comstocks. Mrs. Comstock would read to them choice bits of poetry and prose, while her husband, after seeing that all were welcomed and made comfortable, would sit as a listener among them. I sat with them often. It seemed to me that Mrs. Comstock's favorites were the Quaker poet Whittier, the naturalist Thoreau, and the story-teller Kipling. But she loved all good literature and her readings ranged from Emerson's philosophical essays to "funnies." Of the latter class, many will remember this one:

> A little pig with a querly tail,
> All soft as satin and pinky pale,
> Is a very different thing by far
> From the lumps of iniquity big pigs are.

After the publication of the *Manual* and *How to Know the Butterflies,* their names ceased to appear as joint authors of books, but their joint interest and mutual help went into every book that either of them wrote. He was as proud of her achievements as she was of his.

At the time of Professor Comstock's retirement from active teaching in 1914, Dr. David Starr Jordan wrote:

> His marriage intensified his influence in every way. His home became the center of nature study, as of human friendliness. Scores of youth of promise at Cornell have owed as much to the personal sympathy of the Comstocks as to anything anybody taught them in school. Not one of them—men or women—but renders grateful tribute today, not to Comstock alone, but equally to the gifted and bighearted colleague, who, as helpmeet, has kept full step with him through all these years.

They had many distinguished guests. Some were foreign scholars, visitors to the University, whose activities were in different fields. All were simply and delightfully entertained. Mrs. Comstock told me that they once had a British entomologist for a week-long guest, at a time when they were without hired help, and she was doing her own housework, both cooking and serving. The guest, wholly self-centered and oblivious to household affairs, put his shoes in the hall outside his chamber door each night to have them shined. Professor Comstock, knowing the English custom, rather than have a guest disappointed, took the shoes each night and shined them himself.

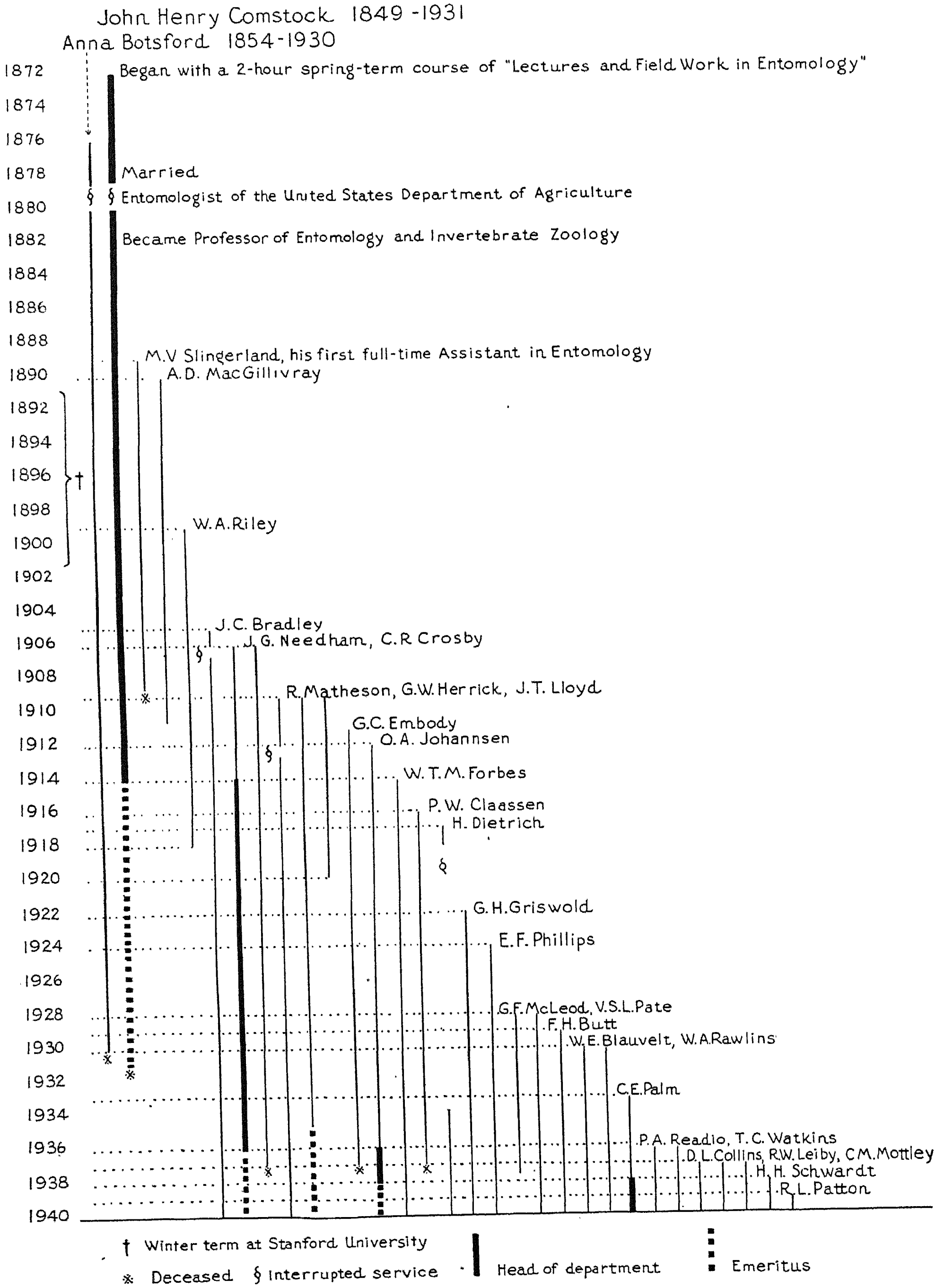
Of their home life, I will let the two friends who know them best speak:

George Lincoln Burr said: "Their tastes were congenial. . . . Their home, in which I lived for years, and which I knew well from our college days, was one of the loveliest I have ever known."

Simon Henry Gage said: "There was ever present in that home the glowing hearth-fire of human kindness."

Now, a bit more departmental history. In order to show, graphically and chronologically, the development of the teaching staff of the Department, I have prepared the accompanying tabular statement. My faculty colleague, Professor Bristow Adams, who was a friend of the Comstocks first at Stanford University and then at Cornell, has wrought

The Lengthened Shadow of a Man and His Wife



it into acceptable form for presentation. Perhaps I may be allowed to call it a *shadowgraph* because of its special function: it slants like a shadow from the two great founders. It will serve as a condensed record for the first 68 years of the University, with names and dates of entrance of the principal members of the staff of the Department down to the year 1940.

It shows at the top the long initial period of struggle for recognition, while as yet entomology had no place in university curricula. It shows that for nearly half of the years of his teaching service Comstock was without a single paid assistant. These were years of slow and steady progress. After them came a period of rapid expansion of the Department, with belated recognition of the service that Comstock was rendering. The public was beginning to learn the importance of insects in agriculture and in public health. No one of intelligence now scoffs at the study of insects. Greater financial support was coming to the agricultural colleges and their associated experiment stations. The work in nature study had turned the eyes of the children toward Cornell University, and they were flocking to the College of Agriculture in rapidly increasing numbers.

The shadowgraph does not show two shifts of boundaries that were made during the latter years of the Comstocks, when I was head of the Department. One was the shift of nature study into the Department of Rural Education. This change was made in Mrs. Comstock's time and with her approval.

The other transfer was due to change of sources of financial support. It involved vertebrate zoology. It was a double shift, first, into the Department of Entomology, and, some years later, out again; the work in ornithology under Dr. Arthur A. Allen, and that in systematic vertebrate zoology under Dr. Albert H. Wright. These two are omitted from the shadowgraph because their work was in no part entomological. As head of the Department when they were in it, I want to say that I was very proud of the work done by these two colleagues and their helpers, both in teaching and in research. The temporary association was a happy one.

There is no need that I should write of the work of my fellow-teachers who have come into the Department in my own time. That is recent history. With new men have come new courses of instruction: medical entomology, aquiculture, insect ecology, insect embryology, etc. Increase of knowledge has brought specialization. The work in applied entomology has been greatly expanded, its tasks differentiated. There is not space to speak of the work in extension, or of the great changes in the field of economic entomology; of the new methods and shifts of emphasis accompanying better knowledge of insect physiology and new discoveries in insecticides.

Among the things least changed by time is Comstock's basic course in general entomology, which was taken over first by Professor G. W. Herrick, and, after his retirement, by Dr. Robert Matheson. Most worthy of preservation in the Department is the Comstock tradition of sincerity, reverence for truth, clean living, good fellowship, and human kindness, of work for the joy of the working, and for the spread of the resulting public benefits.

So AT the end of this, my partly recorded and partly remembered tale, it comes about that the story of the development of the first university Department of Entomology is also in brief the story of the life and times and teamwork of John Henry and Anna Botsford Comstock, the like of which we shall not see again.

NATURALISTS FOR THE FOREIGN SERVICE

By KARL PATTERSON SCHMIDT
CHICAGO NATURAL HISTORY MUSEUM

THE TERM naturalist covers a somewhat vague category of persons interested in the natural sciences, either as amateurs or professionals, but in either case with the implication of a breadth of outlook that removes them from the more precisely definable botanists, zoologists, and geologists. My own definition of a naturalist as being a biologist who has traveled will not bear much inspection. It may be agreed, however, that naturalists are those persons who take a keen delight in their natural surroundings, and that at best they may combine the objectivity of the scientist with the subjective warmth of the artist. Difficult though they be to define, naturalists are well-known among us, and seem to have some capacity for self-preservation and for the propagation of their kind. Naturalists are not always appreciated by the general public, though they make the best of teachers, and, when their natural history is an avocation, may include representatives of such respected professions as banking, the law, "the cloth," or engineering.

The fundamental generic quality of a capacity for active interest in a particular environment, has suggested to me a distinctive use for naturalists. From intimate and long-continued personal observation of the United States Foreign Service, I envisage the possibility of bringing together in the public interest an important demand and need for personnel and an unrecognized source of supply. Realizing that the proposal is by no means a new one, I propose the wide use of naturalists in the field of foreign relations, and specifically in a much-needed expansion of the American foreign service at its several levels.

The inadequacy of our prewar foreign relations personnel has been pointedly established by the experiences of the second World War. In Peru I once made a thousand-mile journey to reach the nearest consulate, and in 1929 there was no official representative of the United States between the Fiji Islands and Java. In non-European and in many small countries we have for the most part been represented only at the capital city.

It therefore requires no great perspicacity to predict that the foreign representation of the United States, in diplomatic and consular posts, will require very great expansion in the postwar era now facing us with its problems. It is commonplace to predict greatly increased and world-wide air travel. On the ground, the highway from Alaska to Tierra del Fuego is approaching completion. Even New Guinea, the last great reservoir of the unknown, has suddenly become accessible. The extent of the expansion required may at present only be guessed at. At a venture, from a glance at the map, I should recommend a fivefold increase in the number of our foreign posts and a tenfold increase in personnel as a minimum, if we are to establish some real contact with other nations and other peoples.

If there is to be any expansion at all, an immediate problem arises, for the State Department is already faced with difficulties in staffing its more remote posts; and new consulates must be established in still more remote regions. At best the average American citizen does not take kindly to foreign service, and is likely to find himself bored, finally beyond the limits of his endurance, in a

tropical locality, or in any locality lacking his standards of civilization.

There are good reasons enough for disliking many a tropical situation. Monotonous heat and glaring sunshine, too frequent rains, continued cloudy weather, or too much wind may set the stage with a climate intrinsically difficult for persons adjusted to the Temperate Zone. Insect pests in unfamiliar variety and in pervasive hords may be present. The disagreeable features of a human environment often dirty, not without danger from unfamiliar diseases, and usually correspondingly and exasperatingly inefficient, added to an unfavorable climate, may readily come to dominate one's whole attitude unless there is a counterbalancing interest in something in the country itself, something that is to be found nowhere else.

Every kind of foreign service would profit if we had the means of filling the more out-of-the-way stations with persons who, far from being bored in them, would regard the opportunity to live in such places as a privilege. On the average, at least, naturalists as a class present precisely this characteristic. Boredom is so impossible to the naturalist that it seems a cardinal sin. Everything different from his environment at home offers him something to study. The very weather is a natural interest. An attempt to understand the physiography of his area may lead him on one hand into geological studies, and on the other into the inexhaustible problems of the distribution of plants and animals. We think of our world as geographically explored; but its exploration for the smaller animals, for the limits of distribution of both animals and plants, and especially for the understanding of those limits, is only just getting under way. To a naturalist it seems evident that the employment of trained or self-trained naturalists in all faraway consular positions would thus contribute to the solution of a variety of scientific problems, and would at the same time solve the fundamental personnel problem of foreign service.

At a fairly simple level, our naturalist in a foreign post need be no more than a collector, learning to collect effectively as he corresponds with the museums or with the specialists to whom he sends his collections. Collecting plants and animals may, of course, become one of the most exciting of occupations, involving all of the elements of hunting for sport, with far more significant and permanent rewards. It is grand and romantic to search for the fossil remains of the animals of past ages, or to collect a series of beetles that prove to represent a "new species," or to contribute specimens valuable in the attack on important biological problems. A dozen museums would be delighted to correspond with such amateur collectors at tropical stations, and amateur collectors frequently develop into first-rate naturalists.

My suggestion really aims much higher. We should send trained scientists to *become* naturalists in these foreign stations. Young people already trained in one or more of the biological, geological, geographical, or anthropological sciences, and already filled with the determination to devote their lives to these sciences, are available in the graduate departments of almost every university in the country. There is no reasonable doubt that young men and women of this type could fulfill the normal duties of a consular office with distinction. A university group especially suitable for the foreign service is presented by the graduate students of geography. Within my own generation, the appreciation of human geography as an adult interest has led to the establishment of a flourishing department of geography at nearly every large university.

There is a considerable tradition for the pursuit of scientific interests in con-

junction with foreign service. Now, on the threshhold of an age in which our lives are to be dominated by science as never before, such association gains vastly in importance. It may, indeed, be brought into relation with any program for the fostering of scientific research under government auspices.

My own experience points to a factor of vital importance to the success of our foreign representatives. Anyone who positively dislikes the foreign environment in which he is placed cannot well establish friendly relations with the people among whom he finds himself, and he is not likely to make the effort even to learn their language. With respect to the language, the naturalist is vitally concerned with learning it as a tool for the pursuit of his scientific studies. It has been my own familiar experience in Latin America to find that however bad one's Spanish, the attempt to speak it is seized upon as evidence of sympathy and of interest in the country itself. The naturalist who expresses specific curiosity about *anything* native to the country in which he travels finds his interest regarded by the residents as a profound compliment. His activities, in fact, afford a continuing avenue of contact with the foreign society in which he is located, extending from the urchins who bring him specimens at a centavo each to the most cultured persons encountered. I hesitate to affirm, but I have often suspected, that naturalists bear a better reputation abroad than at home in our United States.

To make my proposal concrete, it may be pointed out that the salary of a foreign service employee begins at $2,500 per annum, with certain other allowances at the more distant stations, and with transportation for himself and family. After a minimum stay of two years, his return expenses, including those of his family, are allowed. Training in specifically foreign service duties is given in a special training period, with pay. Let us suggest a three-year contract. At its termination the State Department could offer further opportunities to a staff member with considerable experience and good educational background. If the young man should decide against further foreign service, he can return to his graduate work in a university with a rich addition to his real education, and in many cases with the material for a thesis of genuine value. Above all, in the teaching career to which he may turn for his further livelihood, his foreign experience will command prestige and give him an invaluable contact with his students.

I believe that a cooperative arrangement between the graduate schools of the universities of the country and the State Department, for the supply of staff to foreign posts, would redound to the credit of both, and prove profoundly beneficial to the nation. Such an arrangement might be coordinated by the National Research Council. The possibilities of benefit both to the foreign service and to science and scientific education should receive consideration in government plans for aid to the scientific activities of the nation.

THE SCORING OF ATHLETIC CONTESTS

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In athletic contests, the word "team" is used in two different senses. In the first sense, team is a group of associates who subordinate personal prominence to the efficiency of the whole; a football team is an example. In the second sense—and a track team is an example—team is a group of individuals, each of whom has his specialty, which he performs to the best of his ability, with essentially no help from his teammates.

For teams in the first sense, either comparative scores or absolute scores are practically meaningless. Varying excellencies in different departments of play make it a common thing for team A to beat team B, B to beat C, and C to beat A. Further, the subordination of personal prominence must be so complete that an individual must, for the sake of his team, be ready to perform in a manner inferior to the best he can do. Examples are the intentional base on balls in baseball, and the intentional safety in football. We conclude that in football, baseball, lacrosse, hockey, basketball, soccer, and polo the scoring systems involve little that is worthy of scientific study. And we note explicitly two paradoxes:

The Ring-Paradox. This consists in A beating B, B beating C, and C beating A.

The Control-Paradox. This consists in an inferior performance by which defeat is turned into victory.

As examples of teams in the second sense, we have track, cross-country, swimming, boxing, tennis, golf, gymnastics, fencing, wrestling, rifle, winter sports, and the like. In many of the events which occur in these sports, the whole idea is to get somewhere first, or to throw something farthest. The situation is more complicated in boxing, wrestling, tennis, the fancy dive, fencing, and form in ski jumping, but that need not concern us here. It is true that in running a race one may be bothered by his opponents, or helped by a teammate, but this influence is on the whole small, and in a study of scoring systems may be disregarded. Further, to simplify the situation, we omit entirely the human element. In this discussion, our runners must always run a race in the same time; our jumpers must always jump the same distance. All contestants are reduced to automatons who always give their best performance, and this best performance is a constant, unless—and this will probably seem unlikely to the reader—the control-paradox occurs in the scoring systems, and our contestants take advantage of it.

Now when representatives of two teams run a mile race, it is easy to determine the winner. He is the man who finishes first. In a javelin throw, it is the man who throws the javelin the farthest. But when we come to combine a mile race with a javelin throw, we introduce complications. Shall we merely score the winners (as the British do)? Or shall we count seconds and thirds (as is common in the United States), and, if so, how much? Shall a superlative throw of the javelin count a little more than a narrow victory in a rather mediocre mile?

We must have definitions. Quite generally we may say:

A *team* consists of individuals, whose individual performances contribute to a common score, with the purpose of establishing the general superiority of their team over one or more other teams.

An *event* is an athletic contest in which one or more individuals from two or more teams participate.

A *meet* is a group of events. Its purpose is to determine the order of general excellence of the teams involved.

A *scoring system* is the technical device by which this order of general excellence is established. It is assumed to be impartial, and adapted to the peculiarities of the events which it measures. The independent variables of a scoring system are the times, distances, orders, etc., of the individuals in an event. Using these variables directly, or others dependent on them, the system assigns *event-numbers* to each team in each event. It is assumed that the event-numbers monotonically increase or decrease with team excellence in that event. It is assumed that if "increase" is the pattern in one event, it is the pattern in all events. It is finally assumed that the event-numbers for a team are merely added together to give a *meet-number*. Order of excellence is established by the order of the meet-numbers.

The essence of a scoring system is the method of assignment of the event-numbers. These methods may differ radically in the orders of excellence which they establish. Suppose 2 golfers A and B play 3 holes, and suppose their scores are:

A	7	3	3
B	4	4	4

In the scoring system called medal play, these numbers are themselves the event-numbers, and B beats A 12 to 13. In the scoring system called match play, the significant thing is winning the event, and for this the event-number unity is given; hence the event-numbers are:

A	0	1	1
B	1	0	0

and A beats B by a score of 2 to 1 (technically one-up). On the evidence furnished, B is better than A at medal play; A is better than B at match play; and the question as to who is the better golfer, being undefined, is still unanswered.

The reader will note a close analogy between this and the election of a President of the United States. The Electoral College (match play) yields a result which may be quite different from the popular vote (medal play).

But note that match play involves the ring-paradox, whereas medal play does not. A single example suffices to show this. Suppose 3 golfers play 3 holes—remember they are automatons—and A has scores of 7, 3, and 4; B scores of 2, 4, and 6; and C scores of 3, 5, and 2. Then, in dual match play we have these results (the winning score is italicized):

A	7	*3*	*4*	B	*2*	*4*	6	A	7	*3*	4
B	*2*	4	6	C	3	5	*2*	C	*3*	5	*2*

Thus match play can produce the ring-paradox; and obviously medal play never can. But, in my judgment, this is no valid reason for saying that therefore medal play should be preferred.

The ring-paradox is, in fact, inherent in many more systems than is generally supposed. Consider a cross-country triple meet (actually a meet of 1 event) in which 5 representatives of each of 3 teams compete. The only thing that matters is the order of the finish. The winner is assigned the number 1, the next man the number 2, and so on. The numbers of the individuals on a team are added for a team score, and low team score wins. Suppose the order of the finish is:

A	B	C
1	6	2
4	7	3
5	8	11
14	9	12
15	10	13
39	40	41

We can now say that in a triple meet, team A is best. But it by no means

follows that we can draw any conclusions as to what will happen in dual meets. A glance at the numbers shows that in a dual meet between A and B, A will take the first 3 places, B the next 5, and A ninth and tenth. The results of 3 dual meets are:

A	B	B	C	A	C
1	4	3	1	1	2
2	5	4	2	4	3
3	6	5	8	5	6
9	7	6	9	9	7
10	8	7	10	10	8
25	30	25	30	29	26

an excellent example of the ring-paradox.

The question of which scoring systems involve the ring-paradox and which do not can be completely answered after an analysis of the following ring-paradox, the simplest that exists. Three teams consist of a single individual each, and they compete in 3 events. In event (1) A always wins, then B, then C. In event (2) the order is B, C, A; in event (3) the order is C, A, B. Now consider these 3 dual meets, in which winner scores 1 point in an event:

	A	B	B	C	A	C
(1)	1	0	1	0	1	0
(2)	0	1	1	0	0	1
(3)	1	0	0	1	0	1
	2	1	2	1	1	2

This gives us the clue. In (1), A is one better than B, B is one better than C; yet A is only one better than C. Somehow, the scoring system should make A *two* better than C. More generally, it can be demonstrated very easily that if the symbol S(A, B) means the amount by which A's event-number exceeds B's event-number, the necessary and sufficient condition that a scoring system avoid the ring-paradox is that the following functional equation hold:

$$S(A, B) + S(B, C) = S(A, C)$$

The reader familiar with scoring rules can now verify that the ring-paradox is inherent in the following: track, cross-country, swimming, gymnastics (American rules), winter sports, boxing, tennis, fencing, wrestling, and match play at golf. The ring-paradox can never occur in rifle, bowling, gymnastics (Canadian rules), medal play at golf, the pentathlon, and decathlon.

Can scoring systems be devised which would eliminate the ring-paradox? It is easily demonstrable that to do this the system must produce an *absolute* rating S(A) for each team in each event: that is, S(A, B) = S(A) − S(B). An absolute rating in boxing, tennis, fencing, or wrestling is absurd. An absolute rating in golf simply means that you discard match play for medal play; and in gymnastics it means that you adopt Canadian rules. In track, cross-country, swimming, and winter sports, absolute ratings are possible, but in my judgment are highly undesirable. In a track meet it would require the determination of the time of every runner by electrical devices at least to the nearest .01 second. It would mean an appalling amount of mathematical computation. The winner of a close race would not be sufficiently rewarded. And most unreasonable and intolerable is the fact that team B might win 14 first places and 15 second places in 15 events, and yet be beaten by the superlative effort of one individual on team A, performing in only 1 event. The ring-paradox is much easier to endure than these absurdities.

The control-paradox is not as common as the ring-paradox. Its actual occurrence in the sports listed requires (*a*) two mutually antagonistic systems unwisely interlaced, or (*b*) a single illogical system. Several examples of (*a*) are known to exist; they are more amusing than harmful. I know of only one example of (*b*), a method used in scoring the *slalom* in skiing.

As an example of control-paradox,

variety (a), consider a "meet" which consists of a triple track meet with its 15 events in which first, second, and third count as usual, 5, 3, and 1; and in addition a sixteenth event, a cross-country run, in which the winning team is determined in the usual way, and then 5, 3, and 1 points are arbitrarily assigned as event-numbers. The cross-country race is held last. Prior to its running, the scores are:

A	B	C
48	45	42

In the cross-country race, the sixteenth event, the order of finish is:

A	B	C
9	2	1
12	5	3
13	6	4
14	7	10
15	8	11
63	28	29

Whereupon team B is awarded 5 points; C, 3; and A, 1. These added to the totals above give B, 50; A, 49; and C, 45.

But suppose the A-man who finished ninth, had sat down on the grass, and waited for the two C-men, whom he could have beaten, to finish ahead of him; and then casually strolled across the finish line. We should then have:

A	B	C
11	2	1
12	5	3
13	6	4
14	7	9
15	8	10
65	28	27

By this maneuver, team C, which was not the dangerous competitor, is handed the victory, and gets 5 points, B gets 3, and A is no worse off with 1. The final score is:

A	B	C
49	48	47

Thus by performing as badly as is humanly possible, team A was able to turn defeat into victory. The ability to "control" the scores of adversaries is widely prevalent in triple meets, but in general a team which exercises this control loses more than it gains. This example is an unusual exception.

A scoring system, widely used in winter sports in the *slalom,* downhill, or *Langlauf,* inherently contains the control-paradox, variety (b), even in a dual meet. In this system, the winner (the man with the least time) is given 100 points. Every other competitor is given a fraction of 100 points, the numerator of the fraction being the winner's time, and the denominator, his own time. Now consider this situation for two 4-men teams:

Team A: Time	Points	Team B: Time	Points
79	100.00	80	98.75
81	97.53	130	60.77
82	96.34	131	60.31
83	95.18	132	59.85
	389.05		279.68

The totals (or, in practice, one-quarter of the totals, which does not affect the situation) are defined as the event-numbers, and A is ahead by 109.37 points. But suppose the winning A-man had slowed down to 80 seconds. The score would then be:

Team A: Time	Points	Team B: Time	Points
80	100.00	80	100.00
81	98.77	130	61.54
82	97.56	131	61.07
83	96.39	132	60.61
	392.72		283.22

Team A is now ahead by 109.50 points. Thus if the winning A-man is willing to give up his personal victory, and accept a tie for first place, he can thereby strengthen his team's score, and in a close meet turn defeat into victory.

But although the paradox exists whenever one team is sufficiently superior to the other, it does not seem to be of much practical importance. In fact, when a winter sports meet involves 10 or 12 teams, the extraordinary amount of calculation involved in this method seems (at least to one who has often assisted in scoring) a more valid objection than the hazard of the control-paradox.

In conclusion, in judging any scoring system, or in formulating any new one, the first question to ask is: How many men do you want to have contribute scores in any event? Meets in boxing, tennis, fencing, wrestling, and golf usually consist of several 2-man events, and the winner alone contributes to the score; 1 point for the winner and 0 for the loser seems to be about as good a solution as any. In track, the British answer is 1, the winner. The American answer is 3 (or even 5 in a multiple-meet), and the desire for "balanced" teams has led to an increase in the value of seconds and thirds. Formerly, the system 5–2–1 was used; now it is generally 5–3–1, and in multiple-meets, 5–4–3–2–1. In swimming and gymnastics, 3 places count in this country. There seems to be no valid reason for changing any of these systems.

In cross-country, the answer is different, as 5 men on each team are to contribute to the score. The emphasis here is on team balance. In a multiple-meet of 15 or 20 teams, it is not unreasonable for no member of the winning team to finish among the first 10. Again, it is perfectly possible for a team to have 4 men finish first, second, third, and fourth, and yet lose the meet through the indifferent performance of the fifth member. These facts are well-recognized, and are not considered undesirable. Further, a cross-country meet is unique in that it is a meet with only one event, and therefore it is reasonable to keep the scoring system which is now used.

American winter sports meets constitute the real exception. In each event, 4 men on each team are to contribute to the score. No other sport has ever put such emphasis on team and balance, and, in consequence, no other sport has ever faced such difficulties in formulating a scoring system. There is no need for reviewing all the systems which have been tried. They are all complicated, as indeed they always must be. A good scoring system should be capable of easy explanation to the judges, contestants, newspaper reporters, and general public. It is desirable to have a system which can be kept up to date as a meet progresses, and which will yield the final results soon after the end of the meet. But when 3 competent scorers, all professional scientists, equipped with slide rules and electric computing machines, require 4 to 6 hours to determine the winner, I would venture the categorical statement that the scoring system is a bad one. Further, if the performances of 4 men on each team are to be considered significant, I know of no system which could prevent this situation: team A finishes first, second, and third in every event, and also finishes fourth in every event but one, and yet loses the meet. These difficulties are very real, and they greatly outweigh the minor abnormalities of the ring-paradox and the control-paradox. The proponents of winter sports believe 4-man teams in each event are desirable, and they have good arguments for their position. But they must always pay a high price in the complex and undesirable features of any possible scoring system.

SOCIAL PARASITES AMONG BIRDS

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THE WAYS of a parasite are opprobrious to men, who subconsciously judge them by their own code of ethics. We are pervaded with a sense of fair play and by democratic ideas of equality of opportunity to such a degree that the animal parasite, as indeed the predator, tends to assume a sinister aspect. No technical biologist will admit anthropomorphic bias in interpreting the actions of animals; but, as rigidly mechanistic as he may be in his explanation of their behavior, we note his interest is often piqued by a situation which by human standards is irregular or insidious.

In the unmoral animal world, success rather than virtue is the keynote, success in maintaining life and in reproducing. Any means to these ends is biologically acceptable if it works. Parasites display remarkably neat methods of attaining their objectives, methods quite as elaborate as those of free-living animals. But a parasite makes sacrifices in becoming dependent on another species. It narrows its own evolutionary possibilities and limits its expansion in numbers. It casts its fate with its host and it cannot overuse its "meal ticket." The host's welfare as a species is of especial importance to the parasite, and it is usually regarded as axiomatic that the most successful parasites are those that do not cause the early death of their hosts.

Parasitism as an animal industry has offered many openings—opportunities for developing the art in diverse ways. Like any unoccupied ecologic niche or industry, it becomes filled by progressive modification of pre-existing free-living animals. Similar kinds of parasitism have evolved independently a number of times, each arriving at a similar end state by different routes and from different starting points. Such has been true of social parasitism among birds.

Social parasitism is in some respects a poor term for what I describe, but it is now well-accepted English usage. It refers to the parasitism of the nest of one species by birds of another species, and the consequent dependence on the host to incubate the parasite's eggs, and to raise and protect its young. Strictly speaking, this is not parasitism of the society of which an individual is a member because it is not an intraspecific affair. Only in the sense that many bird species are conceived as forming one ecologic society is the term justifiable. The German term *Brutparasitismus* is, on the other hand, entirely accurate.

The fact that the European cuckoo is parasitic was known in Aristotle's day, but lack of precise information about its activities prevailed until two decades ago, in spite of the great fame this bird gained from its unusual reproductive methods. Misconceptions are evident in the meaning of the word "cuckold," based on the cuckoo's supposed habits. The implication in this word is that the female cuckoo is unfaithful to her mate. There is not much good evidence to show that she is; she merely passes off on someone else her domestic duties of raising young, while she spends her time in the job of finding the nests of victims. Some modern society women do much the same thing with their own children. In fact, the term social parasite might more correctly be applied to them, for such women are parasitizing

their own society, their own species—but this observation may have theromorphic bias.

Not until the early nineteenth century was it discovered that some birds other than cuckoos are parasitic, and only in the past few decades have several more groups been added to this category. We now know of parasitism in five separate families of birds, belonging to four distinct and not closely related orders. Obviously it is an independent development in each group, stemming from ancestors that had normal nesting and brooding habits. The groups are: the cuckoos of the Old World (not the North American cuckoos); the honey guides of Africa; the cowbirds of North and South America; the weaver finches of Africa; and the ducks (the Argentine black-headed duck only).

To understand the origin of parasitism we must review a few elements of normal breeding behavior.

(1) In a great many birds, as the nesting season approaches, interest is taken in a particular area in which the nest later will be built. This area, or focal points in it, is defended, especially by the male, against others of the same species. It is called specifically the "territory" and it serves variously, according to the species, one or all of the following functions: protects the mate and isolates the pair, thus preserving the bond between the members of the pair and in general promoting monogamy; protects the nest from molestation; assures a food supply—an adequate source area for food—for the raising of young.

(2) Either before or after establishment of territory by the male, pair formation takes place in monogamous species. Nuptial display, common interest in a territory, courtship feeding, and nest-building may serve to strengthen this bond, which usually is held in force for the period in which a brood of young is being raised. The sexes seem to be recognized initially either by characteristic marking or by behavior, but recognition of the mate soon becomes a matter of individual recognition, of which birds are fully capable.

(3) Nest construction takes place after pairing, usually long after territory is established, although in some species the establishment of territory may follow upon selection of the nest site and consist of an expansion of the sphere of protection centering at the site. Nest construction may be engaged in by both male and female or by the female alone. It stimulates sexual activity, and parts of the building activity may be taken over as an element of courtship display, thus at times contributing not at all to construction of the nest. The architecture of the nest is usually characteristic for the species. In its broad aspects, and even in many details, it is hereditarily determined, as are most actions of birds.

(4) Ovulation, or laying, is not purely a culmination of a seasonal reproductive cycle set in motion by the endocrine system, in timing with daylight or other external cycles, but is further dependent in most species on the stimulus of courtship, nest construction, sight of and feel of the nest, and immediate conditions of food and weather.

(5) Once the eggs are laid, the chain of subsequent activity, of brooding, of feeding young, and of protecting them, follows, governed by internal events in the endocrine system and by later external stimuli, such as the hatching of eggs and the begging of young. The male responds to these situations as a result of external stimuli; yet in some species his activity in brooding and in raising the young may be even more intense than that of the female.

By its brevity the foregoing sketch does violence to many special fields of behavior study, and would require much elaboration and some qualification for

any particular species. However, it may be thought of as applying fairly well to most small songbirds and to the ancestral types in several groups that have evolved parasitism.

Steps in the modification of instincts in the cowbirds fortunately can be traced in part through two South American species (Friedmann, *The Cowbirds,* 1929), one of which verges on parasitism yet still raises its own young. The other has an imperfect parasitic procedure, probably but recently evolved. The first of these is the bay-winged cowbird (*Molothrus badius*), of Argentina. It pairs up in the spring and is strictly monogamous, each pair sorting out of the winter flocks and going its own way. But instead of establishing territories before, or even following, pairing the pair wanders about looking for old or empty nests of other species. Often they fight with the rightful owners and usually are successful in ousting them. They even throw out the eggs and young from occupied nests. Having thus taken possession of either an old or an occupied nest, they lay their own eggs, incubate them, and raise their young. After acquiring a nest, they establish a territory radially around it and proceed to renovate the nest, sometimes adding significantly to it. Indeed occasionally they build their own nests and do quite well. Thus they have not lost the instinct to construct. It should be pointed out that the family to which the cowbirds belong is noted for its elaborate, well-built nest structures (the New World orioles are members of this group).

The significant departure from the norm in this species seems to be an upset in timing, that is, an aroused interest in nest structure before a home territory is established. There is overconcern for nest structures per se and special susceptibility to the stimulus of the sight of a nest of whatever kind. Territorial instincts are not only late in manifestation but are in general not strong. One further point to be noted is the weakness of the brooding and feeding instincts of the females; the male does the major share of the work at the nest.

The screaming cowbird (*Molothrus rufo-axillaris*), of the same geographic area, is closely related to the bay-wing, and, according to Friedmann, principal student of these birds, has certainly been evolved from it. Screaming cowbirds are monogamous, and may even remain in pairs throughout the year. They establish territories but defend them very weakly, often tolerate other pairs, and may shift about from one place to another during the early part of the spring season. Egg-laying is long delayed; and, when it does finally occur, the only nesting activity of other species still in progress is that of their relatives, the bay-wings. They then lay in the bay-wings' nests, parasitizing this species solely.

"Assuming that in most ways the original habits of the screaming cowbird were similar to those of the bay-wing, we would expect that the birds tried to [take over and] breed in nests of ovenbirds, woodhewers, etc. . . ., but tried to do so early in the season. . . . The struggle for nests is much greater [then] . . . than later on, and the screaming cowbird, handicapped hereditarily by a weakened territorial instinct, probably could not succeed in this struggle. . . . [The frequent desertion of territories by this species] indicates very strongly that the weakened territorial instinct of the male often is insufficient to maintain its influence long enough" for the egg-laying of the female to follow in normal sequence. With no adequate defense of a stolen nest, the female laid in nests the pair attempted to acquire. As in the ancestral bay-wing, the female was weak in her brooding instincts, and now in this species the protecting and brooding instincts of the male also became weak-

ened and the bond between pair and nest broken. The eggs were then left to the foster parents, who, never having been driven away effectively, returned and raised the parasite and perhaps some of their own young. The screaming cowbird was delayed until late in the spring because of the difficulty in getting at nests. At that time the nests most available were those of the bay-wing, who was itself none too aggressive in defense of its nest. The screaming cowbird thus could most readily succeed in parasitizing this species and is now found to do so exclusively.

In the North American cowbird (*Molothrus ater*), as in several South American species, parasitism is developed more perfectly. In these, territorialism has in effect disappeared. There is, to be sure, interest in or attachment to an area but no defense of it. When many cowbirds are present in a region, they are tolerant of one another. Also, they become promiscuous, or, more usually, polyandrous, for the males outnumber the females in these species. Further, they parasitize a wide range of species—kinds almost always somewhat smaller than themselves. There is no host specificity.

Thus, pair formation, territorialism, nest construction, brooding, and feeding instincts have all been lost. I think of a group of cowbirds that lived about a mesquite thicket where I camped one spring on the Tucson desert. A single female was the center of the show. Six males were in constant attendance, squeaking, and ruffling their feathers in courtship antics; they were not effectively aggressive toward each other. As the female flew from one tree to another, the flock of attendants moved with her in close formation; they seemed to be mobbing the female, as crows would an owl. For the female this looked, anthropomorphically, to be a carefree, though hectic, existence.

The devices of parasitism which in a sense replace the lost instincts are several. There seems no doubt that the female cowbird will be able to mate. The problems for the species now become: (1) finding nests in proper stage for parasitism; (2) insuring that the host is not disadvantageously disturbed by the deposit of the foreign egg; (3) insurance that the young will be fledged by the foster parents in competition with their own young. Several adaptations claimed to be effective in accomplishing these ends have with recent, more precise observation proved to be nonexistent. Only recently has the laying and egg-removing action of the female cowbird been exactly recorded.

The story runs as follows: The female takes a sharp interest in nest-building of other birds, and is stimulated by sight of this and perhaps by their courtship. On her beat, she finds numerous nests by observation of building activity and by special search in the plant cover. Not only are these found, but also they are visited and inspected. When all this interest induces ovulation, she has a supply of nests to use and is in touch with laying activity in them. She usually lays only after one egg of the host has been deposited.

Until eight years ago there had been reported only one satisfactory observation of a North American cowbird sitting on a nest, laying; cowbird eggs merely were known to appear in the nests of hosts. They were easily recognized by their characteristic markings. Patient work on the part of Hann (*Wilson Bulletin*, 1937, 1941) gave the clue to the difficulty. The cowbird lays exceptionally early in the morning before the host lays for the day in question. Normally, host birds do not stay on the nest at night until the clutch is nearly or quite complete. The cowbird, then, gets to the nest before the owner. Hann reports it was usually impossible for him

to make out the form of the cowbird when it was quiet, in the dim light. But his photoflash records tell the story. This early visit produces a minimum of disturbance at the nest. The egg can be laid in thirty seconds, and the hosts may be unaware of the unhappy event.

The practical difficulty of knowing in advance what nest the cowbird will visit can well be imagined. What nest shall the faithful ornithologist sit by in the dark before dawn? Some clues were found that aided materially. From study of the eggs in the nests of host species in a restricted area and with knowledge of the sequence of their appearance, it could be established that a female cowbird laid once a day, four or five days in succession; these groups of eggs, incidentally, correspond to the clutches of the same number laid by the nonparasitic bay-wing. If the observer has been as keen as the cowbird in finding nests, he will know which nests on the cowbird's beat will be in a favorable stage for parasitism. Further, Hann found that the cowbird usually made a close inspection of the nest of the intended victim on the afternoon of the day before. This action, observable in broad daylight, if one is at the right place, serves as a guiding sign, although it is not infallible.

Contrary to the belief held before Hann's photographic work, the cowbird does not remove an egg of the host from the nest when she visits to lay. Instead she takes it on the afternoon before, or later in the morning of her laying. This sneak visit, of course, can be very rapid and again will not cause prolonged disturbance of the hosts. About 85 percent of the time this removal takes place. Accidentally, the remaining eggs of the host may be damaged by the claws of the cowbird as she settles in the unfamiliar nest.

An egg is removed only if two or more eggs of some sort are present in the nest when the visit is made. This instinct helps to insure that the cowbird will not remove her own egg; for, in an emergency, cowbirds may lay before any of the eggs of the host have been deposited. Only rarely does the cowbird make a mistake and remove her own egg or that of another cowbird that might have found the same nest. The eggs which are taken are speared with the bill and carried away to be eaten or crushed.

Host reaction is a subject about which much more information is needed. Once the disturbance of the cowbird's visit has passed, most hosts show little concern. We have some evidence that they are aware of the foreign egg, but seldom are they able to remove it; nor do they persist in efforts to do so, even though the egg may be twice the size of their own and strikingly different in pattern. Sometimes the host deserts the nest and starts afresh somewhere else. Certain species are much more readily disturbed than others, but qualitative data on desertion are sadly lacking. One reaction often seen in parasitized warblers is reconstruction of the nest, with a false bottom placed over the first set of eggs containing that of the cowbird. The warbler, of course, then lays another set.

Young cowbirds hatch with the host young, call lustily for food, and get it. Young of some species, such as orioles and warblers, may fail in competition; those of others, such as song sparrows, receive enough food to develop normally. Though larger than its host nestmates (sometimes 50 percent or more), the young cowbird develops rapidly enough to be ready to leave the nest with its associates. Like them, it is fed for ten days to two weeks subsequently and then is independent. Since most small birds feed their young on insects, young cowbirds are likely to receive an adequate, nutritious diet, regardless of the specific menu of the host. Certainly they are not fastidious.

Overenthusiasm in the search for adaptive devices and the influence of knowledge about cuckoos have led to some false ideas concerning the cowbird's special abilities in parasitism. It has been claimed that its eggs are unusually small, the better to resemble those of the hosts. In comparison with nonparasitic members of its family, the cowbird's eggs are no smaller; they average about 9 percent of the body weight. There is no increased thickness of the egg shell in adaptation to resisting breakage by the host during attempts at removal. The most persistent and favored notion has been that the cowbird's egg hatches before those of its host as a result of a shortening of the developmental period of the embryo. Again, the cowbird has speeded up the process not at all in comparison with its relatives, the American blackbirds. Its incubation period of eleven or twelve days is short, shorter than that of some of its hosts but identical with most of them. The cowbird's ancestral stock possessed certain characteristics helpful in parasitism, such as egg size and short incubation period. These properly are viewed as preadaptations. They seem not to have been enhanced in the slightest since the birds became parasites. Young cowbirds display no antagonism toward their nestmates. They make no efforts to pick at them, throw them out of the nest, or sit upon them. We shall see how this contrasts with certain cuckoos.

One study of the success of the North American cowbird has been pursued extensively enough to yield a reliable picture (Nice, *Trans. Linnaean Soc. of New York*, 1937). In the Middle West, song sparrows (*Melospiza melodia*) are favored hosts. Figures for survival in about 100 nests show 32 percent of the cowbird eggs laid were hatched and the young fledged. This compares with 36 percent, in general, for song sparrow eggs in nonparasitized nests. The lesser figure for cowbirds may be attributed to occasional desertions by the song sparrows as a result of the appearance of the cowbird egg. Broods of song sparrows raised in nonparasitized nests averaged 3.4 young per nest; in parasitized nests, 2.4 young. Hence, each cowbird would seem to have been raised at the expense of one song sparrow.

The European cuckoo (*Cuculus canorus*) is more complicated in its devices for parasitism. Striking is its well-developed, though not always perfect, host specificity. Different tribes, or gentes, exist within the species, even in the same area, each adherent to a different host species and each specialized in at least one respect for that one species. Thus in England there are cuckoos parasitic on meadow pipits, and others that are tree-pipit cuckoos, hedge-sparrow cuckoos, or pied-wagtail cuckoos.

Much of the knowledge about these cuckoo tribes has been derived from close examination of the egg pattern. The situation is analogous to that in fingerprints. There is so much variability in spotting and coloration, but constancy in the product of one female, that individual identification often can be made, and the egg-laying history of particular birds can be satisfactorily followed if one persistently searches out the nests of all conceivable hosts in a restricted area. But, in spite of the remarkably wide range of individual variability of egg pattern, there are common elements in the pattern of each tribe, or gens; and these mimic in some considerable measure those of the host species of that gens. Often the mimicry is so exact that the cuckoo egg can be distinguished on first inspection from those of the host only by the texture of the egg surface or by the thickness of the shell.

Edgar Chance (*The Cuckoo's Secret*, 1922; *The Truth about the Cuckoo*, 1940) deserves credit for establishing

many critical facts about cuckoos. He followed the activities of a single female cuckoo for five seasons, finding her strictly limited to the meadow pipit (*Anthus pratensis*) as a host, except when he manipulated the supply of usable nests and was thus able rarely to force her to try other species.

Such a cuckoo maintains a definite territory, driving off other females; and she seems, as far as is known, to have one male in attendance—a principal spouse, if not an exclusive one. Maintenance of the egg mimicry of each tribe would require that the male be a member of the same tribe. We do not know how egg patterns in cuckoos are inherited, but it is highly probable that the pattern factors are transmitted both by male and female, not exclusively through the female in the *Y* chromosome peculiar to that sex. Under such conditions, the interest of the male in the same host as that concentrated upon by the female would be important. Definite pairing, within the gens, and territorial establishment in territories of the host species would tend to hold the gens intact and preserve the mimicry of the eggs in the tribe. Mimicry has been observed to break down in individual instances, we may suppose either because of cross-mating between gentes or through failure of a female to find a nest of her normal host when she is ready to lay. Presumably, the more frequent elimination of these misfit eggs by the hosts supplies the selective pressure that maintains the mimicry.

Territorialism in the cuckoo has yet another advantage. Only one cuckoo can be raised in a given nest, owing to the special reactions and requirements of the young. Hence, if two cuckoos lay in the same nest, one egg, or the young hatched from it, is sure to fail. In the economy of the species it is disadvantageous for females of the same gens thus wastefully to duplicate effort. Territorial antagonism insures that this will not take place.

We may return for a moment to speculate on how it happens that young cuckoos return to parasitize the host species by which they were raised, for they must usually do this or the entity of the gens would be lost. Possibly cuckoos of different gens respond differently to the call notes, sight, and other actions of various potential hosts. That is, there may be some inherited recognition reaction peculiar to each gens. More likely, however, is the view that recognition of the host species is learned by each cuckoo in the long period when as a juvenile it is fed by that host. Attachments to animate objects other than their own parents, which are formed early in their lives, have been repeatedly observed in captive young birds of species with normal breeding behavior. It is to be expected that the migrant yearling cuckoo returning to its home range would evince an interest in the actions of its foster parental species, respond to its notes, and follow the course of its nuptial and nesting procedure. The response to its cuckoo mate, on the other hand, would have to be purely a matter of instinct—an inherited affair.

Mr. Chance devised a plan of study wherein he destroyed the nests of the host within the territory of the cuckoo on such a schedule that only one nest would be available in the proper condition when the cuckoo was ready to lay. Host specificity and territorialism made this feasible in the cuckoo, whereas it would not be so with North American cowbirds. By cultivating and managing the nest supply, Chance was able to be at the nests when cuckoos laid and to take critical motion pictures.

The following are the events in his story: A female European cuckoo lays every other day in the afternoon. The appropriate nests are found by watching and are visited in advance of laying,

much as in cowbirds. Before laying, the bird sits motionless on a lookout for an hour or two. This long period of waiting and attention to the intended nest is evidently the period when the egg is moving to the lower part of the oviduct; in fact, probably into the terminal chamber, the cloaca. She is then ready for a rapid and remarkable delivery. A few seconds before laying she glides down to the nest without wingbeats, in a peculiar, steady glide, suggesting that any undue exertion in the air might cause premature ejection. She alights on the nest and, because usually too large for it, crouches flat over it and drops or rolls the egg into the nest cup. In domed nests the cloacal area is merely pressed to the entrance, while the bird flutters and clings and the egg is projected inward.

Two attributes of the egg doubtless are important in this connection. First, it is small for the size of the cuckoo. It is but 3 percent of the body weight, compared with a normal of 9 percent. The laying of a small egg should be easier to control and to accomplish rapidly. Seven or eight seconds is the minimum time taken. Second, the egg is thick-shelled so that it can withstand rough treatment in deposition. Both these features are otherwise important in mimicry and in withstanding host attacks. The thick shell may have been an attribute that appeared incidental to reduction in size. The runt eggs occasionally laid by other birds are usually thick-shelled. The shell glands are equipped to deposit a certain amount of shell; if, then, a small ovum is presented, these glands seem to overload it with shell.

One of the most persistent myths about the European cuckoo is that it lays its egg on the ground and then carries it in the bill or within the mouth and thus places it in the host's nest. Several bits of circumstantial evidence have contributed to this untenable belief. A cuckoo takes an egg from the host's nest as it arrives to lay, not before or after, as in the cowbird. While laying, it holds the host's egg in its bill and, following laying, it flies away conspicuously carrying the egg. The laying of its own egg is so rapid, and direct laying in domed nests seemed so unlikely, that lack of critical observation and the improbability of the true action maintained the erroneous belief. Chance's motion pictures of the behavior were decisive.

An extraordinary behaviorism, which has been repeatedly verified, is the action of the young cuckoo in ejecting its nestmates. Soon after hatching and while blind and largely naked, the young cuckoo thrusts itself beneath any object it comes in contact with in the nest, balances the object, young or egg, on its peculiarly flattened and depressed back, braces it with its stubby wings thrust backward and upward, and clambers with its load up the edge of the nest. At the rim it gives a sudden lurch and pitches its nestmate out. This instinct subsides after about four days, as found by testing older cuckoo nestlings.

Expulsion of nest competitors as a refinement in parasitism is doubtless especially necessary in a bird the size of the cuckoo. So large a bird could scarcely keep pace in development with the smaller host young. Also, if it is to grow adequately it must have the entire food ration which the small host species can deliver. Ludicrous indeed is the feeding of the giant cuckoo when it has attained its full growth, is out of the nest, but is still dependent. Small warblers which may be its foster parents have been seen sitting on its shoulders or clinging to its neck in order to reach its gaping mouth with the food. The hosts are occupied so long with raising the cuckoo that there is no time for subsequent nesting and, accordingly, they may raise none of their own young in that season. The parasite is here a

serious drain on the propagation of the host species.

The American cuckoos afford some clues to the origin of parasitism. In the nonparasitic members of the cuculine order, one notes instances of weakness in nestbuilding instincts—the nests are poorly built almost to the point of inadequacy. Irregularity in laying is another feature. Eggs are deposited at long and various intervals, as in the case of the California road-runner, where they may be laid in the nest after young from the first eggs have hatched and are half-grown. The timing of the series of reproductive instincts is thus poor. This leads to the occasional deposition of eggs in nests of other species of birds when their own nests have, through inadequacy, become destroyed, or when eggs are produced after their own nests are crowded with young. One group of species, the Anis, have become communal in their nesting, several pairs building, tending, and laying in a single nest. The communal group as a unit is territorial, driving off foreign members of the species.

There is ample background then for development of parasitism through enforced scattering of eggs; such procedure, if successful in yielding young cuckoos, might comparatively soon become the normal pattern of action. Unlike the cowbirds, the breakdown of normal behavior was not loss of territorialism but abandonment of eggs. This type of parasitic evolution has been termed *egg parasitism* to contrast with *nest parasitism* wherein the cowbirds, with diminished interest in territory, became overconcerned with nests and initially stole them for their own use. The end stages, as we have seen, have much in common, although they are not as similar in detail as was once supposed.

Resort to parasitism under several sets of circumstances has not presented great evolutionary difficulties, although its appearance seemingly has depended on the presence of some essential preadaptations that chanced to be available. Having once taken the decisive step and become dependent, the parasitic species may have added refinements, such as rapid delivery of eggs, killing of competing young, and mimicry of egg pattern. In these respects cuckoos are vastly greater specialists than cowbirds, but they probably have been practicing parasitism for a much longer time.

THE DEVELOPMENT OF THE CONCEPT OF HEAT—II*

FROM THE FIRE PRINCIPLE OF HERACLITUS THROUGH THE CALORIC THEORY OF JOSEPH BLACK

By MARTIN K. BARNETT

Development of the Caloric Hypothesis. As a matter of fact, the caloric theory was scarcely in need of the support coming to it by virtue of the popularity of Newton's emission theory of radiation, for the hypothesis of an imponderable heat fluid was already proving most fruitful in explaining "mixture experiments," as well as the phenomena of liquefaction and vaporization. And if these investigations appear as overwhelmingly important not only for the heat concept but for thermodynamics generally it is precisely because, in them, attention is concentrated on that particular aspect of changes which we now characterize as "their initial and final states," an attempt being made to define the imponderable heat fluid in such a manner that it will appear as quantitatively conserved in the processes in question.

"Mixture experiments" were not new to the eighteenth century; indeed, Renaldini (1694) had already employed mixtures of boiling and freezing water in varying proportions for the purpose of thermometer graduation. However, the mixture experiments of Fahrenheit, carried out at the request of Boerhaave (1732), were viewed from an entirely new standpoint: the thermometer scale was regarded as already given, the final reading of the thermometer being interpreted as indicative of the amounts of heat contributed to the mixture by the constituents. Fahrenheit found that two equal volumes of water at different temperatures attain, on rapid mixing, a temperature which is the arithmetical mean of the two initial temperatures,[11] but that when equal volumes of water and mercury at different temperatures are mixed, the temperature of the mixture is higher or lower than the arithmetical mean, depending on whether water is the warmer or colder constituent. Only when two volumes of water were mixed with three of mercury was the final temperature found to be the arithmetical mean of the initial temperatures.

The standpoint which Boerhaave assumes in attempting to explain Fahrenheit's experiments is typical of the fluid theory adherents. He regards, as a fundamental axiom, the proposition that there exists an imponderable, indestructible fluid which, in the mixture experiments, merely suffers redistribution. The fundamental question then becomes: How was this fluid distributed between the different bodies before mixing? From the viewpoint of historical criticism, Boerhaave's fundamental question is seen to amount to just this: How may the imponderable fluid be defined so as

* Continued from page 172 of preceding issue.

11 Strictly speaking, if the final temperature is the arithmetical mean of the initial temperatures when one thermometer is employed, it will not be so when another thermometer, employing a different thermometric substance, is used instead, unless the expansion of the first thermometric substance is proportional to that of the second. The appreciation of this fact had to await the exhaustive investigation of thermometric scales by Dulong and Petit (1817).

to satisfy the axiomatic requirement of conservation?

Fahrenheit's experiments revealed *one* weight of water to have the same heating effect, per degree change of temperature, as *twenty* weights of mercury. Although his experiments had, at the same time, revealed that *two volumes* of water are equivalent to *three volumes* of mercury, Boerhaave, no doubt preferring to attribute to experimental error the deviation of three-halves from unity rather than that of twenty, sees fit to state that heat is probably distributed between bodies at the same temperature in accordance with their volumes. Thus we see that Boerhaave's heat fluid was endowed with precisely those properties which, more than two thousand years earlier, were attributed to the Eleatic Being, namely, corporeality (space-filling), homogeneity (uniform distribution), and indestructibility.[12]

Confusion of Temperature and Heat. In the thought of Boerhaave we can detect a confusion of the intensity and capacity factors of heat which was quite prevalent in his day. This had already been evident in the case of Newton (1701), who thought that the "heat" of a hot body must be proportional to its "heat loss" and, at the same time, considered the readings of his actually arbitrary thermometric scale to be a measure of the latter and therefore, he thought, of the former as well. When Boerhaave decides, as a result of the mixture experiments, that heat is probably distributed according to volume, he thinks this view substantiated by the fact that bodies in contact come to a common temperature, thus revealing his confusion between uniform temperature and uniform distribution of heat.

The same confusion in terminology exists in the writings of Richmann (1750), who attempted to give mathematical formulation to the results of mixture experiments employing different quantities of the same substance.[13] He does not distinguish between temperature and quantity of heat but refers to both as "heat" (calor). On the basis of experiment and theoretical considerations, he stated that the "heat" u of a mass m on being distributed between the masses m and m' yields the "heat" $m\,u/(m+m')$, so that if two masses m and m' with "heats" u and u', respectively, are mixed, the resulting "heat" is $(m\,u+m'u')/(m+m')$. In general, when masses $m, m', m'', \ldots$ of "heats" $u, u', u'', \ldots$ are mixed, the final "heat" will be

$$U=\frac{m\,u+m'u'+m''u''+\ldots}{m+m'+m''+\ldots}$$

The close relation of Richmann's "$m\,u$" product to our "quantity of heat" is obvious. We must also accredit him with a certain insight into the relative nature of his measurements, for he states that it is not the "absolute heats" but only the excesses over the zero point of his thermometer that come into consideration.

From the appearance of masses in his formula, we might suppose that Richmann leaned to the opinion that heat is distributed according to mass. This does not seem to have been the case, although he disproved Boerhaave's assertion that the rate of cooling of a body is inversely proportional to its density by observing that a body of mercury cooled more rapidly, also was heated

12 To be sure, the analogy is not perfect, for Boerhaave did not deny the existence of a vacuum nor that of matter or fluids, other than caloric.

13 Kraft (1746) had already made an attempt in this direction when he advanced for U, the final temperature of a water mixture, the empirical formula

$$U=\frac{11\ m\ u+8\ m'u'}{11\ m+8\ m'}$$

where u and u' denote the initial temperatures of the bodies of water of masses m and m', respectively. The asymmetry of Kraft's formula must have been due to experimental error.

more rapidly, than *lighter* bodies of the same size, shape, and initial temperature. However, in his mixture experiments with water, he considered the experimental error due to the heating of flask and thermometer to be eliminated by regarding these as replaced by the same *volumes* of water.

Classic Work of Black. By definitely distinguishing between "heat" and "temperature" and by the introduction of the "heat capacity" and "latent heat" concepts, Joseph Black (1760)[14] probably did more than any other man to convert confusion into order in this most important domain of physical science. He begins, in a thoroughly empirical, yet critical, vein by pointing out that the well-known truth that "all bodies communicating freely with each other, and exposed to no inequality of external action, acquire the same temperature as indicated by a thermometer," is really a remarkable experimental fact which could not have been predicted from any known relation of each of the bodies, separately to heat. This fact, which had previously been referred to as the "equality of heat," Black wisely prefers to call the "equilibrium of heat," since it is not heat equality but temperature equality which is involved. Boerhaave's view, also adopted by Muschenbroek, that two bodies of equal volume and at the same temperature contain the same heats, he recognizes as an instance of confusion between the quantity and intensity factors of heat. Nor is the heat required to raise different bodies through the same number of degrees proportional to their density. Fahrenheit's experiments clearly show that "the same quantity of the matter of heat has more effect in heating quicksilver than in heating an equal measure (volume) of water. . . . Quicksilver, therefore, has less *capacity* for the matter of heat than water . . . has"; in fact, mercury has only two-thirds the capacity for heat that water has.[15] Heat, then, is distributed in a body, neither according to the body's mass nor its volume but "according to its (the body's) particular capacity, or its particular force of attraction for this matter."

Several investigators on the Continent were led to ideas similar to Black's. With Wilke (1781), the heat capacity notion finds expression in the statement that every body is equivalent to some quantity of water at the same temperature, i.e., the given body will have the same effect as its equivalent quantity of water in raising the temperature of a definite quantity of water through a definite temperature interval.[16]

Lambert (1775), like Black, distinguishes between quantity of heat (*Menge der Wärme*) and its intensity (*Kraft* or *Stärke der Wärme*). For bodies of the same substance at the same temperature, the first, he says, is proportional to the volume of the body, but the same quantity of heat has in bodies of different substances of the same volume a different force. Lambert regarded heat as consisting of "fire-particles" (*Feuerteilchen*) and described the Boerhaave-Fahrenheit experiment by stating that three "fire-particles" in water had the same "force of heat" as

[14] Black's posthumous *Lectures on the Elements of Chemistry*, which contained his ideas on heat, was not published till 1803. However, a surreptitious publication, containing an account of his views, appeared in London in 1770. Moreover, after 1760, he disseminated his views among his students orally by means of his lectures (11, ii, p. 161). In this way, many of Black's ideas became dissociated from the name of their originator (4, p. 180).

[15] Black observes that this fact also explains why, if equal volumes of water and mercury are placed at the same distance before a fire, the mercury warms faster, and when the fire is removed, cools more rapidly, than the water.

[16] To Wilke is due the introduction of the term "specific heat," which was suggested by the analogy with specific gravity (11, ii, p. 159).

two "fire-particles" in an equal volume of mercury. From his own observations of the temperature equalization between a liquid contained in a thermometer and a second liquid into which the thermometer was introduced, he concludes that four "fire-particles" have the same heating effect in mercury that six in the same volume of alcohol, and seven in the same volume of water, have.

Concept of Latent Heat. Quite as epoch-making as his ideas concerning heat capacity, was Black's extension of the heat concept to changes of state (liquefaction and vaporization) by means of the notion of "latent heat." When Black first took up the study of these phenomena (around 1760), only one kind of heat was recognized, namely, "sensible heat"; in other words, it was assumed that the addition or subtraction of heat to or from a body must, in every case, make itself evident by a change in the reading of a thermometer in contact with the body. Black demonstrated, with considerable finality, that this view, in the light of observed facts, was quite unscientific and untenable. Thus, he notes that in the case of fusion, if the accepted view be correct, a body of ice at the freezing temperature would be converted, wholly and instantaneously, into liquid by the smallest addition of heat. But the well-known slowness of the melting process shows us that this never happens. To confirm his opinion, Black suspended a body of ice and a body of cold water in a warm room and noted the changes in temperature of each. The temperature of the water rose continuously, but that of the ice rose to the freezing point, remained there until fusion was complete, then again rose continuously. The experiment, says Black, reveals clearly that during fusion heat is absorbed without change of temperature, since the body of ice and the body of water were both receiving heat under exactly the same exterior conditions. This was substantiated when he detected a stream of cold air descending from the ice during fusion, this being correctly described as air which had given up its heat to the ice. On solidification of the melted ice, Black reasoned that the same heat must be liberated which had been absorbed on fusion (Principle of Conservation of Heat). The liberation of heat on freezing was ably proven by reference to the supercooling and subsequent solidification of water accompanied by the rise in temperature of the mixture to the freezing point, a phenomenon known to Fahrenheit. The sudden rise of temperature shows, says Black, that it is not the loss of "sensible," but that of "latent," heat which is the condition of freezing.

From these experiments Black feels himself forced to conclude that the addition of "latent heat" at constant temperature is the "principal and most immediate cause of fluidity," whereas the subtraction of the same is the cause of solidity.[17]

Black gives added weight to his views by determining the heat of fusion of ice by two different methods. The first was based on the observation already referred to: a body of ice and a body of water, each at the freezing temperature and of known weight, were placed in a warm room, and from the rise in temperature of the water, the amount of heat absorbed by the ice during fusion was readily calculated. The second method was modeled after the mixture experiments of Boerhaave and Fahrenheit: a small piece of ice at the freezing point was intro-

[17] Some investigators of this time, e.g., Muschenbroek, adopting an ancient view of Democritus, held that water was a fluid not by virtue of heat imparted to it but because of an "essential quality," depending upon the supposed spherical shape of its particles. Freezing, they supposed due to the addition of "frigorific particles," a view supported, in the case of water, by the increase in volume on freezing (4, p. 33).

duced into a large body of warm water and the fall in temperature of the water during fusion noted. This with the weight of the water gave the heat absorbed by the ice which, divided by the initial weight of the ice gave the heat of fusion per unit weight. When we consider that this determination was being made for the first time in the history of physics, Black's value of 77 calories per gram for the latent heat of fusion of ice appears remarkably close to the modern value.

Black advanced exactly similar views to account for the facts of vaporization and liquefaction. Entirely unacquainted with any notion of "latent," as opposed to "sensible," heat many of Black's predecessors had held that the bubbles of gas which are generated in, and subsequently expelled from, a boiling liquid are bubbles of heat itself, and that the temperature of the liquid does not rise simply because this heat passes through the liquid instead of being absorbed. At the same time, others maintained that the bubbles were those of air. Black's careful reasoning, coupled with shrewd observation and cleverly designed experiments brought order into this wilderness of confused ideas. Having identified the bubbles as those of the vapor of the liquid, he shows that the constant temperature during vaporization is consistent with only one assumption, namely, that the absorption of "latent heat" is the cause of the formation of vapor, just as it had been the cause of fusion or liquidity. Indeed, says Black, if this were not the case, the liquid on reaching the boiling point, would, on the addition of the smallest quantity of heat, explode into vapor. Black's analysis of vaporization also explained why hot water, on being brought to boiling by subjecting it to reduced pressure, experienced a sudden and decided cooling, a phenomenon first noted by Boyle.

Black determined the heat of vaporization of water and then checked this against a determination of the heat of liquefaction, which, he correctly reasoned, should be equal to the former. The first was determined by steadily heating an initially cold body of water until vaporization was complete. By assuming that the rate of heat absorption during boiling was the same as that before the boiling point was attained, he was able to calculate, from the time elapsing during boiling, the latent heat of vaporization. The heat of liquefaction was measured by distilling a definite quantity of water into a known quantity of cooling water whose rise in temperature was noted. In another experiment the water was distilled into an ice calorimeter and the weight of melted ice noted. Black's first values for the heat of vaporization of water were too low (445, 456 calories per gram). James Watt, his famous student and assistant, introducing several refinements, concluded that the correct value lay between 495 and 525 calories per gram, values which compare favorably with the accurate value, 536, obtained over half a century later by Regnault.

Speculations on the Absolute Zero. Black insisted that hot and cold were purely relative terms and stated that there was no evidence for any lower limit of the temperature scale. Irvine and Crawford (1778), on the other hand, regarded the fusion experiments as supplying data for the determination of the absolute zero, i.e., the temperature corresponding to the state of zero heat content. Instead of regarding the latent heat of fusion as simply the direct cause of fluidity, as Black had done, they reasoned that, since the heat capacity of water is greater than that of ice, the latent heat must represent the net excess of total heat of the water at the freezing temperature over that contained in the ice at the same temperature. This as-

sumption, coupled with the supposed invariability of heat capacity with temperature, provided all that was necessary for the calculation of the absolute zero of temperature. Thus if c and c' be the heat capacities (specific) of water and ice, respectively, and the absolute zero be t degrees below the freezing point, the view of Irvine and Crawford requires that Q, the heat of fusion, be given by

$$Q = tc - tc'$$

Substituting $Q = 80$, $2c' = c$, we obtain $t = 160$, i.e., the absolute zero is at $-160°$ C. Further, this view requires that the heat of fusion decreases with decreasing temperature. Thus, for ice, if T be the Centigrade temperature, we have $Q = \frac{1}{2}ct = \frac{1}{2}(160 + T)$.

Dalton, who also adopted this view, showed that the same reasoning, applied to the heat of fusion of mercury, gave the absolute zero as $-2021°$ C. Gadolin, adopting similar assumptions to account for the heat developed on mixing two liquids at the same temperature, found that the heat of solution of sulfuric acid in water indicated the absolute zero to be between $-830°$ C. and $-1720°$ C. Still other "absolute zeros" were calculated from other mixture experiments and from the heat of chemical reactions.[18]

Specific Heat Determinations. Through introduction of the concepts heat capacity and latent heat, Black provided a secure theoretical basis for the measurement of quantities of heat, and hence specific heats, by two distinct methods, the method of mixtures and the method of melting ice. Black himself determined a number of specific heats, but most of the determinations date from those of Irvine (1763), Black's student, who measured the specific heats of mercury, glass, iron filings, and numerous other substances by the method of mixtures.[19]

Wilke (1772) employed the ice calorimeter for the measurement of specific heats as also did Laplace and Lavoisier (1780). To the latter is due the recognition of the general variation of specific heat with temperature and the realization that the specific heat can be precisely defined only by means of the calculus. They thus wrote: $s = dQ/dt$. They are with Black in refusing to assume any absolute zero of temperature, nor do they accept the view that the heat of a chemical reaction is to be attributed generally to the difference in heat capacities of reactants and products.

The first accurate measurements of specific heats were those of Dulong and Petit (1813), who employed the water calorimeter. They confirmed the variability of specific heat with temperature, already observed by Laplace and Lavoisier. They also discovered (1819) the law, for which they are best known, that

[18] As Mach (10, p. 168) notes, even though the assumption that the heat of fusion represents the net excess of the heat of the liquid over that of the solid at the freezing temperature, along with that of the invariability of heat capacity with temperature, be accepted, still the data need not necessarily be regarded as indicative of an absolute zero: if the temperature calculated be assumed to be that at which the heat of fusion in question assumes a zero value, then one may assume, equally well, that below this temperature the heat of fusion acquires *negative* values. Indeed, such a point of view would have eradicated the apparent discrepancies between the different "absolute zeros," since there would be no reason for supposing that all substances exhibited a zero heat of fusion at one and the same temperature.

[19] Let m and m' be, respectively, the mass of the body introduced and the water value of the calorimeter in grams of water. Then if the initial temperatures of body and *water calorimeter* are t and t', respectively, whereas the final common temperature is t'', we have

$$m(t - t'')s = m'(t'' - t')$$

from which s, the specific heat of the substance composing the body in question, is determined.

Again, if a body of mass m, specific heat s, and temperature t causes by its introduction into the *ice calorimeter* the melting of μ grams of ice, we have

$$mst = 80\mu$$

from which s may be calculated.

the atomic heat capacities of most solid elements have approximately the same value, namely, about six calories. A similar generalization for the molecular heats of certain classes of solid compounds was later discovered by Neumann (1831).

The determination of the specific heats of gases have a special significance for the history of thermodynamics because it was to bodies in the gaseous state that the first and second laws were first developed analytically. The first attempts in this direction were those of Crawford (1778) who introduced heated metal cylinders, containing weighed quantities of the gases, directly into a calorimeter. The results were unsatisfactory because of the relatively small weights of gas employed and the consequently small heating effect of the *gas*.[20] Laplace and Lavoisier (1784) remedied this by sweeping a large quantity m of gas through the coiled tube of an ice calorimeter, noting the fall of the temperature of the gas θ and the weight μ of ice melted. Then $ms\theta = 80\,\mu$, where s is the specific heat of the gas.

Clement and Desormes (1819) filled the same flasks with different gases, in turn, at the same temperature and pressure, and introduced it into the water calorimeter, whereupon the heat capacities of these masses of gases were placed proportional to the times required to warm the gases through the same temperature interval.

The first accurate measurement of specific heats of gases were those of Dalaroche and Berard (1813). The gas at temperature u_1 and constant pressure was led at the rate of m grams per minute through the coiled tube of a water calorimeter where it was cooled to u_2. If a steady state has been reached, indicated by a constant temperature of the calorimeter, the latter must be losing heat to the surroundings at the same rate at which it is absorbing heat from the gas. Hence, if the water value of the calorimeter (at the constant final temperature) be w and if the calorimeter is known to cool at the rate of v°C. per minute, when no gas is supplied, then

$$ms(u_1 - u_2) = wv,$$

where s is the specific heat of the gas.

Delaroche and Berard's experiments indicated that different gases have different heat capacities, but Haycraft (1824), refining their methods, thought he could conclude that equal *volumes* of different gases at the same pressure have equal heat capacities, a view supported later by Delarive and Marcet (1827). However, the most careful investigations of Regnault (1837), by the method of Delaroche and Berard, showed this to be true only for oxygen, hydrogen, and nitrogen, whose specific heats he also found to be independent of temperature. He was not able to detect any variation in the specific heats (unit weight) of gases with variation in pressure.

We must note that some of the above methods yield the specific heat at constant volume, others the specific heat at constant pressure. The early workers in this field did not clearly appreciate the significance of this distinction, and the confusion arising from attempts to explain the discrepancy involved, along with that of the cooling of a gas incurred on expansion as well as its heating on compression, constituted a principal factor in the fall of the caloric theory and the enunciation of the First Law of Thermodynamics.

Heat of Chemical Reaction. However, in the meantime, the caloric hypothesis was also demonstrating its great service in other branches of the phenomena of heat. Lavoisier, in researches carried out with Laplace on specific heats and

[20] Thus Crawford found, for the specific heat of air, 2 calories per gram(!). Lavoisier and Laplace, with their much improved method, got 0.33, the correct value being 0.2374.

latent heats, had further demonstrated the great scientific value of these concepts, and he attempted to extend the idea of latent heat to account for the heat of chemical reactions. In this line of thought, caloric (*calorique*), though admittedly imponderable, nevertheless plays somewhat the role of a chemical element.[21] Since to convert solids to liquids, and the latter to gases, a great amount of caloric must be supplied, Lavoisier supposed that gases, air in particular, contain the greatest amount of caloric. Accordingly, when a solid burns in air to form a solid oxide, Lavoisier supposes the caloric liberated to be that which was latent in the air, and states that the heat of combustion must be greatest when two gases combine to form a solid. He thus associates the liberation of caloric in a chemical reaction with the change of state of aggregation occurring (12, p. 27). However, in the case where both reactants and products are gaseous, he is forced to explain the heat effect as due to the difference in specific heats of reactants and products.

The conviction that caloric is a conservative quantity led Laplace and Lavoisier to assert that the heat liberated when a system changes its state is equal to that consumed when the system goes through the reverse change, a generalization of the principle employed by Black in dealing with fusion and vaporization. This generalization, which Laplace and Lavoisier were able to verify experimentally to a certain extent, later attained great significance in the work of Hess (1840) where it assumed the form that the heat liberated in a chemical change is independent of the path of the change.[22]

The caloric theory also served to clarify the phenomenon of cooling. As early as 1740, Martine had discovered deviations from Newton's Law of Cooling and these were verified by Kraft, Richmann, Leslie, and Dalton; in fact, the latter had proposed a new temperature scale for which Newton's law would be exact. The exact investigation of the rate of cooling of a body was first carried out by Dulong and Petit (1817). Whereas Newton's treatment had been obscured by the confusion of temperature and heat, Dulong and Petit, profiting by the advances instituted by Black, clearly distinguish between these two concepts. Adopting the fundamental notion of heat as a conservative quantity, they regard the net heat lost per unit time by a cooling body as equal to the excess of the heat radiated over that absorbed per unit time. The rate of heat radiation and absorption was, in turn, recognized as being dependent on the temperatures of body and surroundings, on the form and extent of surface of the cooling body, on the total mass of the body, on the peculiar nature of the surface of the body (coefficient of emissivity), and on the nature of the surrounding medium (4, p. 504).

Caloric and Heat Conduction. Another great support of the fluid theory

[21] Light (*lumière*) and caloric (*calorique*) appeared at the very top of Lavoisier's table of elements (*tableau des substances simples*), followed by the chemical elements known in his day. As older terms for caloric, he lists heat, principle of heat, igneous fluid, fire, matter of fire, matter of heat, but not phlogiston. Yet, in expounding his theory of combustion (1777), he states that caloric, as well as light, are evolved in every combustion, quite in the manner of the phlogistinians (17, p. 55). The essential point here is, of course, that Lavoisier recognizes that the liberation of the *imponderable* caloric is only one aspect of combustion and is accompanied by combinations with the *ponderable* oxygen of the air.

[22] The influence of Lavoisier's relegation of caloric to the same class as the chemical elements is seen in Hess's attempt to extend the law of multiple proportions to caloric, i.e., he tried to show that "when two substances combine in several proportions, the quantities of heat which are produced in the formation of the different compounds stand to one another in multiple proportions" (13, p. 504).

was found in the firm foundation it provided for the theory of heat conduction which attained its highest stage of development in the hands of Fourier (1822). The earliest problems in heat conductivity seem to have been associated with the variation in temperature at the different points of an iron bar subjected to a constant source of heat at one end. The first attempt at a quantitative treatment was that of Amontons (1703), who thought that the temperature increased linearly with the distance from the cold end. To Lambert (1778) is due the realization that the "steady state" attained by such a bar is really a dynamic one in which the heat gained by any element from the hot end equals the sum of the heat given up by the element to the surrounding air and that passed on to the colder part of the bar.

The confusion of radiated heat and conducted heat was naturally associated with the confusion of emissivity and conductivity (often referred to as "outer," or "surface conductivity," and "inner conductivity," respectively). Thus Franklin supposed that, if bars of different metals, but having the same dimensions, be coated with wax and all exposed, at one end, to the same source of heat, then the distances over which the wax was melted, in the steady state, would be proportional to their conductivities for heat. Ingenhouss (1785) adopted this point of view and carried out the experiments. But J. T. Mayer (1791), reasoning that the best conductors of heat should be those which lose their heat to the air most rapidly, concluded that Ingenhouss' best conductors were actually the worst.

The first sound theoretical treatment of the stationary state of a unilaterally heated bar is due to Biot (1804). He reasoned that, at any point, the heat gained from the hot end equals that lost to the air throughout the remainder of the bar, which, in turn, may be computed from Newton's law of cooling. In this way, he deduced the law that, for distances from the cold end which form an arithmetical progression, the corresponding temperature excesses over the surroundings form a geometric progression, also verifying it experimentally over a considerable temperature range.

All of the laws of heat conduction were developed in a marvellously systematic and comprehensive way by Fourier, working from 1807 to 1822, when his great *Théorie analytique de la Chaleur* was published. He distinguished clearly between heat capacity, emissivity, and conductivity, and formulated the first precise analytical definition of the latter concept. His whole theory of conduction followed rigorously from a simple fact, taken as first principle: the quantities of heat exchanged between two parts of a conducting body, lying very close together, are proportional to their temperature difference. The solution of the problem of the distribution of heat in a long bar in the steady state appeared as a mere detail in Fourier's comprehensive elaboration: he showed that, assuming invariability of conductivity with temperature, the distances over which the wax was melted in Ingenhouss' experiment were proportional not to the conductivities themselves but to their squares.

Although Fourier does not commit himself on the nature of heat, the latter concept appears in his equations precisely as if it were an imponderable fluid which suffers change of distribution but never gain or loss in total quantity, and which strives to attain a uniform "temperature level" just as a ponderable fluid seeks a condition of uniform height above sea level. The fluid theory of electricity had already been advanced in the eighteenth century by Franklin, Aepinus, Coulomb, and others; and Ohm (1826), impressed by Fourier's work, conceived of the analogy between flow

of electricity and flow of heat. He argued that an analogue of the temperature difference of Fourier's theory should exist for electric currents and was thus led to formulate that most useful concept, electromotive force ("electroscopic force").

Caloric as Substance. With so many victories to its credit, it is not surprising that the caloric theory came to occupy a position of commanding influence and relative security, a position which it did not completely relinquish until the middle of the nineteenth century. In particular, the growing number of facts consistent with the caloric hypotheses had the effect of bringing increasingly into prominence the conviction that caloric was an actual substance.

This is already evident in the terminology of Boerhaave who, by referring to heat by the Latin term *ignis,* reveals the close relationship between the eighteenth century heat fluid and the Greek "fire-matter."

It is significant that Black, who carefully warned scientists against attempts at ultimate explanations in terms of hypothetical abstractions (15, pp. 282–285), nevertheless revealed, in his terminology, a firm belief in the substantial nature of caloric. For he refers to heat as "matter of heat" and characterizes the "equilibrium of heat" by the very materialistic phrase "equality of saturation." Moreover, adopting Cleghorn's assumption that caloric exhibits an attractive force for different substances, he identifies the heat capacity of a body as "its particular force of attraction for this matter."

Berthollet, to whom Ellis (2, p. 186) attributes the introduction of the term caloric, cautiously defined it simply as the "cause of heat." Lavoisier was, at times, equally careful, for he says (2, p. 186), "rigorously speaking, we are not even obliged to suppose that caloric is a real substance; it suffices that it may be any cause whatever which separates the molecules of matter, and we can thus consider its effects in an abstract and mathematical way." Yet the phraseology of this very excerpt ("we are not *even obliged*" to consider it a substance) indicates that Lavoisier considered it both natural and legitimate, even though unnecessary, to regard caloric as a "real substance." That this was indeed his view is corroborated by his explanation of the explosive force of gunpowder: this, he said, is due to the sudden liberation of caloric, "that highly elastic fluid," also by his reference to fusion as a process of "solution in caloric." But the most convincing evidence of his belief in the substantial nature of heat is found in the circumstance that even after he had, by his investigation of combustion, banished phlogiston from science, he nevertheless continued to retain "caloric" in his table of chemical elements.

Cleghorn and Black had endowed caloric with the fundamental Newtonian property of matter, that of attraction (for other kinds of matter), and Lavoisier had even placed caloric in his table of elements. From these circumstances, it is not surprising that some eighteenth century calorists thought that caloric possessed at least *some* mass (4, p. 32). Otherwise Rumford, a great opponent of the caloric theory, would hardly have considered it worth-while to carry out his investigation, *An Inquiry Concerning the Weight of Heat* (1799). He was able to show conclusively that caloric, if it exists at all, does not possess mass.

After Rumford's investigations, all calorists recognized that caloric, like electricity and magnetism, must be an imponderable. However, the property of weight had never been regarded as fundamental, except insofar as it was implied by the postulated attraction of caloric for matter. A more fundamen-

tal property, which had been assumed from the beginning, was that of the elasticity and self-repellent nature ascribed to the heat fluid.[23] Thus Dalton (16, ii, p. 393) says that besides the force of attraction between particles, "we find another force that is likewise universal, . . . a force of repulsion. This is now generally, and I think properly, ascribed to the agency of heat. An atmosphere of this subtle fluid constantly surrounds the atoms of all bodies and prevents them from being drawn into actual contact." Also Fourier speaks of "the equilibrium which exists, in the interior of a solid mass, between the repulsive force of heat and molecular attraction." He also says that "heat is the origin of all elasticity."

This self-repellent property of caloric explained why the volumes of bodies quite generally increased on heating (at constant pressure). The anomalous cases of the fusion of ice and the contraction of water between the freezing point and 4°C. were "explained" by reference to analogous cases in mixtures of ordinary matter; e.g., the solution of water and alcohol, also that of copper in tin, and numerous chemical reactions are accompanied by a decrease in volume.

Indeed, the magnificent edifice comprising both theory and experiment, erected on the basis of the caloric hypothesis, must cause us to place it among the most fruitful hypotheses in the history of science. However, in the meantime, the facts of frictional and electrical generation of heat and the heating and cooling of gases on compression and expansion, respectively, were coming into increased prominence and were destined, after a battle of half a century, to bring about the fall of the fluid theory. This fall was coincident with the establishment of the First Law of Thermodynamics and, in many cases, with the revival of the kinetic theory of heat.

[23] As early as 1733, the same property had been attributed to each of the two electric fluids by Dufay (11, ii, p. 201).

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BASIC ENGLISH FOR SCIENCE

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THE following letter (evidently not written to be posted) I found in the English notebook of one of my students in Freshman Composition:

TO THE AUTHORS OF *A Textbook of Botany:*

As a student in the botany course which is given at our university, I have become familiar with your textbook and workbook which are used in connection with the course. I wish to inform you that I am having a great deal of difficulty with botany, and I believe your books are largely responsible.

The purpose of botany, I believe, is to acquaint the student with the different types of plant life and to help him understand the growth and structure of plants. In my estimation *A Textbook of Botany* defeats that purpose. The average student is lost in the maze of difficult and highly technical language of your text and in the complexity of the demonstrations and problems in your workbook. For example, in describing the beginning of a leaf you state that "development of a leaf begins with the proliferation of a primordium"—without any previous hint of what a primordium is!

As a result of your heavy treatment of the subject, botany is dreaded and disliked by the majority of students on this campus. Many have failed the course because of this dislike—for which your boring textbook is largely responsible. You have, in the eyes of many students, attached a stigma to the useful science of botany.

The evident sincerity of this letter, I hope it will be agreed, entitles it to a fair hearing. What college freshman has not at some time or another felt a similar protest rising within him as he tried to advance through the maze of language between him and the subject he was studying? The writer of the above letter may pass her course in botany and give the lie to her fears. She may even go on to like botany. But what a pity that she must arrive in spite of the language in which her textbooks are written. Much has been said on the teachers' side of the difficulty instructors of physics, of botany, and of chemistry have in getting their students interested in these branches of science. Perhaps the main reason lies not in reason mainly is not the students' dislike for the subject itself but for the language in which the subject is presented to them.

Too many college texts in science are burdened with an unnecessarily heavy style. The use of essential scientific words makes for economy; certainly the author is not expected to eschew them to the point of repeating long definitions. But why cannot he occasionally use "growth" instead of his beloved "proliferation"? "Chain of events" instead of "series of concatenations"? "Scaling off" instead of "desquamation"? It would seem that some authors of secondary science texts think that unless they write in the style of Herbert Spencer's definition of evolution, they cannot impress their readers with the importance of their subjects; as if what is stated simply cannot be worth learning. Clear exposition is a craft which scientific writers ought to regard as highly as the validity of their ideas. Generally speaking, they seem not to be aware of its existence; or, if they are, acknowledge it by keeping as far as possible from it—after the example of Professor Longbore, who used to open his science lectures each quarter with this warning, the only intelligible sentence in his discourses: "I do not intend to make clear to you in twelve weeks what it took me fifty years to learn."

The style of Professor Longbore and his ilk is probably the result of a passive rather than an active state of mind. As

one turns the pages of a ponderously written text in college zoology, for example, he begins to wonder whether the author may not have drifted into his style merely by following the course of least resistance. A polysyllabic style is a lazy style. It is easy to master the learned jargon of any science, and mastery of the jargon is too often mistaken by publishers' readers for mastery of the subject. "Easy writing makes cursed hard reading," observed Dick Sheridan; and although laborious writing is not guaranteed *per se* to make easy reading, it has a good chance to, if the writer knows what he wants to say and tries hard enough to say it. My point is that it is downright hard work to express scientific concepts in a clear, mature style. And yet texts written for college students ought to be worth that much effort.

For some writers, no doubt, there is a fascination in the weighty language of which my student complained. Thus the trap is baited and set for the author's complete undoing: he lets words take the place of thought. He has seen these splendid terms so often; they were right to him in the books he read. Are they not as good in his own? He does not stop to ask what the words really mean, how he expects his reader to interpret them. If by any chance a conscientious student narrows his eyes and carefully examines this lingo, the result is usually a feeling of dismay like that expressed in the letter at the beginning of this article.

Is there, for example, any reason why a book in psychology should be written in this style?

The apperception of self-motivation is a psychological fact. A concomitant phenomenon is the consciousness that the origin of this motivation is internal and not external.

Is not this what the writer *means?*

The mind is conscious that it is self-moving; and at the same time, that the motion comes from within itself.

The last sentence above is written in Basic English. This simplified English ought to have an especial appeal to scientific writers because its discovery was analogous to the procedure of the scientist seeking basic principles in the natural world. The originators of Basic English, sifting the thousands of words in our language, isolated 850 indispensable terms by which the meanings of the others could be expressed. For science an additional list of 100 words is provided.

The methods by which the Basic word list was determined can be tested by anyone who takes a dictionary in his hand. He will find in reading definitions that certain words keep returning time after time—usually little words such as *go, get, make, be, thing, name, true, good,* together with necessary conjunctions and prepositions. These words and others of their kind *are* the basic vocabulary of our language. They make a restricted common ground on which it is possible for writer and reader to meet with the least possible chance for confusion or mistake. In its inductive origin, as well as in its purposes, Basic English is scientific English.

It is not urged here that all writers of college texts in science adopt at once the Basic English vocabulary. The Spartan simplicity of Basic, though it is the handmaiden of truth, does not always serve other ideals as faithfully. Variety and subtlety, for example, are not main properties of Basic. These virtues and other qualities of a pleasing style ought not to be lacking from the books our science students read. Nevertheless Basic English could have a tonic effect upon these books. It could dispel much foggy thinking, which is the real cause of bad writing. If an author *thought* in Basic first, he would not write "heliotropic inclination toward the illuminating source." He would see that the meaning of his first word is repeated needlessly in the

five that follow and might decide that his whole phrase could be put thus: "turning in the direction of the light"—which is good science and good Basic. No one can compose in Basic without having in his mind a pretty clear idea of what he wants to say. There are no superfluous terms in Basic to get between him and his manuscript. He will often be reminded that between his idea *A* and the words *B* that represent it there ought to be the same relation as between an object *a* held before a mirror and its reflection *b*. A true reflection requires a good mirror. Basic English has the makings of a good mirror because its vocabularly is level and impersonal—a plane reflector. Even though the scientific writer makes use of a larger vocabulary, if he keeps firmly in mind Basic equivalents as he composes his sentences, his writing will gain clearness, whatever words he finally chooses. And his readers—his students or his peers—will call him blessed.

But there is another field of scientific writing where the need for Basic English is far more pressing. I mean the scientific books and magazines printed in this country and Great Britain. A great many foreigners before World War II were coming into English via Basic. Now as an international language Basic is gaining steadily in general esteem everywhere. Public interest in it was greatly stimulated by Winston Churchill's ardent approval of Basic at Harvard in 1943. No artificial language can meet the stern needs of an international tongue as Basic English can. First, it has behind it the compelling prestige of the Anglo-Saxon tradition; it "looks" like English and it *is* English, the vital heart and core of the language of Shakespeare and Jefferson. Basic is easy for the non-English speaker to learn. A few weeks' steady effort under skilled direction can make an intelligent foreigner at home in written and spoken Basic. The demand for books in Basic, both here and abroad, is on the upswing. It is one sign of the world-hunger for unity and commonalty among the peoples of our shrinking planet.

In satisfying this hunger the place of science is nothing less than strategic. It remains for science to recognize some of the practical aspects of its position. Science, as an international agency, must create or adopt an international tongue. The scientist today is faced with the problems faced by English traders 500 years ago as they carried their goods and their language into the Seven Seas. Through necessity, between them and their brown-, black-, and yellow-skinned customers, a species of international language slowly developed. The barbarous pidgin ("merchant") English of the Far East is a natural phenomenon brought into being by the needs of men groping toward each other's minds. These needs are a hundred times more imperative today. The very existence of the race may depend upon our finding right answers to them. Science, like trade, now has the earth as its province. More fortunate than trade, science does not have to await the development of a crude, mass-made English. A scientifically evolved speech is at hand; in the words of Mr. Churchill, "a very carefully wrought plan for an international language, capable of very wide transactions."

It is a truism to say that the great impetus felt by scientific research during the past five years will continue and accelerate. Parallel with this step-up of activity in the ranks of the scientists is a keen public concern about what they are doing. Jet-propelled aircraft and atomic bombs have drawn the fearful attention of everyone to the laboratory of the technician. This public interest cannot be written off as mere curiosity. We are hearing it said on all sides: Why, if the scientist is so expert in devising the

machines of death and destruction, why cannot he turn his talents as effectively to the service of humanity? This protest is admittedly naïve: Burbank and Edison were scientists. But the protest still stands. Its ultimate meaning is that everyone the world over wants to know what the scientist is about.

Modern science has therefore a vast new social responsibility which it cannot ignore. The day of unadulterated "pure" research is about over. Even though the scientist may not, like Terence, agree that "Everyman's business is my business," Everyman is telling the world and himself that "the scientist's business is my business." And Everyman pays the taxes and makes the grants that keep the scientist going. Everyman is a Chinese farmer, a Chicago businessman, a French taxi driver, a Greek fisherman, a Russian fur dealer. All these are invading the hitherto sacred confines of the technician's laboratory. And they have a right to do so.

In practical terms this means that the findings of the technician must be put on paper. Books must be written, articles contributed to scientific and lay journals. At present the chances are twenty to one that the native tongue of our hypothetical scientist will be English. Why should he not address himself to his world-wide audience in a truly international language—Basic English?

Basic is surprisingly easy for the English user to learn. With a little experience a copy writer can translate a full-English draft into Basic about as rapidly as he can compose. It is most desirable, of course, that the scientific writer prepare his own Basic version of his books and articles. Thus the thoughts of such authorities as Sir James Jeans, J. B. S. Haldane, Walter S. Landis, and Sir Arthur Stanley Eddington could go directly to the minds of men all over the earth without the warped meanings and false emphases that lurk in translations.

In facilitating this direct communication between the writing scientist and his universal reader, the American and British scientific journals have a place of unique importance. Their large circulation is a token of the immense service they can render to science and to humanity. By the use of complete articles in Basic English and by special Basic editions and supplements, they can directly interpret the findings of modern science to a circle of readers that in a very true sense is world-wide. In so doing they will be assuming their share in the large responsibilities borne by science in the world today.

I wish to conclude by submitting a brief specimen of Basic translation. The original, which follows, was chosen from Sir Charles Lyell's well-known *Progress of Geology:*

(1) For more than two centuries the shelly strata of the Subapennine hills afforded matter of speculation to the early geologists of Italy, and few of them had any suspicion that similar deposits were then forming in the neighboring sea. (2) Some imagined that the strata, so rich in organic remains, instead of being due to secondary agents, had been so created in the beginning of things by the fiat of the Almighty. (3) Others ascribed the imbedded fossil bodies to some plastic power which resided in the earth in the early ages of the world. (4) In what manner were these dogmas at length exploded? (5) The fossil relics were carefully compared with their living analogues, and all doubts as to their organic origin were eventually dispelled. (6) So, also, in regard to the nature of the containing beds of mud, sand, and limestone: those parts of the bottom of the sea were examined where shells are now becoming annually entombed in new deposits. (7) Donati explored the bed of the Adriatic, and found the closest resemblance between the strata there forming, and those which constituted hills above a thousand feet high in various parts of the Italian peninsula. (8) He ascertained by dredging that living testacea were there grouped together in precisely the same manner as were their fossil analogues in the inland strata; and while some of the recent shells of the Adriatic were becoming incrusted with calcareous rock, he observed that others had been newly buried in sand and clay, precisely as fossil shells occur in the Subapennine hills.

Basic English

(1) For more than 200 years the shell layers of the small mountains near the Apennines had been a question for discussion among the persons in Italy who first became interested in the science of the earth's history as recorded in beds of rock. (1a) Only a very small number had any idea that like deposits were then forming in the nearby sea. (2) Some had the idea that the rock layers, which had in them a great amount of the dead substance of things once living, had been made not by the decomposition of those things, but by an order of the Almighty when He made the earth. (3) Others gave the explanation that the plant and animal bodies in the stone beds were deposited there by some force of swelling and contraction which was in the earth in its early days. (4) In what way was the demonstration made at last that these ideas were false? (5) An exact comparison was made between the stone plants and animals and the living ones like them, and all doubts that the stone forms had come from living forms were put away at last. (6) The same fact was made clear about the substances in the sea-beds of earth, sand, and *lime* stone (stone having a great amount of chalk): tests were made of those parts of the sea-floor where shells are now year by year being covered over in new deposits. (7) Donati took samples from different parts of the bed of the Adriatic and made the discovery that the layers forming there were very much like those which made up small mountains over 1,000 feet high in different parts of Italy itself. (8) He made the discovery by taking up samples from the seafloor that living *testacea* (a species of small shell-covered animals) were there grouped together in exactly the same way as were their like stone forms in the inland layers; and at the same time some of the new shells in the Adriatic were being covered with *lime*-stone rock, he took note that others had been newly covered by sand and sticky earth, exactly as the stone-covered shells were, in the small mountains near the Apennines.

POSSIBILITY

If grass is green beyond the galaxies
Beneath blue skies of undiscovered spheres
Where other beings see their hopes and fears
In stars that haunt their own mythologies;
If other worlds are rimmed by sounding seas
Endlessly moving over measured years,
And eyes of men beyond their brim of tears
Are shining into distant mysteries;

If another earth is near a sun unknown
Among the hinterlands of stars unfound,
The colors of its captive dreams may be
Within the shattered spectrum of our own,
Its silent fantasies of inner sound
The voices of our own eternity.

JOEL W. HEDGPETH

SCIENCE ON THE MARCH

SAINT LOUIS TODAY

IT IS A pleasure for the cultural institutions of St. Louis to serve again as hosts to the members of the American Association for the Advancement of Science, March 27–30, 1946. In spite of many handicaps of the past four years a considerable degree of progress has been achieved by the industrial, social, and educational organizations of the city. Those scientists who attended the meetings that were held here in 1935 will find new points of interest and notable additions to the old ones.

Opposite the entrance to Union Station the Milles Fountain group, symbolizing the confluence of the great Mississippi and Missouri rivers a few miles north of the city, now offers a decidedly more pleasing greeting to visitors than the unsightly buildings that formerly existed on the site.

Immediately prior to the war many blocks of the older river-front buildings were demolished in preparation for a great memorial plaza. The war interrupted these plans, but enough has been accomplished to show that on their completion St. Louis will stand out as one of the most beautiful riverside cities in the world. And to those who have not visited this Midwestern city during the past ten years the most striking change will be its cleanliness. With the passing of an effective antismoke ordinance St. Louis now ranks with the tidiest of America's large industrial communities.

St. Louis and Washington universities will be points of most immediate interest aside from the scheduled meetings in the Kiel Auditorium and various hotels. At Washington University, Brown Hall, housing the School of Social Studies, was opened in 1937, and presents an attractive architectural as well as educational addition. In a special building near

St. Louis Chamber of Commerce

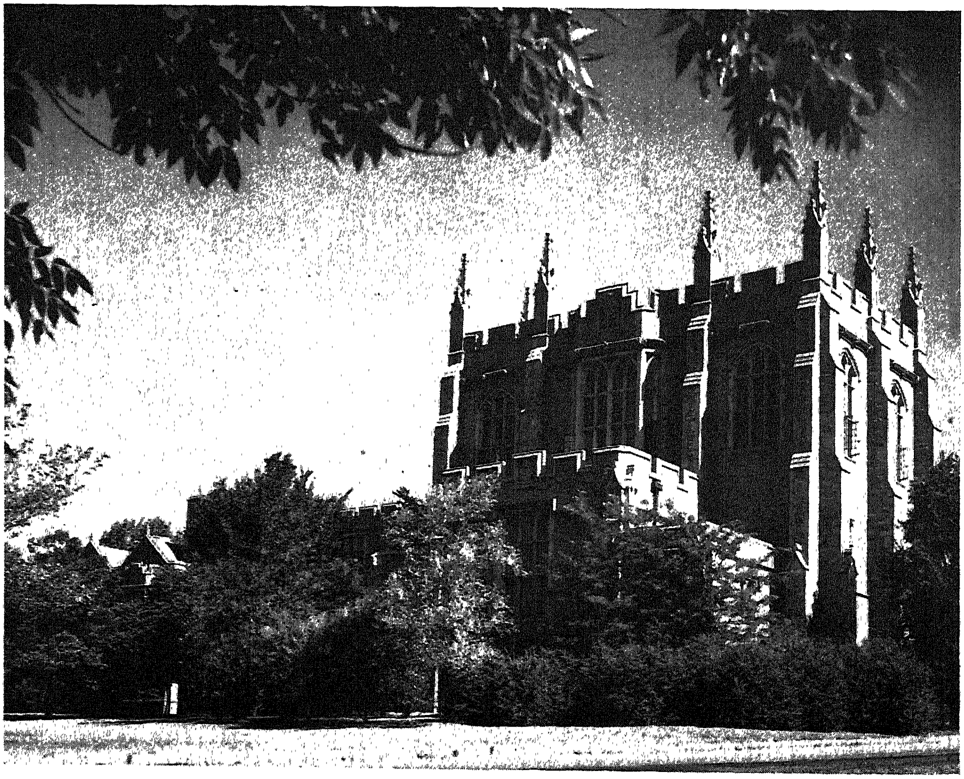
THE BARNES HOSPITAL GROUP NEAR FOREST PARK

Crow Hall the cyclotron that played an important part in the earlier phases of wartime atomic research should not be overlooked by those attending the physical science meetings.

St. Louis University, located in the heart of the city, is especially noted for its work in the fields of medicine and seismology. It was, in fact, the first institution of learning in the world to establish a department of geophysics and is still the only university in the United States to have a separately organized department of this kind. By means of an elaborate system of seismographs, strategically placed at five points in Missouri and Arkansas, the University "keeps its finger on the pulse of the earth." In the field of medicine the work of the recent Nobel Prize winner, Dr. Edward A. Doisy, is representative of the great strides that have been made of late years in the Medical School of this University.

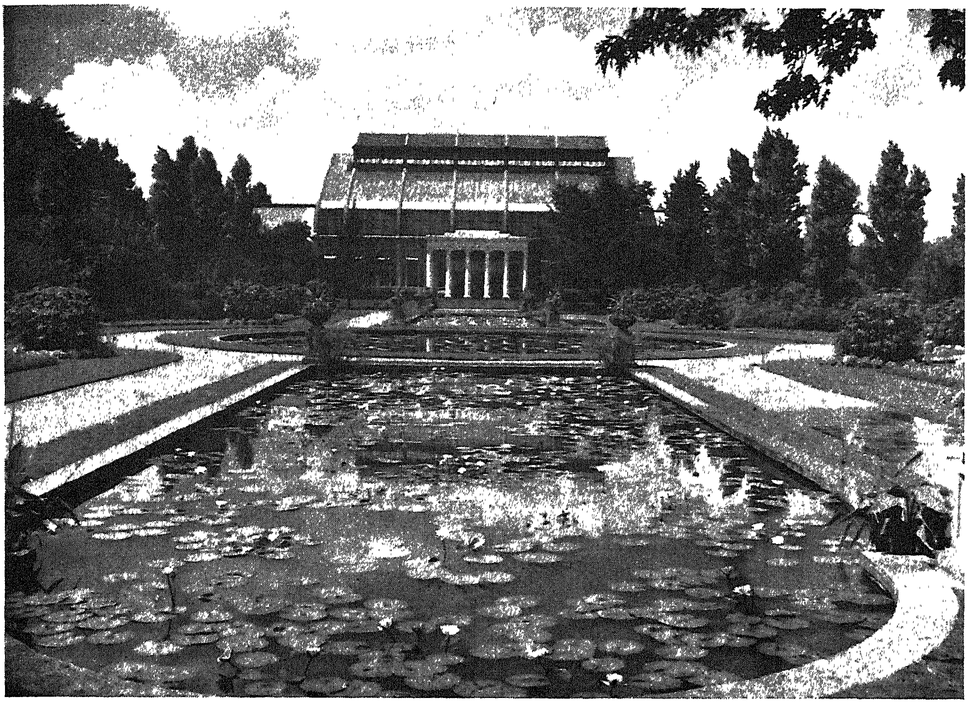
March is not an especially favorable time for outdoor plantings at the Missouri Botanical Garden (Shaw's Garden), but the greenhouses in the city garden show many improvements over ten years ago, and a special floral display will be presented at the time of the Association meetings. The library and herbarium will attract those whose activities center in the more technical phases of the botanical sciences.

The most evident progress at the Missouri Botanical Garden during the past decade is in the development of the Arboretum at Gray Summit, on the northern fringe of the Ozarks and overlooking the Meramec River. The entire orchid collection of the Garden, numbering some 20,000 plants, is now housed in green-



Day Photographers

BROWN HALL, WASHINGTON UNIVERSITY'S NEWEST BUILDING



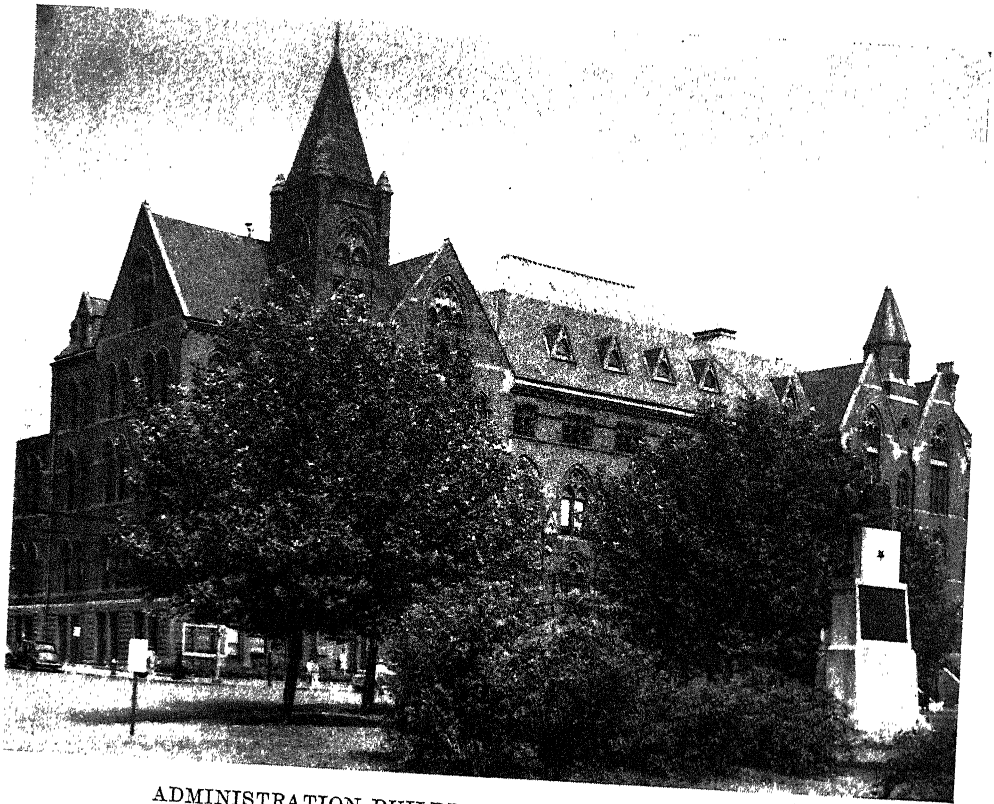
MAIN GREENHOUSE, MISSOURI BOTANICAL GARDEN

houses on the Arboretum grounds. During the past four years outstanding research has been carried on dealing with the light relations and the hybridizing of orchids, and with the technique of hydroponics in their culture. Variations of this method are now employed on a large scale.

For those with a few hours of time from busy meetings the municipal Forest Park of 1,400 acres offers a unique assemblage of attractions. The more serious-minded will appreciate the Art Museum where there are constantly changing exhibits covering many phases of art, as well as the large and splendid permanent collections. On the north side of the Park, directly opposite the Art Museum, is the Jefferson Memorial. This is occupied in part by the fine library of the Missouri Historical Society, containing a large collection of Jefferson manuscripts and others pertaining to the Hamilton-Burr controversy. And during the past decade the Lindbergh Collection has attracted many millions of visitors.

Only a short distance from the Art Museum are the Zoological Gardens, housing a magnificent collection of animals from all corners of the earth. In recent years the Zoo has improved in the acquisition of rare animals as well as in landscaping and in the enlargement of the houses. The reptile and bird houses are acclaimed as the finest in the world—zoological treasures to the professional student and layman alike.

The Park also includes a magnificent modern greenhouse, known as the Jewel Box, which presents exquisitely arranged floral displays according to the season. With the advent of spring, at about the time planned for the meetings, Forest Park should be given high priority on the visitor's list.



ADMINISTRATION BUILDING, ST. LOUIS UNIVERSITY *Famous Barr Co.*

For the historically-minded who have an hour or two to spare a number of places may be visited which hark back to the glamorous river days of the early nineteenth century, when St. Louis was the chief embarkation point for the western caravans that rumbled over the high plains to the forests of the Pacific Coast.

The Old Cathedral and Court House, both constructed in the 1830's, are only a few blocks from the downtown meeting points of the Association and are rich in the historic lore of the river-front days of a century ago. The Campbell House and Field Home are also representative historic landmarks. The former, built by Robert Campbell, a fur trader of the 1820's, contains the original furnishings and is typical of a wealthy home of the period. The Field Home, where the poet Eugene Field was born in 1850, is located near the downtown business district. It has been rehabilitated and refurnished with articles used by Field in his earlier days.

Space does not permit even a listing of the many industrial plants that might be of interest to the varied scientific callings represented among the Association's members. However, particularly noteworthy for biologists are the many breweries for which this city is justly renowned. Thousands of persons are conducted through these plants each year, thus being afforded an opportunity to see the most modern techniques and applications of intensive research in industrial microbiology.

HENRY N. ANDREWS
MISSOURI BOTANICAL GARDEN

LET'S HEAR ABOUT IT

THERE is an old saying among otologists (ear doctors) that one should not put anything in his right ear except his left elbow. The advice, if followed, would prevent many a ruptured eardrum.

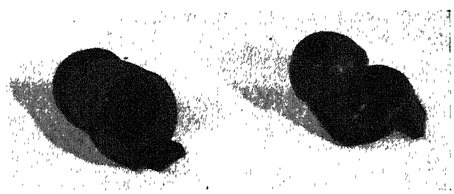
The Navy, of course, has more than an academic interest in the hearing of its personnel. Before a man is asked to sign on the dotted line, he is given a pseudoscientific test in the form of a whisper, tick of a watch, or the click of a coin. Before he is handed his honorable discharge, he is tested again with the same pseudoscientific test, and it is hoped that his hearing has not been impaired to the extent of furnishing, at a later date, a basis for claims against the Government.

It may be that the prefix "pseudo" will raise unpleasant connotations, but any test which ranges from the dulcet whisper of a corpsman from South Carolina to the gravelly rumble of a corpsman from Maine certainly is not scientific. Likewise, the click of a coin or the tick of a dollar watch can hardly be trusted. However, there is no known record of any man in the Navy having hearing so bad that he could not hear the call to chow.

After a man has entered the service, his hearing is subjected to many influences. In addition to the ordinary noises that he encountered in what is euphemistically called civilian life, he is exposed to the noise and blast effect of the guns and, in certain ships, to the high noise level of the Diesel engine rooms. The effect of the guns upon hearing is not open to argument; too many ruptured eardrums offer their eloquent testimony. The effect upon the hearing of personnel being subjected to the high noise level of Diesel engine rooms is questionable. The specialists cannot seem to come to an agreement as to whether there is such a thing as occupational deafness.

It would seem to the layman that continual exposure to high noise levels would eventually have a deleterious effect upon hearing. A number of medical men subscribe to this theory. Another group who venerate statistics maintain with more heat than light that, as we have no scientific test data on the hearing of these men before they were subjected to the high noise level, we have no means of knowing whether the loss of hearing was occupational or due to the ordinary vicissitudes of advancing age.

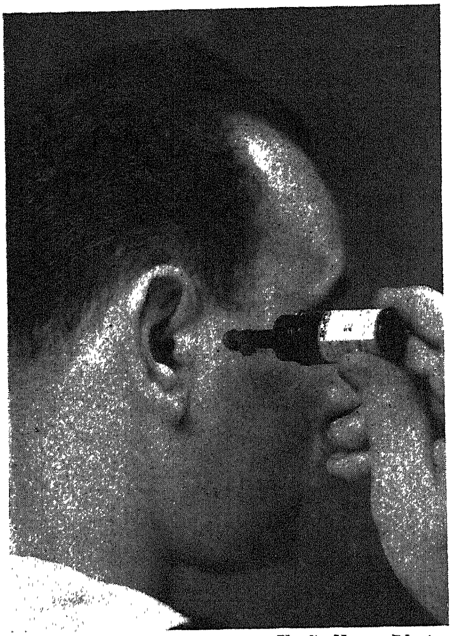
There is one point upon which all the great minds seem to agree; that is, noise *does* fatigue. Standing a watch over a couple of laboring Diesel engines takes more out of a man than standing watch over a pair of purring turbines. The continued rat-a-tat of a riveting machine in an aircraft factory raises the curve of absenteeism, although no notice has been taken of this saboteur.



U. S. Navy Photo

THE V–51R EAR WARDEN

The Navy's answer to this problem has been cotton—wads of it. It is said that the Union admirals were considerably disturbed during the Civil War over the necessity of using cotton before starting a shore bombardment problem. But that is the only period when there was any doubt about using cotton. For generations it has been the universal panacea against noise and blast. It must be admitted in deference to the Cotton Growers Association that cotton



U. S. Navy Photo

INSERTING WARDEN

properly inserted in the ear canal offers good protection against blast and noise.

As the tempo of the recent war increased, and the noise level on ships likewise, the Navy sought the best ear protection obtainable. Was it cotton or was it some mechanical device? Of the making of earplugs there is no end. Some are good, some are bad, and some are indifferent. Before a suitable ear protective device could be selected for universal naval issue, it had to meet certain basic requirements.

The first and most important requirement was that the device would have to offer greater protection than cotton. There would be no point in introducing a mechanical device that would not do the job better than cotton.

The next consideration was that of comfort, for any safety device must be reasonably comfortable or men will not use it. Safety engineers sometimes lose sight of this fact. Men can be ordered to use ear protection or goggles or anything else, but unless they are comfortable they will find an infinite number of ways for evading their use.

It also had to be borne in mind that practically everyone on board ship from the captain to the cook at some time or other must wear the "phones." This introduces two separate and distinct problems. The ear protectors must not interfere with the use of the phones and, while excluding noise, they must not reduce the possibility of hearing commands and orders—requirements that appear to be contradictory and impossible to meet.

Finally, as the bulk of the fleet operations were being conducted in the tropics, there was the ever-present danger of fungus growth, which has been described as "athlete's foot" of the ear. Would the introduction of a mechanical device into the ear increase the possibility of infection?

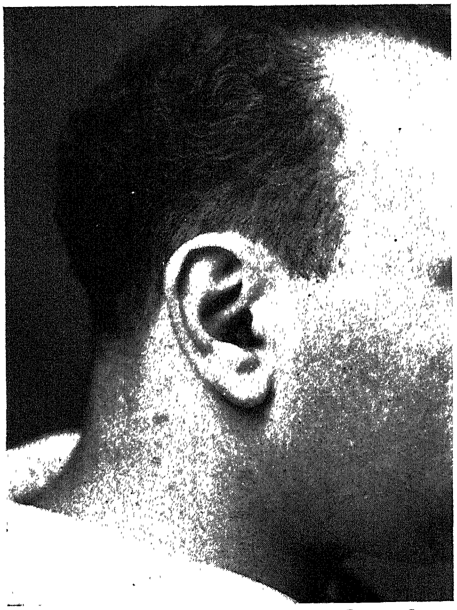
All too often when faced with a problem that contained so many unknowns in the equation, it has been necessary for the Navy Department to institute a program of research. After the research has been conducted and the data analyzed, a laboratory device must be designed and submitted to exhaustive tests. Finally, the laboratory model has to be transmuted into a production model. This takes time, valuable time.

In the matter of ear devices, time had been seized by the forelock. Research on ear protective devices had been going forward for a number of years. Dr. Verne Knudson, of the University of California, aided by a number of able assistants, had collected data on ear protectors and embodied the result of these researches into a number of fundamental designs. The Psycho-Acoustic Laboratory, Harvard University, working under a contract from the National Defense Research Committee, continued Knudson's investigations. At the same time, it conducted a program of comparative

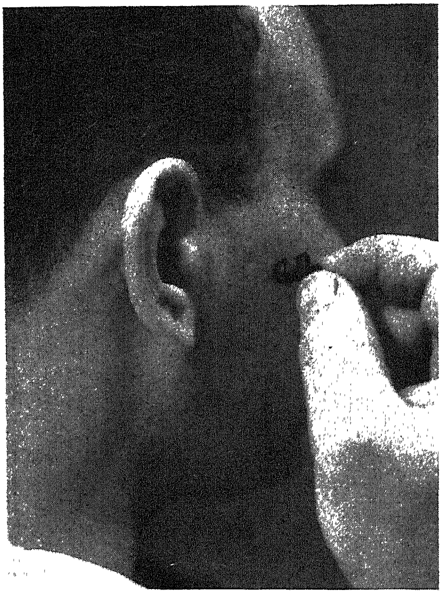
tests of such ear protectors as were commercially available. The sum total of all research was embodied in an ear protector known as the V-51R Ear Warden. Upon the basis of objective tests, this device seemed to be the nearest approach to a solution of the problem.

Test data definitely indicated that over the frequency range of audible sound waves the V-51R Ear Warden, in addition to being superior to other mechanical devices tested, offered greater protection than cotton. Further, under conditions of high noise level, intelligibility of speech over the sound-powered phones was increased when the ear wardens were used. These two basic conclusions were enough to warrant the procurement of the device for naval issue.

The possibility of infection of the ear canal by the introduction of the wardens was recognized. It was not certain, however, whether cotton or the wardens offered the greater hazard. Although cotton was antiseptic, the fingers that



U. S. Navy Photo
WARDEN IN PLACE



U. S. Navy Photo
REMOVING WARDEN

pulled it out of a container and stuffed it into the ear were not. The wardens could be sterilized without too much difficulty in an antiseptic solution.

Neoprene was selected as the material from which the ear wardens were to be molded. In addition to meeting the technical requirements, it was as nearly nontoxic as any other available material. People whose skin was irritated by rubber were not, in the great majority of cases, disturbed by neoprene. With an eye cocked toward the requirement of comfort, the designers insisted upon a degree of softness in the material that caused the manufacturers to writhe in anguish. They said it couldn't be done, but, to their everlasting credit, it was.

It is interesting to note that once the laboratory produced ear wardens that satisfied all interested parties and the specifications for their production were approved, it required eight months before the wardens started to roll off the production line. The over-all time from the decision to purchase large quantities for service issue to their actual commer-

cial production was approximately a year. There is no telling how long it would have taken to complete the basic research, if it had not been started before the war. These facts are introduced to buttress the argument for continuous research. Problems must be foreseen and the basic research conducted upon them long before the problem is recognized generally in the Fleet.

Ear wardens are now being given to the Fleet as general issue. As the tempo of production increases, they will have universal distribution. They should be used by all men exposed to gunfire or continuous high noise levels. Under such conditions they should be viewed in exactly the same light as a pair of goggles in a grinding operation. They represent a comfortable, usable safety device.

One thing remains to be done. We are now issuing devices to protect the hearing. We must now develop a device, or modify for service use an existing device, to test adequately the hearing of naval personnel. These tests should be conducted before a man is inducted and again before he is discharged. The click of a coin is a welcome sound in the pocket but a travesty as a test of hearing.

LT. COMDR. GEORGE W. DYSON

THE USE OF ATOMIC TRAIL BLAZERS IN PHYSIOLOGICAL INVESTIGATIONS WITH PLANTS

ONE of the greatest triumphs of mind over matter has been the discovery of methods of artificially transmuting one kind of atom into another. The atomic bomb is the most spectacular development from researches in this field, but other applications of this modern brand of alchemy hold greater promise for the welfare of man. Modern techniques of atomic physics make it possible to label certain molecules in such a way that they can subsequently be distinguished from other molecules of the same kind. The use of such identifiable molecules finds a number of important applications in biology and medicine.

The commonest method of tagging molecules is by incorporating into them atoms which have been rendered artificially radioactive. Such radioactive *isotopes* (isotopes of an element are different varieties of atoms of that element having different masses but the same nuclear charge) of many of the elements can be prepared by bombardment of the atomic nuclei in a cyclotron and in other ways. A radioactive isotope is identical in its chemical properties with the commoner isotopes of the same element, which for most elements are nonradioactive. A radioactive isotope of any element betrays its presence wherever it may be by the continual emission of radiations or charged particles which can be detected with suitable instruments. Thus it is possible to trace such molecules, after ingestion or absorption, through an organism and often even to determine the chemical reactions in which they participate. This has never been possible by ordinary methods of chemical analysis because by such methods it is impossible to distinguish between the introduced molecules and others of the same species which were already present in the organism.

The radioactive tracer technique has been successfully employed in a number of investigations of medical interest. Such representative problems as the accumulation of iodine in the thyroid gland, the phosphorus metabolism of the human body, and the role of iron in the synthesis of hemoglobin have all been investigated by this method. Important advances in our knowledge of the physiology of other animals, plants, and microorganisms have also been made with the help of radioactive tracers.

A natural application of the tracer technique is to the problem of the movement of substances in plants. The prin-

cipal tissues of translocation in plants are the xylem and the phloem, corresponding, in the stems of woody plants, to the wood and the inner bark, respectively. It has long been recognized that upward movement of water in plants occurs through the xylem (in woody plants only through the outer layers) and that downward movement of the foods synthesized in the leaves occurs in the phloem. Regarding the route traversed through the plant by the mineral salts absorbed from the soil there has been much less unanimity of opinion. Results of some investigators seem to indicate the phloem, of others the xylem, as the tissue through which most transport of mineral salts takes place from the roots to the aerial organs.

The use of radioactive compounds of sodium, potassium, and phosphorus has permitted a critical decision to be made between these two viewpoints. When willow and geranium plants were allowed to absorb such compounds, the results show unquestionably that their upward movement occurred in the xylem. No evidence was found of any upward movement in the phloem, although considerable lateral movement of the salts from the xylem to the phloem did take place.

In other experiments on the translocation of salts in plants the effect of injecting leaves with a radioactive phosphorus compound was tried. The molecules of this substance were found to be translocated out of the leaves and in the downward direction through the phloem, which is continuous from the leaves into the stem. Once in the stem a large proportion of the molecules moved laterally from the phloem into the xylem, in which they reascended the stem. These results suggest the occurrence of a regular circulation of at least some of the mineral salts in plants. Apparently most of the mineral salts absorbed by the roots of plants are translocated directly to the leaves. Some of the molecules of a salt may be retained in a leaf or used metabolically in the leaf cells, particularly if it is in an early stage of growth. Other molecules of the same salt may move out of the leaf through the phloem and eventually back into the same or other leaves through the xylem. A given molecule may repeat this translocation cycle several to many times before actually becoming immobilized in a cell.

Not only have the movements of mineral salts within plants been studied by this method, but knowledge regarding the mechanism of ionic exchanges between the roots and soil has been advanced by applications of this technique. A two-way movement of potassium ions between roots and soil has been demonstrated by this method. Radioactive potassium has been shown to move from the roots of barley plants into the surrounding medium at the same time that nonradioactive potassium was moving from the medium into the roots. These results indicate that a constant exchange of potassium ions in the medium with potassium ions in the roots is in progress, even when no net absorption of the ions by the roots is occurring.

Photosynthesis is another process which has been studied with scientific profit with the aid of tagged molecules. This is the process in which carbohydrates are synthesized in green plants from carbon dioxide and water with the accompanying release of oxygen. Photosynthesis occurs only in light which furnishes the necessary energy. The entire world of plants and animals operates at the expense of the energy and organic capital accumulated in photosynthesis. Although this reaction runs smoothly in any green leaf exposed to proper conditions, man has never been able to duplicate it in the laboratory, and the essential mechanism of the process has thus far eluded all experimental probings.

Recent experiments with radioactive

carbon have thrown a new and different light on the probable mechanism of photosynthesis. When green plants were allowed to absorb carbon dioxide made with artificially radioactive carbon, it was possible, by suitable chemical analyses, to determine the kinds of chemical compounds into which the tagged carbon atoms were incorporated. The results of these investigations indicate that the first step in photosynthesis—which may take place in the dark as well as in the light—is the conversion of the absorbed carbon dioxide into carboxyl (–COOH) groups attached to molecules of very large molecular weight. This represents a considerable departure from most previous theories of the mechanism of photosynthesis. One tenacious, but never well-founded, theory of photosynthesis has been that formaldehyde is an intermediate product in the synthesis of carbohydrates. No evidence could be found in this investigation that any of the tagged carbon was present in molecules of formaldehyde or similar compounds, and hence no evidence in favor of this theory could be found by this technique.

For some elements there are several isotopes which can be used as tracers in physiological investigations. Carbon is an example of such an element. The usual variety of carbon atom has an atomic weight of 12, but isotopes with atomic weights of 11, 13, and 14 are also known. The unstable, radioactive C-11 isotope is the one which was used in the previously described investigations on photosynthesis, but presents some difficulties in experimental work because its radioactivity is not retained very long. The C-14 isotope is also radioactive, but thus far has not been much used because it is a weak emitter of charged particles and therefore difficult to detect. C-13 is a stable, nonradioactive isotope, but by a modification in technique such isotopes can also be used as tracers. Their presence is detected, not by their emission of radiations or charged particles, but by their larger mass as compared with ordinary carbon. Such determinations can be made with an instrument known as a mass spectrograph. The distribution of the recently made photosynthate throughout bean plants has recently been traced by allowing the leaves to absorb carbon dioxide containing the C-13 isotope. Rapid translocation of carbohydrates synthesized in photosynthesis to the growing stem and root tips was readily demonstrated by this method. Translocation of the carbohydrates, whether in the upward or downward direction, was found to occur in the phloem, thus confirming, with an entirely new technique, the generally accepted idea of the pathway of transport of carbohydrates in plants.

B. S. Meyer

Department of Botany
The Ohio State University

COMMENTS AND CRITICISMS

Armchair Geology

I'm always glad to meet an old friend, and by the time I was two-thirds of the way through Mr. Chapman Grant's letter, in your January number, I knew I had found one.

A little over twenty-five years ago J. C. Branner, a geologist working on the Brazilian coast suggested the same hypothesis for the formation of beach cusps that Mr. Grant proposes. Branner's views are set forth in an article in the *Journal of Geology*, Volume 8, pages 481–484, for the year 1900. The idea received some credence for a time, especially among ''armchair'' geologists. It is an enticing hypothesis but it won't work. If Mr. Grant will check up by finding a smooth sand beach without cusps where cross-waves are coming in, I think he will see why.

Since the publication of my article, ''Scientific Beachcombing,'' I have been watching each number of THE SCIENTIFIC MONTHLY for some criticism, from your readers, of the use of the method. So far the result is zero. I was, of course, interested in the use of the subjective method by Paul D. Harwood. The biography of John Ericsson is a ''gem,'' but that is what we always expect from Owen Johnson.

O. F. EVANS

Oil, Water, and Vino

Dr. Adolph Knopf's article on strategic mineral supplies was fascinating and called attention to the magnificent work of geologists and engineers in exploration and inventory during the war.

However, he overlooked that group of geologists and engineers who find and produce the most valuable mineral of all, namely, water.

If national petroleum is a four-billion-a-year industry, national water is about an eight-billion-a-year industry. Overseas, the American soldier required more than 100 gallons of purified water a week (compared to 50 for petroleum products).

During the war huge water supplies had to be found and developed for hundreds of Army camps, thousands of industries, and many new and expanding cities and towns. Consulting water engineers, geologists, drillers, and chemists were strained to the utmost, while the Water Resources Branch of the Geological Survey, U. S. Department of Interior, answered thousands of water inquiries, and prepared plans for water supply in cooperation with the Army in all theaters of war.

Overseas, water production engineers outnumbered petroleum distribution engineers, and their mission was as vital. Rotary and cable tool well-drilling equipment was in continual use in search for underground water. As soldiers, too, water geologists fulfilled their mission of finding the ''strategic mineral.''

Water geology is too often overlooked by economic geologists when they consider the minerals of the earth. With all due respect to Dr. Knopf for his splendid article, he forgot the most important, and at times most strategic (e.g. El Alamein), mineral of them all.

I humbly quote the immortal words of Capt. William C. Rasmussen, water operations engineer of the Italian campaign, who has said: ''There are times when a gallon of water is almost worth a bottle of vino.''

WILLIAM C. RASMUSSEN

Zionism

For nine years I have looked in vain for an article on Zionism in THE SCIENTIFIC MONTHLY, so it is with great disappointment that I had to read the anti-Zionist statements masked under the title ''The Social Significance of Jewish-Christian Intermarriage'' by George Wolff.

It is with difficulty that I restrain resultant emotions on the subject to write you at this time. I recall also having written to you several years previously requesting publication of some reference to the achievements of the modern Jewish renascence in Palestine.

Evidently the front-page position of Palestinian news to-day prompted your printing the above-mentioned article. Unfortunately it expresses the opinions of the minority among Jews as well as Americans. (The Roper Poll recently showed American Jews to be 81 percent pro-Zionist, and the passage by both Houses of Congress of the Zionist Resolution for a Jewish Commonwealth with overwhelming majorities, proves how the American people as a whole insists its elected representatives vote.)

G. W. would have his reader believe that Zionism longs for ''a new Jewish theocracy in Palestine.'' Nothing could be further from the actuality, or even the hopes of the religious wing among Zionists (Mizrachi). On the contrary, ''Zionism is the expression of the national will to live of the Jewish people.'' (Quoted first sentence in the first chapter of *Zionist Education in the U. S.*, a survey by Samuel Dinin, Ph. D., published in October 1944 by the Education Committee of the Zion-

ist Organization of America, many of whose leaders are Rabbis.)

The author of the above-mentioned article insists that a "Jewish State would simply mean a glorified ghetto, narrow, undemocratic, and may well turn out to be reactionary." Such criticism is pure poppycock compared to the position of Zionist Palestine today. After much insistence on the part of Palestinian and world Jewry, reactionary colonial Britain allowed Palestine to present a Jewish Brigade against the tottering Nazis in the last days of the recent war. "Undemocratic" Jewish Palestine had 21 parties represented in the fall, 1944, elections. "Narrow" Jewish Palestine provided one of the chief strong points in the Near East of the Allies. The Hebrew University gave courses of advanced training for Allied Medical Officers in that theater. Jewish farms were an important source of fresh fruits, vegetables, and dairy products for Allied military personnel in the Near East.

Any GI who spent time in the rest centers near Tel Aviv looked on that spot as being of the widest horizon in that part of the world.

G. W. says "Nationalism has always and everywhere narrowed the mind." Does he think such is the case in American Nationalism? In Palestine the Jews are educating the Arab masses to progressive living (to wit: decreased Arab death rate, especially infantile, and increased Arab wages and standards of living). This latter activity has been the cause of feudal Arabian leaders' opposition to Zionism.

I wish he would have at least finished writing with the sentence "a permanent solution can only be found on the basis of an international covenant of nations." However, the irony of the situation that presents itself even now in the UNO is that the Jews, because they have not a few acres of soil they are free to call their own, cannot say a word in their own defense, or for their own welfare.

I. M. SATUREN, V.M.D.

Three Jeers for the Editor

It seems to me that you have done no service to THE SCIENTIFIC MONTHLY or the A. A. A. S. in publishing Owen Johnson's article on John Ericsson in your January issue. The MONTHLY ordinarily follows such standards of accuracy and interest that this aberration calls for comment.

I base my objection to the article on two general grounds: 1—The source; 2—The content.

1. *The Source.* It appears clearly below the ideals of the MONTHLY to accept material from a publication of such dubious integrity as the present *Reader's Digest.* At least it is to the credit of the MONTHLY to have acknowledged the source.

2. *The Content.* The article is an uncritical sanctification of Ericsson in the worst tradition of romantic biography. That, however, may be called a matter of taste and therefore only my personal opinion. So let us turn to some of Mr. Johnson's factual statements and implications and see whether they are appropriate to a scientific journal.

Take for a starter page 15, col. 2, where Johnson speaks of Robert Fulton "little dreaming that a genius was born who would *nullify all that he was creating*" (emphasis added). Comment on this leap of fancy seems superfluous.

On page 16, col. 1, Johnson speaks of "poverty, the greatest spur of all." I wonder how many will agree with that characterization, having in mind, let us say, Darwin and Marconi?

On page 18, col. 1, the author says of the paddle-wheel steamer, "but even under favorable circumstances it could develop a speed of only four to six knots." Johnson was born in New York; did he never see the old sidewheeler *Mary Powell* which for nearly sixty years reeled off sixteen to twenty knots on the Hudson?

On the same page, col. 2, Johnson discusses the Francis B. Ogden, Ericsson's screw steamer. Of the Admiralty he says, "They had listened to the engineer corps of the nation, which was arrayed *unanimously* against this ridiculous invention" (emphasis added). How does he know the opinion was unanimous? It seems unlikely, and if true, authority should have been given. Further on he says the Ogden smoothly and effortlessly moved . . . "without in *the least* shaking their convictions," and, "*Not one* had the *slightest* suspicion that he had taken part in the first successful demonstration . . . " (emphasis added). "A second time Ericsson had stood on the brink of fame and seen it denied him." Aside from the questionable use of superlatives here which have doubtful factual justification, the whole story is weighted in a sense with which at least one authority disagrees. The *Encyclopedia Britannica*, 1944, vol. 8, pp. 684–5 says:

"In 1836 he took out a patent for a screw-propeller, and though the priority of his invention could not be maintained, he was afterwards awarded a one-fifth share of the £20,000 given by the Admiralty for it. At this time Capt. Stockton, of the U. S. Navy, gave an order for a small iron vessel to be built by Laird of Birkenhead, and to be fitted by Ericsson with engines and a screw. This vessel reached New York in May 1839."

Nothing about these details in Mr. Johnson's article, but they quite alter the picture, don't they? Of course Mr. Johnson may be right and the *Britannica* wrong; but in that case at least a sentence of discussion seems required.

And finally Mr. Johnson says nothing about Ericsson's obstinacy in persisting with the colossal failure of the *Caloric*, a vessel powered with hot-air or "caloric" engines. She was built in defiance of sound theory and was a total loss on, I believe, her maiden voyage.

This letter is not intended as an attack on Mr. Johnson and still less on Ericsson, whose inventive successes are sufficient to maintain his reputation without suppression or coloring of fact. It is an attack on lending the support of THE SCIENTIFIC MONTHLY to a faulty and misleading biographical article that will doubtless be broadcast over the world by *Reader's Digest* as THE SCIENTIFIC MONTHLY'S.

In the above discussion I have selected only a few passages to illustrate my point. Many more could be cited and the discussion lengthened. But it hardly seemed worthwhile.

MARSTON L. HAMLIN

I am writing to comment on the short biography of John Ericsson which appeared in the January issue of THE SCIENTIFIC MONTHLY. It seems to me to be of a poor quality that does not often appear in the magazine, and if it represents the best efforts of a novelist there is a clear mandate to stick to scientific men as your contributors.

The exhortative tone is hardly suitable, and the bits of drama, the references to great but undemonstrated influences on subsequent history, are neither one typical of SCIENTIFIC MONTHLY articles. The paragraphs at the end of page 15 and continuing on page 16 exemplify these objectionable attitudes.

Moreover, the policy of accepting articles from the digest magazines does not seem appropriate to a scientific periodical even if they were of higher quality than this one. I should prefer to pay somewhat higher dues in order to be assured of the independence of the periodical from outside organizations.

If publication of the Ericsson article causes other reader comment, I would appreciate seeing a consensus of it on your editorial page.

DAVID H. MILLER

In "Meet the Authors," during the comment on Owen Johnson, it is suggested that *Reader's Digest* planted the article. Is it a policy of THE SCIENTIFIC MONTHLY to accept RD plants or is this the first occasion?

Since so many people, I think justifiably, look with deep suspicion on any material emanating from or "republished" by RD, I wonder if such articles should not be so labelled at their heading so that readers who wish to may apply their customary grain of salt where it seems called for. Perhaps SM does not make a policy of accepting plants which would seem to require such treatment, but RD is reputedly famous for slipping such material into seemingly innocuous articles on the bees and flowers.

What worries me is that should SM publish RD plants unannounced, it might cause readers to lose respect for or faith in the integrity of the whole magazine as being a stooge for *Reader's Digest.* That would be a great pity and quite undeserved.

AUSTIN W. MORRILL, JR.

O. F. Evans appears to be the only Owen Johnson fan among those who commented on "John Ericsson." The antagonists objected to the article on two counts: its lack of scientific accuracy and restraint; and its origin, from the *Reader's Digest.* The first objection is sound, and I plead guilty of having accepted a manuscript from a nonscientific source without checking the validity of the facts and interpretations given. As a rule we can rely on the accuracy of our articles, because most of them are written by scientists. One might expect a novelist to be more concerned with dramatic effects than with plodding facts, and it was for the drama of Johnson's article that I accepted it, hoping that the facts had been treated with respect. I have contended, and still assert, that it is possible for scientists to inject drama into their stories without violation of facts. I hoped that the article on Ericsson might encourage scientists to experiment with a more attractive style of writing than they habitually use, exemplified recently by O. F. Evans on beach cusps and Paul D. Harwood on phenothiazine.

The implication that there is something sinister in accepting an article offered by the *Reader's Digest* seems unjustified. Like any other manuscript that we consider, the article on Ericsson was offered gratis. I was free to accept or reject it. In answer to reader Morrill's question, this was my first occasion. If there should be a second, I will, if necessary ask for the opinions of appropriate advisers.—ED.

THE BROWNSTONE TOWER

At the forthcoming meetings of the A. A. A. S. in St. Louis, March 27–30, 1946, professional science writers, representing the country's great newspapers and magazines, will report scientific developments as revealed by papers presented at the meetings. As a rule, these men and women do not get their information by listening to speakers as they appear on the program; they must depend largely on the contents of manuscripts or abstracts of manuscripts submitted by the authors to the Press Service of the Association prior to the opening of the meetings.

Although the information provided by scientists for the use of science writers has been improving in recent years both in quantity and in quality, we feel that many scientists still do not fully realize the importance of submitting information about their work to the press. They may think that their work is of no interest to the public or that the science writers are not capable of reporting it correctly. It is true, of course, that some investigations are of greater interest to the public than others, but no scientist should assume that his work is too recondite to have meaning to laymen if properly interpreted. He should submit the desired manuscripts or abstracts and let the science writers judge their news value. These writers are competent scientists who can and will report accurately those features of any investigation that seem to them to have significance to the public. They have a broader understanding of the advancing front of science than any specialist in research. It is largely through their reporting of scientific progress that public understanding and support of research are increased.

Any scientist who is convinced that he owes it to himself, his institution, and the public to make the results of his work available to science writers should then take the trouble to submit adequate information about his work. It is most helpful to send to the Office of the Association for the use of the Press Service two copies of each manuscript and two copies of an abstract of it. The abstract, if properly written, will enable the science writer to determine quickly whether the investigation should be reported; he can then turn to the manuscript for details, if needed. If copies of the manuscript cannot be provided, the abstract should be made to contain all essential information: the purpose of the work, the methods used, the results obtained, and the technical significance of the results. Science writers do not need or desire abstracts written down to an assumed layman's level. Technical terminology is familiar to them, and they prefer a thorough professional abstract to a scientific bedtime story. Science writers complain most bitterly of inadequate abstracts (summaries) which state only that certain problems or subjects were studied without telling exactly what was found out. Let it be resolved, therefore, to give the Press Service complete information.

In the SM comments on previously published articles cannot, as a rule, be included in the issue immediately following the articles in question. Consequently, readers may have forgotten the contents of these articles by the time criticisms of them appear. As readers cannot be expected to reread the older articles, it is desirable that comments should be intrinsically interesting and understandable without reference to the articles that gave rise to them. It might be desirable for commentators to summarize the points on which they are commenting.

In a few instances we have published short original articles in Comments and Criticisms to save space. We shall limit this practice and discontinue it if possible.

F. L. CAMPBELL

THE SCIENCE LIBRARY

A.A.A.S. MEETINGS, ST. LOUIS, MARCH 27–30, 1946

THE revival of annual meetings by the whole Association, after a lapse of nearly five years except for the meeting in Cleveland in 1941, brings with it the resumption of the Science Exhibition and one of its notable features, The Science Library. Scientists who have traveled the circuit of Association meetings in the past twenty years will well remember this part of the meetings; younger scientists who will be attending a meeting of the Association for the first time have a treat in store for them. Through the cooperation of book publishers, university presses, libraries, and foreign governments a collection of more than one thousand of the most important recent scientific books and publications has been secured and will be placed conveniently for examination. Practically every American publisher of scientific material is represented in The Science Library. With the help of various foreign embassies and information services, it is expected that there will be several hundred books from overseas. The inclusion of foreign books on so great a scale is an innovation which it is hoped will lead to even larger exhibits as the years and the meetings pass.

On the following pages there appears a complete list of the books submitted for The Science Library as this issue of the SM goes to press. Some publishers have not yet submitted the titles they expect to ship to St. Louis, and the time available after it became possible to hold a meeting has not been sufficient to receive new books from overseas. But they have been promised and are said to be on the way. It is expected that in the six weeks that will elapse between this writing and the opening of the meeting in St. Louis all the necessary information about foreign books and publications will be available, and it will be incorporated in the reprints of the lists to be distributed at The Science Library.

The books are classified by major branches of science. In addition, each publisher has keyed his titles so that their level of use can easily be determined. The key letters appear at the end of the citation for each book and can be interpreted from the following explanation:

B: Biography.
GS: General Science.
J: Juvenile.
P: Popular.
R: Reference.
S: Study and Teaching.
TH: Textbooks for High Schools.
TU: Textbooks for University and College.

The Science Library will be open from 8:30 A.M. to 6:00 P.M. Through the cooperation of the local universities and the St. Louis Public Library trained librarians will be in attendance all day. Members who wish to order copies of books that they have inspected may do so by means of the publishers' order blanks, which will be forwarded to their offices at the end of each day.

The staff members of the Association who have worked on this project extend an invitation to every attending member and visitor to browse among this unusual collection of scientific books. The entire Science Library Exhibition will be located in the St. Louis Municipal Auditorium, and ample facilities will be available for an unhurried inspection of these books.

General

BAITSELL, G. A. (ed.). *Science in Progress.* First Series. 322 pp. (1st ed.) 1939. $4.00. Yale. R.

BAITSELL, G. A. (ed.). *Science in Progress.* Second Series. 317 pp. (1st ed.) 1940. $4.00. Yale. R.

BAITSELL, G. A. (ed.). *Science in Progress.* Third Series. 322 pp. (1st ed.) 1942. $3.00. Yale. R.

BAITSELL, G. A. (ed.). *Science in Progress.* Fourth Series. 331 pp. (1st ed.) 1945. $3.00. Yale. R. (Four volumes of *Science in Progress* sold as set. $12.50.)

BAWDEN. *Man's Physical Universe, rev.* 832 pp. $4.00. Macmillan. TU.

FLOHERTY, J. J. *Behind the Microphone.* 207 pp. (1st ed.) 1944. $2.00. Lippincott. J.

FLOHERTY, J. J. *Flowing Gold.* 256 pp. (1st ed.) 1945. $2.50. Lippincott. P.

FLOHERTY, J. J. *Inside the F.B.I.* 192 pp. (1st ed.) 1942. $2.00. Lippincott. P.

FURNAS, C. C. *Storehouse of Civilization.* 562 pp. (1st ed.) 1939. $3.25. Columbia University. R.

GILL, HENRY V. *Fact and Fiction in Modern Science.* viii plus 136 pp. (2nd printing) 1945. Reg. ed., cloth: $2.50. Fordham University Press. P.

GILL, HENRY V. *Fact and Fiction in Modern Science.* viii plus 136 pp. (2nd printing) 1945. College ed., paper: $1.00. Fordham University Press. TU.

GUBERLET, MURIEL. *Hermie's Trailer House.* 32 pp. (1st printing) 1945. $1.25. Cattell. J.

HARRISON, GEORGE RUSSELL. *How Things Work: Science for Young Americans.* 288 pp. (1st ed.) 1941. $2.75. Morrow. J.

HUNTINGTON, E. *Mainsprings of Civilization.* 660 pp. (1st ed.) 1945. $4.75. Wiley. TU.

ILIN, M. (pseud). *Ring and a Riddle.* 70 pp. (1st ed.) 1944. $2.00. Lippincott. J.

ILIN, M. (pseud). *100,000 Whys.* 137 pp. (1st ed.) 1933. $1.60. Lippincott. J.

LOW. *Science Looks Ahead.* 640 pp. (1st ed.) 1942. $4.50. Oxford. J.

LUCAS, J. M. *Indian Harvest.* 120 pp. (1st ed.) 1945. $2.00. Lippincott. J.

MACNEIL. *Between Earth and Sky.* 64 pp. (1st ed.) 1944. $1.50. Oxford. J.

MCCONNELL, J. *Nurse, Please!* 18 pp. (1st ed.) 1944. $1.00. Lippincott. P.

MOORE, DOM, T. V. *Principles of Ethics.* 405 pp. (1st ed.) 1943. $3.00. Lippincott. R.

POLLACK, PHILLIP. *Careers in Science.* 222 pp. (1st ed.) 1945. $2.75. E. P. Dutton. P.

Science in Soviet Russia, A Symposium of Papers Presented at Congress of American-Soviet Friendship New York City. 100 pp. (1st printing). 1944. $1.50. Cattell.

SELL. *Comprehensive English-Spanish Technical Dictionary.* 1400 pp. (1st ed.) 1944. $30.00. McGraw-Hill.

VAN NOSTRAND. *Scientific Encyclopedia.* 1234 pp. (1st ed.) 1938. $10.00. Van Nostrand. R.

YATES, R. F. *Machines over Men.* 250 pp. (1st ed.) 1939. $2.00. Lippincott. R.

Year Book No. 43. 206 pp. 1944. $1.00 paper; $1.50 cloth. Carnegie Inst. R.

YOST, E. *American Women of Science.* 232 pp. (1st ed.) 1943. $2.00. Lippincott. B.

YOST, E. *Modern Americans in Science and Invention.* 269 pp. (1st ed.) 1941. $2.00. Lippincott. B.

Aeronautics

BENHAM, H. E. *Aerial Navigation.* 344 pp. (1st ed.) 1945. $4.00. Wiley. TU.

FLOHERTY, J. J. *Aviation from Shop to Sky.* 209 pp. (1st ed.) 1941. $2.00. Lippincott. J.

GLAUERT. *Elements of Aerofoil and Airscrew Theory.* 228 pp. $3.50. Macmillan. R.

HADINGHAM, RONALD. *Astronomical Air Navigation.* 132 pp. 1944. $2.50. Crowell. TU & R.

KELLS, KERN, and BLAND. *Navigation.* 479 pp. (1st ed.) 1943. $5.00. McGraw-Hill. TU.

LIMING. *Practical Analytic Geometry with Applications to Aircraft.* 328 pp. $4.50. Macmillan. TU.

MUDGE. *Meteorology for Pilots.* 259 pp. (1st ed.) 1945. $3.00. McGraw-Hill. TU.

NIKOLSKY, ALEXANDER A. *Notes on Helicopter Design Theory.* 236 pp. (1st ed.) 1944. $3.00. Princeton. R.

PARKINSON. *Aerodynamics.* 112 pp. $2.25. Macmillan. P.

ROBERTS, HENRY W. *Aviation Radio.* 652 pp. (1st ed.) 1945. $5.00. Morrow. P.

SERRALES. *English-Spanish and Spanish-English Dictionary of Aviation Terms.* 131 pp. (1st ed.) 1944. $2.50. McGraw-Hill.

STEWART, NICHOLS, WALLING, and HILL. *Aircraft Navigation.* 146 pp. $1.50. Macmillan. TU.

VETTER, ERNEST G. *Visibility Unlimited: An Introduction to the Science of Weather and the Art of Practical Flying.* 356 pp. Illus. (1st ed.) 1942. $4.00. Morrow. P.

VON MISES. *Theory of Flight.* (1st ed.) 1945. $6.00. McGraw-Hill. TU.

Agriculture

ANDERSON. *Introductory Animal Husbandry.* 777 pp. $4.00. Macmillan. TU.

BARGER, E. H., and CARD, L. E. *Diseases and Parasites of Poultry.* 399 pp. (3rd ed.) 1943. $3.75. Lea & Febiger. TU.

BAVER, L. D. *Soil Physics.* 370 pp. (1st ed.) 1940. $4.00. Wiley. TU.

BAXTER, D. V. *Pathology in Forest Practice.* 627 pp. (1st ed.) 1943. $5.50. Wiley. TU.

BIESTER, H. E., and DE VRIES, LOUIS, (ed.). *Diseases of Poultry.* 1,020 pp. (1943, 3rd printing 1945) $8.50. Collegiate Press, Inc. T.

BLACK, JOHN D. *Food Enough.* 280 pp. (1st printing) 1943. $2.50. Cattell. P.

BRANDT, KARL. *The Reconstruction of World Agriculture.* 416 pp. (1st ed.) 1945. $4.00. Norton. P.

BRUNNER, E. DES. *Farmers of the World.* 208 pp. (1st ed.) 1945. $2.50. Columbia. S.

BUNCE, ARTHUR C. *Economics of Soil Conservation.* 227 pp. (1st ed.) 1942. $3.00. Collegiate Press, Inc. R.

CHANDLER, W. H. *Deciduous Orchards.* 438 pp. (1st ed.) 1942. $4.50. Lea & Febiger. TU.

CLARKE. *The Study of the Soil in the Field.* 228 pp. (3rd ed.) 1941. $2.25. Oxford. S.

ESPE, DWIGHT. *Secretion of Milk.* 350 pp. (1938, 3rd ed. 1945) $3.25. Collegiate Press, Inc. T.

GRAHAM. *Natural Principles of Land Use.* 288 pp. (1st ed.) 1944. $3.50. Oxford. P.

GUSTAFSON. *Soils & Soil Management.* 424 pp. (1st ed.) 1941. $3.00. McGraw-Hill. TU.

HAMMER, B. W. *Dairy Bacteriology.* 482 pp. (2d ed.) 1938. $5.00. Wiley. TU.

HOGNER. *Farm Animals.* 196 pp. (1st ed.) 1945. $3.50. Oxford. J.

MILLAR, C. E., and TURK, L. M. *Fundamentals of Soil Science.* 462 pp. (1st ed.) 1943. $3.75. Wiley. TU.

KRYNINE. *Soil Mechanics.* 451 pp. (1st ed.) 1941. $5.00. McGraw-Hill. TU.

LUSH, JAY L. *Animal Breeding Plans.* 444 pp. (1937, 3rd ed. 1945) $3.50. Collegiate Press, Inc. T.

LYON & BUCKMAN. *Nature and Properties of Soils.* 499 pp. (4th ed.) $3.50. Macmillan. TU.

MARSHALL & HALNAN. *Physiology of Farm Animals.* 339 pp. (3rd ed.) $4.50. Macmillan. R.

MERCHANT, I. A. *Veterinary Bacteriology.* 640 pp. (3rd ed.) 1945. $6.50. Collegiate Press, Inc. T.

PERRY, ENOS J. *The Artificial Insemination of Farm Animals.* 266 pp. (1st ed.) 1945. $3.50. Rutgers. S & P.

SCHULTZ. *Redirecting Farm Policy.* 75 pp. $1.00. Macmillan. R.

VAN DERSAL. *The American Land: Its History and Uses.* 231 pp. (1st ed.) 1943. $3.75. Oxford. P.

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BOAS, FRANZ, and others. *General Anthropology.* 729 pp. (1st ed.) 1938. $4.00. Heath. TU.

COLE-COOPER. *Peoples of Malay.* 362 pp. (1st ed.) 1945. $4.00. Van Nostrand. R.

CRESSMAN, L. S., and collaborators. *Archaeological Researches in the Northern Great Basin.* 158 pp. 1943. $3.00 paper; $4.00 cloth. Carnegie Inst. GS.

DUBOIS, CORA. *The People of Alor.* 654 pp. (1st ed.) 1944. $7.50. Univ. of Minnesota. R.

GRAY. *Man and His Physical World.* 665 pp. (1st ed.) 1942. $3.75. Van Nostrand. TU.

ILIN, M. (pseud). *How Man Became a Giant.* 265 pp. (1st ed.) 1942. $2.00. Lippincott. J.

KENNEDY, R. *Bibliography of Indonesian Peoples and Cultures.* 212 pp. (1st ed.) 1945. $2.50. Yale. R.

KIDDER, A. V., and RUPPERT, K., *et al. Contributions to American Anthropology and History Vol. VIII, Nos. 40 to 43.* 260 pp. 1944. $3.50 paper; $4.00 cloth. Carnegie Inst. GS.

LEIGHTON, ALEXANDER H. (Lt. Comdr. USNR, Medical Corps). *The Governing of Men.* 420 pp. (1st ed.) 1945. $3.75. Princeton. P.

LINTON, RALPH. *Cultural Background of Personality.* 157 pp. (1st ed.) 1945. $1.50. Appleton-Century. R.

LINTON, R. *The Science of Man in the World Crisis.* 520 pp. (1st ed.) 1945. $4.00. Columbia. S.

LOWIE, R. H. *An Introduction to Cultural Anthropology.* 584 pp. 1940. $4.00. Rinehart. TU.

MACCURDY, G. G. *Early Man.* 362 pp. (1st ed.) 1937. $5.00. Lippincott. S.

MALINOWSKI, B. *The Dynamics of Culture Change.* 171 pp. (1st ed.) 1945. $2.50. Yale. R.

MEAD, MARGARET. *From the South Seas: Studies of Adolescence and Sex in Primitive Societies.* 1088 pp. (1st ed.) 1939. $4.00. Morrow. P.

MONTAGU, M. F. ASHLEY. *Man's Most Dangerous Myth: The Fallacy of Race.* 304 pp. (2nd ed.) 1945. $3.25. Columbia. S.

MURDOCK, G. P., *et al.* *Outline of Cultural Materials.* 56 pp. (1st ed.) 1945. $1.00. Yale. R.

ROYS, RALPH L. *The Indian Background of Colonial Yucatan.* 244 pp. 1944. $1.75 paper; $2.75 cloth. Carnegie Inst. GS.

RUPPERT, KARL, and DENISON, JR. JOHN H. *Archaeological Reconnaissance in Campeche, Quintana Roo, and Peten.* 156 pp. 1943. $4.25 paper; $4.75 cloth. Carnegie Inst. GS.

VILLA, ALFONSO R. *The Maya of East Central Quintana Roo.* 182 pp. 1945. $2.25 paper; $2.75 cloth. Carnegie Inst. GS.

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BAKER. *Astronomy.* 527 pp. (3rd ed.) 1938. $3.75. Van Nostrand. TU.

BARTON and BARTON. *Guide to the Constellations.* 80 pp. (3rd ed.) 1943. $3.00. McGraw-Hill. P.

BEET. *A Text-book of Elementary Astronomy.* 110 pp. $2.00. Macmillan. TU.

BOK, B. J. and P. F. *The Milky Way.* 224 pp. (2nd ed.) 1945. $3.00. Blakiston. S.

CAMPBELL, L., and JACCHIA, L. *The Story of Variable Stars.* 226 pp. (1st ed.) 1941, reprinted 1945. $3.00. Blakiston. S.

CHANDRASEKHAR, SUBRAHMANYAN. *Introduction to Stellar Structure.* 509 pp. (1st ed.) 1939. $10.00. Univ. of Chicago. R.

CHANDRASEKHAR, SUBRAHMANYAN. *Principles of Stellar Dynamics.* 251 pp. (1st ed.) 1942. $5.00. Univ. of Chicago. R.

DIMITROFF, G. Z., and BAKER, J. G. *Telescopes and Accessories.* 309 pp. (1st ed.) 1945. $3.00. Blakiston. S.

DUNCAN, JOHN CHARLES. *Astronomy.* 520 pp. (4th ed.) 1946. $4.50. Harper. TU.

DUNHAM. *What's in the Sky?* 48 pp. (1st ed.) 1941. $1.00. Oxford. J.

DYSON and WOOLLEY. *Eclipses of the Sun and Moon.* 168 pp. (1st ed.) 1937. $5.50. Oxford. S.

FATH. *Elements of Astronomy.* (4th ed.) 1944. $3.00. McGraw-Hill. TU.

FISHER, C., and LOCKWOOD, M. *Astronomy.* 205 pp. (1st ed.) 1940. $2.25. Wiley. TU.

GOLDBERG, L., and ALLER, L. H. *Atoms, Stars and Nebulae.* 323 pp. (1st ed.) 1943, reprinted 1945. $3.00. Blakiston. S.

MIEVILLE. *Astronomical Navigation Without Math.* 25 pp. $.65. Macmillan. P.

PANETH. *The Origin of Meteorites.* 27 pp. (1st ed.) 1940. $.85. Oxford. S.

SHAPLEY, H. *Galaxies.* 229 pp. (1st ed.) 1943, reprinted 1945. $3.00. Blakiston. S.

SHUTE, SHIRK, PORTER, and HEMENWAY. *Introduction to Navigation and Nautical Astronomy.* 457 pp. $4.50. Macmillan. TU.

SMART. *Text-book of Spherical Astronomy,* (4th ed.) 420 pp. $4.75. Macmillan. TU.

STARR, VICTOR P. *Basic Principles of Weather Forecasting.* 299 pp. (1st ed.) 1942. $3.00. Harper. TU & R.

WATSON, F. G. *Between the Planets.* 222 pp. (1st ed.) 1941, reprinted 1945. $3.00. Blakiston. S.

WHIPPLE, F. L. *Earth, Moon and Planets.* 293 pp. (1st ed.) 1941, reprinted 1945. $3.00. Blakiston. S.

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ADOLPH, EDWARD F. *Physiological Regulations.* 500 pp. (1st printing) 1943. $7.50. Cattell. R.

BAKER, F. C. *The Molluscan Family Planorbides.* 530 pp. (1st ed.) 1945. $14.50. Univ. of Illinois. S.

BAWDEN, F. C. *Plant Viruses and Virus Diseases.* 294 pp. (2nd rev. ed.) 1943. $4.75. Chronica Botanica Co.

BEATY, J. Y. *Nature is Stranger than Fiction.* 283 pp. (1st ed.) 1940. $2.00. Lippincott. J.

BECKER, E. R., and ROUDABUSH, R. L. *Brief Directions in Histological Technique.* 90 pp. (1935, 5th printing 1945). $1.00. Collegiate Press, Inc. T.

Biological Symposia, Vol. IV. 302 pp. (1st printing) 1941. $3.00. Cattell. R.

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Biological Symposia, Vol. VI. 360 pp. (1st printing) 1942. $3.50. Cattell. R.

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Biological Symposia, Vol. IX. 145 pp. (2nd printing) 1942. $2.50. Cattell. R.

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BOURNE. *Cytology and Cell Physiology.* 308 pp. (1st ed.) 1942. $6.00. Oxford. S.

BRELAND. *Manual of Comparative Anatomy.* 250 pp. (1st ed.) 1943. $2.00. McGraw-Hill. TU.

BRIDGES, CALVIN B., and BREHME, KATHERINE S. *The Mutants of Drosophila Melanogaster.* 257 pp. 1944. $2.50 paper; $3.00 cloth. Carnegie Inst. GS.

BRODY, SAMUEL. *Bioenergetics and Growth.* 1023 pp. (1st ed.) 1945. $8.50. Reinhold. R.

BRUERE, M. B. *Your Forests.* 159 pp. (1st ed.) 1945. $2.50. Lippincott, J.

CAIN, STANLEY A. *Foundations of Plant Geography.* 556 pp. (1st ed.) 1944. $5.00. Harper. TU & R.

CAMPBELL, ARTHUR S. *The Oceanic Tintinnoina of the Plankton Gathered during the Last Cruise of the Carnegie.* 163 pp. 1942. $1.50 paper; $2.50 cloth. Carnegie Inst. GS.

CHANDLER, A. C. *Introduction to Parasitology.* 716 pp. (7th ed.) 1944. $5.00. Wiley. TU.

CHANEY, RALPH W., *et al. Pliocene Floras of California and Oregon.* 407 pp. 1944. $4.50 paper; $5.00 cloth. Carnegie Inst. GS.

CLAUSEN, JENS, KECK, DAVID D., and HIESEY, WM. M. *Experimental Studies on the Nature of Species. II. Plant Evolution through Amphiploidy and Autoploidy.* 174 pp. 1945. $1.25 paper; $2.00 cloth. Carnegie Inst. GS.

CLEMENTS, F. E., and SHELFORD, V. E. *Bio-Ecology.* 425 pp. (1st ed.) 1939. $4.50. Wiley. TU.

COLE. *A History of Comparative Anatomy.* 524 pp. $7.00. Macmillan. R.

COMSTOCK, ANNA B. *The Handbook of Nature Study.* xx plus 937 pp. Illus. (24th ed.) 1939. $4.50. Comstock. TH.

COWDRY, E. V. *A Textbook of Histology.* 426 pp. (3rd ed.) 1944. $7.00. Lea & Febiger. TU.

CURTIS, W. C., and GUTHRIE, M. J. *Textbook of General Zoology.* 682 pp. (3rd ed.) 1938. $3.75. Wiley. TU.

DAVSON & DANIELLI. *Permeability of Natural Membranes.* 361 pp. $4.75. Macmillan. R.

DETWILER. *Vertebrate Photoreceptors.* 184 pp. $4.00. Macmillan. R.

DOBZHANSKY, TH., and EPLING, CARL. *Contributions to the Genetics, Taxonomy and Ecology of Drosophila pseudoobscura and its Relatives.* 183 pp. 1944. $2.25 paper; $2.75 cloth. Carnegie Inst. GS.

DODDS, G. S. *Human Embryology.* 314 pp. (3rd ed.) 1946. $4.00. Wiley. TU.

DODGE and RICKETT. *Diseases and Pests of Ornamental Plants.* 638 pp. (1st printing) 1943. $6.50. Cattell. R.

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Several weeks have elapsed since the original lists for The Science Library were compiled for printing. During that time we have had more definite word from our overseas correspondents about shipments of books to St. Louis. The following agencies and organizations have informed us that books from their respective countries are now on the way:

American-Soviet Science Society
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The Netherlands Information Bureau
British Information Services
Union of South Africa Government Information Office
The Australian Legation
Swedish Universities and Technical Societies

Every effort will be made to have these books listed in the reprints to be distributed at St. Louis.

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HOPES AND HURDLES IN THE CANCER PROBLEM

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WHENCE comes this optimism that the problem of cancer can be solved? There is hope where a generation ago there was apathy bordering on despair. My friend Seelig reminds me of the notice "All ye who enter here leave hope behind" displayed by a wag over the door of Paul Ehrlich's laboratory when the master had the temerity to attack the cancer problem. And I can well remember being discouraged by my betters from work on cancer when I was a student. Now young men and women who come to the universities flushed with enthusiasm and ideals are not so regularly turned aside from work on cancer with the admonition "Don't waste your time." Instead they are encouraged to get the training and experience without which they cannot expect to make progress.

Today's optimism is not altogether because of the feeling that soon we may be able to put away the smoked glasses and the begging bowl and get along with the job, without having to spend half our time trying to raise the money to pay for experiments. This attractive possibility of getting research adequately financed stems from the fact that the public is becoming interested. A Gallup Poll released on July 21, 1945, shows that 81 percent of Americans favor a Congressional appropriation of $200,000,000 for the study and treatment of cancer and that 75 percent are willing to pay more taxes to provide the money. This poll was taken soon after the national campaign of the American Cancer Society. Let us hope that action will follow before the inevitable swing toward economy in expenditure of Federal funds sets in. The encouraging thing is that people are thinking in terms larger than before, though still microscopic compared with the human need to get rid of cancer, with its annual death rate of about 163,000 a year.

Some of the optimism may be attributed to the rather uncritical accounts of "wonder drugs" dished up for the public. These lead people to believe that medical science can leap over hurdles, which is true. But in cancer there are many hurdles, high and unique in character, as will be explained later. It would be unsafe to bank too much on the argument that because diphtheria, for instance, can be prevented, cancer can also be prevented by some simple means. The medical profession is *still* "from Missouri."

Probably the best way to trace this optimism to its real source is to talk with people who are actually investigating the problem in hospitals, research institutes, and universities; they are the ones who should know cancer best. A few are dejected, but many are somewhat excited by the results they are getting. They

feel that they are on the right track—sometimes after several false starts—and will talk enthusiastically of their experiments for hours. They cool off only (and promptly) when you switch the conversation from their work to yours. Without this consuming faith in what they are doing and refusal to spread their time and interest too thin, they would become useless. Also, they must use judgment in the interpretation of their results. Their habit of self-criticism is good. The difficulty is properly to limit this essential trait, for not infrequently they are also extra-critical of others, and, since diverse experiments have a place in the attack, their judgment is often based on inadequate knowledge of exactly what the other fellows actually are trying to do. Consequently, the visitor is apt to become confused by the mixture of justifiable and loose criticism, and this confusion is likely to increase the more cancer research laboratories he visits. The truth is that there are many right tracks, and that cancer can be attacked from more angles than any other major medical problem.

Why are there so many trails in the cancer problem that can be explored to advantage? One reason is that cancer can be studied not only in man but in a wide range of animals. All kinds of domestic animals—dogs, sheep, cats, goats, horses, and so on—suffer from cancer more or less frequently. Investigations of cancer in fish, frogs, chickens, and mice have been particularly illuminating. Animals of so many sorts are known to suffer from cancer that diligent search has been made for some species the individuals of which never have cancer. It was thought, rightly, that if some such cancer-free species could be found, comparison of it with another species abundantly afflicted with cancer might reveal the nature of the factors that determine resistance and susceptibility to this disease. Accordingly, zoological gardens have been ransacked for some cancer-free species, and animals collected in expeditions in foreign lands have been examined for cancer-free sorts, but without success. In no instance have negative results been obtained in a sufficient number of animals belonging to a single species to justify the conclusion that the species is free from cancer. So much for the vertebrates.

In invertebrates, also, cancer does occur, but its range of distribution is still more obscure. Even in plants a disease rather remotely resembling cancer has been discovered and therefore botany is well represented on the Committee of the National Research Council, financed by the American Cancer Society. Cancer is, then, the antithesis of mental disease. The psychiatrist can operate only at the human level where thoughts can be conveyed by speech. The cancer investigator can operate at numerous levels, perhaps at all the principal levels in the animal scale.

Other pioneer workers tried to find a parallel contrast in man, that is, between a cancer-free race or tribe and a cancer-rich one, again to get at the resistance and susceptibility factors. The first reports that cancer is absent in the Eskimos and others were invariably followed by the discovery of cancer when more individuals were examined. It is also clear that cancer in man is not a modern disease, but has been with us from prehistoric times.

A second reason why cancer supplies such abundant openings for research is found in the fact that, unlike most other diseases, it is not limited to a particular age group. It can develop from before birth to extreme old age. Though the peak of greatest number of deaths from cancer in men is from 60–69, and in women from 50–59, there were in 1942 more deaths from cancer in children under five years of age than from infan-

tile paralysis, or from acute nephritis, or from all diseases of the ear and mastoid lumped together. And in the same year deaths from cancer in the age groups 5–9 and 10–14 exceeded those from dysentery, whooping cough, diphtheria, or measles, which goes to show that, while cancer is not common in infants and children, it is deadly.

Third, cancer can develop in more parts of the body than most other diseases, so that the tissues and the ages at which it can be studied are varied. It is, of course, a disease of cells. For some unknown reason the cells are so modified that they become malignant, in the sense that they multiply without the usual bodily restraints, invade the territory belonging to other cells, and continue to monopolize everything until the patient dies, often extremely emaciated. A committee appointed by Surgeon General Parran has made the pronouncement that "malignancy is a universal cell potentiality, in that any cell has inherent in its make-up the potentialities for unlimited and uncontrolled growth." This may be too sweeping a generalization, but the fact of interest here is that the cancer investigator can profitably study the origin of cancer in cells of many sorts, each one of which poses a problem in itself which demands a concentration of techniques, physical and biochemical, physiological and pharmacological, pathologic and genetic. Multiplying the number of kinds of cells by the number of experts who should study each, one obtains a large figure.

A further consequence of the fact that cancer can present itself for study in any part of the body is difficulty in the establishment of departments of cancer in our medical schools. If created, such departments could not be self-contained units like those of gynecology, ophthalmology, urology, and the other specialties. They could only be superdepartments, that is, cancer hospitals, or else weak organizations treating and studying patients on suffrage in other departments throughout the school: cancer of the breast in the department of surgery, cancer of the uterus in the department of gynecology, cancer of the prostate in the department of urology, and so on.

A fourth feature of cancer which sets it apart from other diseases is the far greater number of agents that can produce it. The simplicity of syphilis (caused by a single type of spiral organism) or of tuberculosis (caused by a single kind of bacterium) is wholly lacking in cancer. The cancer-producing agents are called carcinogens, because under certain conditions they can generate cancer. They are of two kinds: external and internal.

Sunlight is the most universal of external carcinogens, but its power to produce cancer is relatively feeble. Long exposure through many years is ordinarily required. Cancer of the lip, caused by shorter, but more intense, exposure to sunlight, is one of the prices paid by some of our Pacific veterans. Dust, especially in particular kinds of mines, X-rays, radium and radioactive substances, many coal-tar products, particularly tars and certain aniline dyes, and numerous other chemical substances can produce cancer if conditions are favorable. And cancer tends in a small minority of cases to follow burns and mechanical injuries, such as blows on the bones, breasts, and testicles.

The carcinogens that strike from within are more difficult to identify. The male, and more definitely the female, sex hormones are under suspicion as possibly belonging in this category; but, again, one has to stress the word "potential" for they usually do not cause cancer. Without the use of these and other steroid compounds, evolution, as we know it, could not have taken place.

Still another property of cancer con-

tributes to our perplexity. Sometimes it is easy to fix the date of last exposure to a carcinogen and to measure the time elapsing before cancer appears. A worker, after a varying length of service in a dye factory, may change his occupation and never again be subjected to the same cancer hazard. Statistics show that he may develop cancer of the urinary bladder one to seventeen years later. After the last repeated exposure to X-rays, the physician or X-ray technician may get cancer of the skin in one to eleven years. After an acute burn, cancer may develop at the spot in from one month to two years. Evidently there are long paths and short cuts to cancer. The concatenations of circumstances are almost impossible to unravel.

But perhaps the feature of cancer most difficult of analysis is susceptibility. The degree of susceptibility to an infectious disease such as infantile paralysis usually depends on the concentration of specific material antagonistic to the virus in the blood stream. In cancer many types of cells are susceptible and *appear* to have individually different degrees of susceptibility. We say "appear" advisedly, because one encounters at every turn in the cancer problem wheels within wheels, and we cannot tell how much the frequency and speed of cancer development depend on the cells that undergo the transformation and on the activities of other cells and body fluids. The body is a glorious mixture, never the same at different intervals of time, but always changing. The fact that among cells of one and the same type cancer development is subject to variation immensely complicates the problem, but it does hold out a tantalizing element of hope.

Consider first the matter of inheritance. Some families show a cancer-frequency distinctly greater than that observed in the rest of the population. Failing evidence in these families of unusually great exposure to carcinogens, the assumption is that some of their cells are more susceptible to cancer than are cells of the same sorts in families in which there is but little cancer. This is a kind of organ susceptibility. Thus, as many as ten of a family of sixteen children have had retinal cancer, and cancer of the breast and of some other organs have a tendency to run in families. In identical twins cancers usually appear simultaneously and symmetrically, and are similar in kind of growth. Thus, where the hereditary endowment is the same (identical twins), the cellular susceptibility to cancer development is generally the same, and where it is different, as in different families, the cellular susceptibility is also different.

Even within a single individual during his lifetime, the susceptibility of some cells to agents causing cancer is subject to change, as in *Xeroderma pigmentosum,* a childhood disease in which the skin becomes dry (*Xeros*) and pigmented, and the susceptibility of its epidermal cells to the carcinogenic influence of sunlight increases progressively. If a person has a cancer the chances are greater that he will acquire another cancer somewhere else in his body than are those that a normal person will develop a cancer. In other words, the presence of one cancer seems to facilitate the development of other primary cancers in the same individual. Why we cannot tell, but perhaps the first cancer increases the susceptibility of other cells to carcinogens. The qualification "perhaps" must be retained until evidence is forthcoming of equal exposure to carcinogens in cases of single and multiple cancer and of equal susceptibility to begin with.

In lesions styled "precancerous" a somewhat better case can be presented for increase in susceptibility. White-looking surface lesions in the mouth and on the tongue, existent for years, may flare up as cancers. Harmless-looking

moles of long standing may suddenly change, blacken, and give rise to virulent cancers. In deeper parts of the body other precancerous lesions may persist for years without symptoms and suddenly become malignant. But not all of the white plaques, moles, or other so-called precancerous lesions do this, only a minority of each kind. Again we do not know why. The point is that the frequency of the malignant transformation is greater in their cells than in other cells of the same types. This suggests, but does not prove, an increased susceptibility among some of the cells of precancerous lesions.

Since instances of acquired hypersensitiveness (or allergy) are found to be caused by a host of different substances, it seems not unlikely that some of the motley crew of carcinogens, and also some agents which are not cancer-producing, can alter cells in the direction of increased susceptibility. In the long years up to the age when most cancers are noticed they have abundant opportunities to work. It is conceivable that we are concerned also with a decrease in susceptibility to carcinogens. In mice of certain strains the susceptibility of epidermal cells to carcinogen is less in old than in young animals.

It is unnecessary to sketch further the broad scope and intricacy of the cancer problem—the many kinds and ages of animals involved, the many cell types, the host of different cancer-provoking agents, the great variation in time required to produce cancer, and the cellular susceptibility subject to change. No wonder that cancer research is so diversified and that some investigators are of the opinion that before we can get really started more must be learned about normal cellular physiology and growth. Indeed, almost any line of investigation dealing even remotely with cells is helpful, for cancer is unquestionably a cellular problem. But it is desirable to arrange the established facts in such a way that they are a little less overwhelming and so that research can be aimed more accurately at a solution.

A MEASURE of orientation is given by comparing the events leading to muscular contraction with those which result in cancer production. Muscular contraction and cancer production are of course very different phenomena. We are concerned only with the series of steps leading up to them, not at all with the nature of the steps.

First, a few words about muscular contraction. This can result from stimuli of many kinds: light, sound, heat, cold, taste, smell, touch, pressure, and so on. The stimuli activate receptor cells particularly susceptible to them. These receptor cells generate nervous impulses which are transmitted in their long processes, or in nerve cells strung out in series, by many sensory pathways into the central nervous system, where they are correlated and adjusted. From the central nervous system the impulses are carried by motor nerve fibers to a muscle. These motor fibers have been aptly called the *final common path* by the great physiologist Sherrington. They are the *final* nervous elements in the sequence, that is, those which reach directly to the muscle. They constitute a *common path* into which the impulses, initiated by many sorts of receptors, traveling to the central nervous system by diverse sensory nerves, converge and funnel into the muscle. For example, a muscle in the hand on the steering wheel of a motor car can contract in response to something seen, or heard, or felt in the motion of the wheel, or in consequence of some past sensation or experience stored in memory. Yet the impulses, whatever their source, reach the muscle through the same motor nerve fibers acting as their final common path.

In the comparison it is convenient to

consider activating agents, sequence of events, modifiability, and final common path:

(1) *Activating agents.*

In muscular contraction the stimuli are of wide variety. When carefully considered it is found that they are so different that it is not possible to name a property which all of them possess in common, other than the capacity to produce a change. If they act with sufficient strength they initiate a sequence of events, in the actual performance of which they do not participate.

In cancer production the carcinogens are also of many sorts, in which it is similarly not possible to discover a common property. If they act in sufficient strength, or repeatedly over a long enough time, they start chemical changes, in the continuation of which they apparently do not participate.

(2) *Sequence of events.*

Those leading to muscular contraction may be long or short, depending upon the number and the character of the cells in series. Between each cell resistance is greater than in its substance. There is a kind of build-up of force to overcome this resistance, and time is lost.

The events culminating in cancer production are, by comparison, as yet largely hypothetical. But it seems logical to assume the existence of such steps in series differing *inter se* both in number and character, and also to imagine a kind of build-up to overcome inertia between them.

Obviously, to unravel the sequences leading into the final common path of cancer production is much more difficult than to identify those reaching into the final common path of muscular contraction. The structural elements in the neural sequences are definitely arranged in space and stay put, so that they can be seen microscopically and their activities measured quantitatively. The events in carcinogenesis are centered in the same area of tissue (with unknown spread elsewhere), and later events obliterate those going before. Moreover, as already related, cancer can develop in many different sites, the differential characteristics of which may be not without influence on the course of events. These differential characteristics are as numerous as are the kinds of tissues. One immediately thinks of high vascularity, low vascularity, and avascularity, presence or absence of lymphatic drainage, and degree of cellularity. Moreover, the steps up to cancer production take in the tissue fluids, whereas those reaching into the neural final common path are for the most part insulated from them by impermeable myelin sheaths and dense cellular and fibrous investments.

Because of the partial insulation of neural components, events once started are perhaps more likely effectively to reach into the final common path and bring about muscular contraction than the events in carcinogenesis are to cause cancer. In other words, there are fewer opportunities for them to fade out than in cancer production, the operating time for which is also so much longer, and in which so many carcinogens so seldom initiate sequences reaching the goal.

(3) *Modifiability.*

A short cut is possible by elimination of some of the events that ordinarily lead to muscular contraction, as when contraction results from direct electrical stimulation of muscle. Earle has been able to bring about the malignant change simply by the addition of carcinogen to susceptible cells.

The series of events resulting in muscular contraction can be reinforced by the operation of additional activating agents, or stimuli, and by voluntary effort. So, also, the steps eventuating

in cancer production can be made more effective and the time shortened by additional carcinogens stepping into the sequence initiated by the primarily acting ones.

In states of hyperexcitability of the receptors of nervous elements, and perhaps of muscle, ordinarily inadequate stimuli can produce contraction. In conditions of decreased excitability and of block somewhere in the sequence, even strong stimuli can fail to result in contraction. By conscious inhibition the hand can be held against a hot iron, the impulse to remove it being overruled.

Susceptible cells live in a world of potential carcinogens. These ordinarily fail to produce cancer except when the force exercised is sufficient. Some instances of hypersusceptibility on the part of such susceptible cells are known. Cancer production can be speeded up, as when estradiol benzoate is administered to susceptible animals in addition to the carcinogen. And it can be slowed down. Decrease in susceptibility and complete block of cancer production remain more of a mystery than like phenomena in muscular contraction.

(4) *Final common path.*

Muscular contraction at the ends of motor nerves is the same in muscles of a given sort, irrespective of the kind of initial stimulus. Among vertebrates the nerve fibers in the final common paths deliver the same kick to muscle by liberating somehow a definite substance, acetyl choline. The quality of the contraction depends on the type of muscle, whether smooth, skeletal, or cardiac, and perhaps on individual differences within types.

Cancer production in cells of a given type is similar, irrespective of the kind of carcinogen acting on them. Thus, a squamous cell cancer can result from carcinogens as different as solar radiation, radium, and many chemical substances. Moreover, individual cancers of a definite type show striking degrees of resemblance, even though present in animals of different species—indeed, of different classes—of vertebrates. The assumption is not farfetched that the final kick delivered to susceptible cells is one and the same in all cases of cancer production. The quality of the resulting cancer is primarily conditioned by the type of cell thus influenced. Since, as previously noted, a vast array of different types of cells possess malignant potentiality, the different sorts of cancer are legion.

This view of events not only illustrates the difficulties ahead but also indicates where the problem of cancer can be attacked with some hope of success.

Preventive measures of avoidance cannot be expected ever to banish cancer, as is being done in many districts in the case of malaria, for example. It will be helpful to keep up the search for hitherto unrecognized carcinogens and to try to limit exposure to those already known to the point where they are harmless as cancer-producers. To avoid them all, or to reduce them to impotence as cancer-producers, and still to go on living would be about as feasible as to close the doors of our bodies to sensory stimuli from without and from within and to continue alive. Yet, when there is reason to suspect the existence of unusually great susceptibility to cancer, efforts at avoidance of the activating agents must be redoubled. And it is not beyond the realm of possibility that means will eventually be discovered to decrease this susceptibility.

To block the sequences of events initiated by these activating agents before they funnel into the final common path is of course a possibility. The difficulty is that in man we so often do not know that anything untoward is taking place until the cancer appears. The so-called precancerous lesions may or may not be

representative of events leading to cancer. We need to learn more about them. The warning signal of pain, so helpful in promoting survival in other conditions, is usually withheld until some time after the malignant change takes place.

If we could discover what happens at the end of our hypothetical final common path we might be able to prevent the cancerous transformation. The way to get at it seems to be to unearth the fundamental difference—or differences—that exists between normal and cancerous cells of the same type on which the difference in behavior depends. Armed with knowledge of the nature of the difference, we would be better able to ascertain how the change is brought about, or, coming back to our comparison, what is the kick delivered to susceptible cells at the end of the final common path. If it is some kind of chemical substance, or perhaps even a virus, it is conceivable that it might be possible in some way to inactivate it, or to alter the cell membranes so as to protect the cells against it. But people are dying of cancer every day. Therefore, while striving for data on this fundamental difference, in order to make the proper start, unremitting efforts should continue, even blindly, trying to save the cells from the unknown influence. Curare, which prevents muscular contraction by acting where the fibers of the neural final common path impinge on muscle, was a chance discovery by South American Indians.

Coming now to the cells which have in some way become cancerous, obviously it is important to devise some test of blood or urine which will reveal the presence of cancer wherever located in the body. If cancer cells give off some substance not produced in their absence, this might be demonstrated by some test. But the test would have to be extraordinarily delicate, because beginning cancers consist of only a few cells, incapable of releasing much of anything. There is also the possibility that something detectable is released by noncancerous cells, in consequence of the presence of cancerous ones, which likewise is foreign to a body free from cancer. Again the test would have to be very sensitive. If, on the other hand, such specific substances are not formed and we have to do only with the production of more or less of substances present also in the absence of cancer, the chances are obviously not so favorable. Cancers of sufficient size to produce a noticeable increase or decrease might be as readily detected by present methods.

Lastly, and of supreme importance, are efforts to deal with cancer cells which have been identified. The malignant property appears to be permanently engraved on them. If it is in the nature of a mutation, the likelihood is remote of making these cancer cells revert to the status of their forebears, who were useful members of the cellullar community. Yet this must be attempted. Merely to deprive the cells of their malignancy and viciousness with the idea of withdrawing their teeth, so to speak, may turn out to be more feasible, and must be attempted. However, the main line of attack in cancer evidently is to discover the specific vulnerability of cancer cells so that they can be killed off without at the same time destroying normal cells.

There can be no question but that the approach to the central objective is to find out how the manner of life of cancer cells differs from that of normal cells. The National Cancer Institute and the American Cancer Society, the two organizations from which we naturally expect most, are digging into this problem of cellular physiology. Yet the need for relief from the cancer plague is so urgent that, while diligently providing this groundwork, a frontal attack on the cancer cells, armed only with

present knowledge, should not be postponed. This direct offensive should be on cancer cells of a single type in animals on which experimentation is practically unlimited, and it should be supplemented by cautious follow-up experiments on the same sort of cancer cells in humans.

Organization should be on a large scale so as to bring to bear on the malignant cells all the influences which affect cellular activity and do not kill the animals. But this should not be done altogether blindly. The selection of influences to be tested should be based on the carefully considered advice of expert physiologists, endocrinologists, pharmacologists, biochemists, physicists, radiologists, microbiologists introducing new "wonder drugs," and physicians. Particularly should many products hitherto on the secret list in connection with the war be tried. The experiments should not be limited to possible means of altering the conditions of cell life by the injection of substances, by alterations in diet, by subjection to physical agents, but should include surgical intervention by removal of organs or parts of organs which might be expected profoundly to modify these conditions. The spirit of inquiry should be that of trying everything, not omitting even the unlikely influences, such as animated Kettering in the search for a substance that would take the knock out of gasoline, and Edison in the invention of a filament that gave electric light to the world. The driving force of *must,* which has achieved the seemingly impossible in war invention and industry, should prevail. Whatever organization meets this challenge should be ready to work day and night in several shifts to stem the appalling death rate from cancer.

This goal is attainable. In practice, remarkable and significant variations are observed in the behavior of cancers. Some primary cancers produce whole flocks of secondary growths in other parts of the body, others do not. A large primary tumor may produce small secondary growths, whereas a small primary tumor may give rise to huge secondary tumors. Some grow at an amazing speed and others slowly. A single cancer may enlarge rapidly, then enter into a period of slow extension, almost of latency. Another may grow slowly, and then speed up. In extremely rare cases an active cancer may completely disappear, as in the well-authenticated case of Sister Gertrude. Wholly unknown forces are sometimes at work in the body which accelerate, retard, and even abort cancers. Perhaps it was a realization of the operation of such influences which led Ehrlich to attack the cancer problem despite the warning "All ye who enter here leave hope behind."

HUMANISM—A RELIGION FOR SCIENTISTS

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OPPOSITION of science and religion seems sensible to some and absurd to others; which view one holds depends in part upon what one means by religion. If religion is defined as consisting of traditional dogmas and the ecclesiastical systems interested in maintaining them, then the view that science and religion are opposed may well be justified. But if religion be conceived as faith in the worth-whileness of living and active pursuit of what seems worth while, then science and religion are not opposed and science is a very important part of religion.

Narrow religionists and narrow scientists alike agree that each is opposed to the other, but broad religionists and broad scientists recognize that science and religion involve each other. It is said that science deals with facts and religion with values. This is true, but it is not the whole truth. For there are facts about values and vice versa.

As for facts about values, it is a fact that values exist; that they are what they are; and that they are experienceable. Values, as observable data, are open to scientific investigation, and it is a sad commentary on science that such investigation has been so long neglected by so many of those who have faith in science.

As for values about facts, if facts were of no value, scientists would not be interested in them, and if they were not useful, no one would ever become a scientist. Even those "pure" scientists who seek "facts for their own sake" seek values, because for their own sake means that the values are self-contained. Thus science is pursuit of values as well as of facts.

If religion is the comprehensive name for pursuit of values, then science, insofar as it is pursuit of values, is a part of religion. This conclusion is so unsatisfactory to those scientists who are in the habit of thinking of religion in terms of traditional dogmas that some explanation of the "true" nature of religion seems desirable, that is, of that view of the nature of religion which one would reach if he investigated the matter scientifically. In other words, the investigator should be open-minded, tolerant, willing to be guided by experience and reason, and without prejudgment as to what the conclusion will be. Some scientists who maintain such an attitude toward problems within their field fail to do so in other fields, and the difficulty they have in maintaining a scientific attitude in the field of religion, or with respect to values, is so great that they have come to insist that science cannot deal with values and, by implication, with facts about values. But other scientists have faith in the applicability of scientific attitudes to facts about values and have sought to make a scientific approach to religion.

The scientific investigator will make extended and careful observation of data, classify data into types based on similarities and differences, formulate hypotheses about the nature of the data, and verify hypotheses by applying them to additional data. In seeking to understand religion the scientist observes extensively whatever is called religion by different people at different times; he examines hypotheses which others have held as well as those suggested by his own surveys; and he tests all these hypotheses to discover their relative merits in inter-

preting the data. Hypotheses which fail to fit all of the facts remain unacceptable, except that, in the absence of any completely satisfactory theory, that hypothesis which is most adequate is held tentatively as the most satisfactory. In formulating a scientific definition of religion, scientists would seek all those characteristics that together are both necessary and sufficient for the existence of what people call religion.

One such tentative hypothesis is presented here. This hypothesis, briefly stated, is that religion involves five essential characteristics: values, beliefs, desires, faith, and action. A word about each is needed.

Religion involves values (and disvalues) in two senses: felt values and valued objects. Felt values, or feelings and emotions, are of many sorts: love and hate, hope and despair, awe and contempt, security and danger, peace and turmoil, pride and mortification, satisfaction and frustration. "Objectified values" are of many sorts. Since feelings of value and disvalue are naturally "projected into objects," we seem to see what we feel as apart from us. We not only have feelings of love, but we seem to love others because they are lovable; we not only have feelings of hate, but we seem to hate others because they are hateful. We objectify our feelings of good and evil as due to gods and devils.

Religion involves beliefs, or theories believed, of many sorts. These may be summarized as theories about the nature of the universe, about the nature of man, and about the nature of values. The sum total of one's beliefs constitutes his philosophy of life.

Beliefs about the nature of the universe normally include theories of the origin of the universe, its structure or composition, the kinds of changes, processes, and developments taking place in it, and its end, or future prospects. Since men are profoundly interested in values, they are concerned especially about the fate of values and about whether or not the universe is friendly to values. Theories about the nature of the universe are sometimes called "cosmologies," and often include "theologies." Especially today, they rest upon the results of the various sciences. Part of the reason for the alleged antagonism between science and religion is that many religionists insist upon the truth of centuries-old and now-obsolete science. Religion depends on science; or, science is a part of religion. Cultural lag in our ecclesiastical systems has caused conflicts. These conflicts are not essentially between science and religion, but between religion based on modern science and religion based on ancient science.

Beliefs about the nature of man normally include theories identical with those about the nature of the universe. What is man? Is he an eternal or a temporary being? If both men and the world change, can men hope for conservation of their values in the future? Is human nature such that values can be finally and completely achieved, or such that perfection can be hoped for but never achieved? Is human nature inherently good, or evil? Or both? Or neither?

Beliefs about the nature of values normally include theories of the origin of good and evil, the kinds of values and disvalues, the achievability, conservability, creatability of values and disvalues, and the fate or future prospects of values and disvalues. Where did good and evil come from? What caused them? Did they always exist? Did they have personal or impersonal causes? Can they still be created anew? Of what do good and evil consist? Are they different kinds of things, or different aspects of the same kinds of things? Are there great and small values, temporary and eternal values? Since values

are related to desires, perhaps there are as many different kinds of values as there are of desires. These have varied from person to person, culture to culture, and time to time. All, however, have included desires for security (of life, food, children, health, home, etc.), desires for esteem, desires for love and friendship, and desires for more interesting activity. Beginning students of religions seem lost in a maze of unrelated value-beliefs. But persistent students discover common patterns of value-beliefs running through all religions.

Can the "good life" be achieved by all men? What must one do to get what is good and to avoid what is evil? What kinds of conduct or behavior are best to achieve goodness? What kinds of ideas or attitudes should one have? What kinds of instruments should one use? What kinds of beings should one seek to serve or seek to destroy? Are values given to us or made by us? Are values to be waited for or sought after?

Can values, once achieved, be preserved? Is the good life transient or eternal? Does a good deed now deserve an eternal reward, or is each good deed its own reward? Is evil temporary or permanent? Will all values cease some day, or will they endure forever? Will our values always be the same, or will they change and evolve into different values? What guarantees are there about the future of values?

Religion involves desire to achieve values and to avoid disvalues. Not just indifferent belief, but actual desire to achieve values is essential to religion. One must want what is good. It is natural to want what is good, and thus, to this extent at least, it is natural to be religious. Desire to avoid evil, not just helpless acceptance of ills, is also essential to religion. One must want to destroy or diminish evil, and insofar as it is natural to want to do this, men are naturally religious. Since religion seeks abatement of what is bad, the religious naturally desire to abide by justified taboos.

Religion involves faith in the truthfulness or usefulness of one's beliefs and in the realizability of one's desires. Having beliefs is not enough; in order to be religious, one must trust his theories in actual practice. "Faith," someone has said, "is knowledge upon which one is willing to risk himself in action." Having desires is not enough; in order to be religious, one must have faith that his desires for the good life can be fulfilled. He must have faith that "the universe is friendly to values," at least in some measure.

Religion involves action. "Faith without works is dead." Religion involves acting upon one's faith in one's beliefs and desires. Religion is a quest, or a quest*ing,* for the good life. Religion is devotion to values, not so much devoutness of attitude as devoting serious effort to active pursuit of values. Religion is more than an experience of values, more than a philosophy (or science) of life, more than a set of sincere desires, and more than a faith that everything will turn out all right in the end. It involves active practice of one's faith in one's philosophy of values.

So much for a tentative definition of the nature of religion. Critics will contend that it is too general. The reply will be—and this is one of the important points of this article—that most definitions of religion are too narrow. Theology is a possible, but not an essential, ingredient in religion; there are atheistic religions. Those who insist upon theological ingredients do so partly because there is a preponderance among them of those who prefer to base their beliefs upon ancient rather than on modern science. As more investigators without vested interests, financial or educational, in traditional systems study the nature of religion, the less often do theological

items appear among what are considered the essentials of religion.

If this theory of religion is adequate, it will apply to everything that is commonly called religion and to some things that are not. Supernaturalism, naturalism, occultism, communism, animism and atheism may all be varieties of religion. Fetishes, family Bibles, music, marriage, politics, prayer, orgiastic dances, statistical analyses, scientific inquiries and heretical inquisitions; virginity mores, fertility practices, artificial insemination, capital punishment, church attendance, bombing raids, building of libraries and burning of books—all are possible parts of religions. But they are not all—or, rather, not at all—essential to religion. Each scientist can review the foregoing summary of essentials to see whether or not he himself participates in all of the essentials listed. If he does, and if this theory of religion is adequate, then he may properly be called religious, regardless of whether he appreciates or depreciates local religious traditions.

If science is a part of religion, then which religion seems best suited to the interests of modern scientists? Many religious traditions have become modified and have adapted themselves to newer scientific discoveries, and many will no doubt continue to do so. But none is better suited to scientists than a movement called "Religious Humanism" or, simply, "Humanism." Humanism is not the only religion for scientists, for all religions involve some science, and scientists are as much entitled as others to pursue their preferences in matters of values. But Humanism is a religion erected upon the results of modern science, and thus tends to be the religion best suited to the interests of scientists. Humanists do not necessarily shun all traditional ideals, but neither do they find traditional beliefs at all essential to their ways of thinking. What, then, is Humanism?

Humanism is science plus values. Or, if you prefer, values plus science. Humanism is a religion of those who tend—and intend—to accept the conclusions of scientific investigation as their "beliefs" and who have "faith" that their "desires" for a better life will be justified by their "acting" upon their faith in such beliefs. Whoever does this is already a Humanist, whether he knows it or not. Such an answer seems so simple that many will want to inquire about further details. Since the question of the existence and nature of God has loomed so large in many recent religious views, many scientists will want to ask: What about God? The Humanist will reply: What does the scientist say about God? Ancient scientists assumed his existence as a major premise. But centuries of critical analysis have led scientists to less and less certainty about his existence and nature. Modern scientists not only do not place assumptions about the existence and nature of God among their major premises, many of them do not consciously involve him, or it, among their assumptions at all. Humanism is affected by this increasing uncertainty; in fact, Humanism is a product of it. Humanism is a religion in which belief in the existence of God is not essential.

Does this mean that Humanism is atheistic? No. If scientists could prove that there is no God, then, no doubt, atheism would become an essential of Humanism. So long as this seems unlikely, Humanism is a religion in which belief in the nonexistence of God is not necessary. Some Humanists are theists and some are atheists, but, insofar as they are Humanists, they hold some beliefs in common that are more important to them than either their theistic or atheistic beliefs.

Humanism is characterized by an em-

phasis upon the ideal of the brotherhood of man and by a neglect of the ideal of the fatherhood of God. Some Humanists are, and some are not, interested in the doctrine of the fatherhood of God, but all are agreed that the ideal of the brotherhood of man is more important than the ideal of the fatherhood of God, and they are committed to faith in the practical necessity and desirability of the ideal of human brotherhood.

While some Humanists may feel that the ideal of man's brotherhood is fostered and somewhat sustained by the ideal of fatherly origin, most will agree that the brotherly ideal does not necessarily presuppose the fatherly ideal. Those Humanists who hold opposite beliefs with regard to theism naturally entertain some doubts about the wisdom of their opponents, but these doubts remain subordinate to their agreement about the superior importance of the ideal of brotherhood. Atheists who consider denial of the existence of God as more important than affirmation of the value of the ideal of brotherhood are very questionable Humanists, to say the least. Atheism is not only not essential to Humanism, but the more an atheist emphasizes his atheism, as compared to his faith in and practice of fraternalism, the less of a Humanist he is. Likewise, the more a theist insists upon his theism, the less of a Humanist he is. Thus, being a Humanist is a matter of degree.

The basis of the ideal of brotherhood, for Humanists, comes not from the Bible but from biology. Humanists accept the conclusions of biological scientists about the origin and development of man. The biological doctrine of struggle for existence and survival of the fit is buttressed by contributions from all the natural and social scientists, especially by those from social psychologists, political scientists, and historians. The story of human struggle for survival is not a pleasant one, and man's chances for survival seem to be better in a cooperative than in a competitive society. History's lesson, "United we stand, divided we fall," is hard to learn. Humanists have learned it. The belief that men are brothers, to be tolerated, if not loved, has become a practical necessity. The belief has a biological basis, but it is born and reborn as a consequence of the bitter experiences of unbrotherly conduct. Humanists are not particularly sentimental about brotherhood, though they are not unwilling to use sentiments about brotherhood in the interest of human survival and improvement.

Absence of traditional emphasis upon God as responsible for man results in emphasis upon man as responsible for self-control. The fate of humanity rests primarily in human hands, if in any hands at all. Faith in the possibility of some survival and improvement of human values is accompanied by faith in the willingness of human beings to accept full responsibility for actively promoting such survival and improvement. Acceptance of complete determinism does not prevent faith in the effectiveness of such freedom as is involved in self-determinism. Humanists hope that mankind will become increasingly self-determined through becoming increasingly self-determining. The pursuit of values, which constitutes religion, is facilitated by faith that willingness to assume responsibility for self-improvement will increase self-improvement. Humanists are interested in instilling in individuals desires for self-improvement and faith that science provides the best ways toward such self-improvement. Thus Humanism is a scientific religion—a religion for scientists.

Some religions are evangelistic. How about Humanism? Humanists differ. Among those who are not evangelistic, an attitude prevails that increasing Humanization, or "secularization," is taking place, and will continue to take

place, spontaneously, without any special, organized effort on the part of Humanists. Traditional religious systems are declining, judging by membership and financial statistics, and it is better to let them die a slow death than to oppose them openly, thus providing them with the kind of revitalization that thrives on opposition. Humanism progresses better, these Humanists hold, if industrial, governmental, educational, and social advancements are promoted without any suggestion that they are religious in nature. It is best served by "letting nature take its course."

Other Humanists are definitely evangelistic. One group, which organized what is now the American Humanist Association, issued in 1933 "A Humanist Manifesto," setting forth fifteen points of doctrine and calling for concerted action. This Association publishes the *Humanist*, edited by Edwin H. Wilson, at 1201 Union St., Schenectady, N. Y., and sponsors annually a "Conference on the Scientific Spirit and the Democratic Faith" in New York City. Another group, the Humanist Society of Friends, publishes the *Humanist Friend*, edited by Lowell H. Coate, at 2405 West 23rd St., Los Angeles, Cal., and includes more than forty ministers in its official list. A *Humanist Monographs* series is published by Hugh Robert Orr, director of the Humanist Society of San Francisco. Books and pamphlets are published also by the Canadian Humanist Group, with Blodwen Davies, Thornhill, Ontario, as secretary. All these groups advocate vigorous promotion of Humanism as an organized religion.

Between those who prefer to let nature take its course and those who seek to organize Humanism as an evangelical religion are many who are evangelical about promoting Humanistic values but quite indifferent to Humanism as an organized religion. Each scientist who, through research or teaching, wants, in addition to drawing his pay, to promote human welfare may be a Humanist of this kind. Each scientist who believes that his specialty contributes to human progress rather than service to God is a Humanist. And to the extent that he seeks to help others to have faith that the beliefs involved in his specialty will help them, he is evangelistic with respect to human values. Organizations of scientists for purposes of improving science, and thereby improving humanity, are Humanistic organizations. The American Association for the Advancement of Science is an excellent Humanistic organization, if one is to judge by its name, by editorials appearing in its *Bulletin*, by many of the articles appearing in THE SCIENTIFIC MONTHLY, and by published descriptions of its "saints," or honored dead, medalists, and prize winners. Recent *Bulletin* editorials urging scientists to accept more responsibility for guiding postwar reconstruction illustrate evangelism for improving human values.

Despite the disdain that many claim toward values as proper subjects for science, the ultimate aim of science is pursuit of values. If religion is the general name for pursuit of values, then scientists who pursue values are religious. If Humanism is the religion most completely based on science—contemporary science, that is—then Humanism is the most scientific religion and the best religion for scientists. And, if so, most scientists today, whether aware of it or not, are Humanists.

LINES FOR A VERY YOUNG SCHOLAR

By Clarence R. Wylie, Jr.
DEPARTMENT OF MATHEMATICS, THE OHIO STATE UNIVERSITY

There is but one who may intrude unbid
When in my study I at last am rid
Of the world's clamor and the day's affairs.
Sometimes I hear him as he mounts the stairs
Or tiptoes to my door on stealthy feet.
But sometimes, when his triumph is complete,
He moves so softly that he gains my side
Without my knowing it, and stands with pride,
Still as a nesting bird, until my eyes,
Wandering a moment from my book, surprise
His own on me. And then without a smile
We face each other for a little while,
Revealing in our comic gravity
A mutual amazement, I to see
My boisterous son, all roughness put away,
And he to find the comrade of his play
So strangely occupied with quiet things,
Till with a burst of laughter up he springs
To his unchallenged throne upon my knee.

Dear little boy, how easy it would be
To lay the burden of my dreams on you,
And try to make you be, and think, and do
The things I cherish as life's highest aim.
But love has saved me from the patent shame
Of such conceit; I shall be guardian, friend,
All that a father should, but I shall send
A free man, not a slave, against the years
When you go forth. And where the road appears
Fairest to you, there with my blessing, go.
I shall be well content to have it so,
Knowing that you have gathered from this hour,
Often repeated, an imponderable dower,
More precious than any tangible resource,
A love of books, faith in the quiet force
Of clear, unbiased thinking, and the zest
Of kinship with all men who manifest
A love of truth, and make her cause their task.
These are a warrant for the thing I ask:
To see that you escape the deep distress
Of lasting failure or too cheap success.

A NEGRO SCIENTIST OF SLAVERY DAYS[1]

By GEORGE P. MEADE

COLONIAL SUGARS COMPANY, GRAMERCY, LA.

THE little-known story of Norbert Rillieux, Negro engineer, inventor, and scientist of ante-bellum days is scattered through textbooks on evaporation, sugar journals, and sugar reference books.

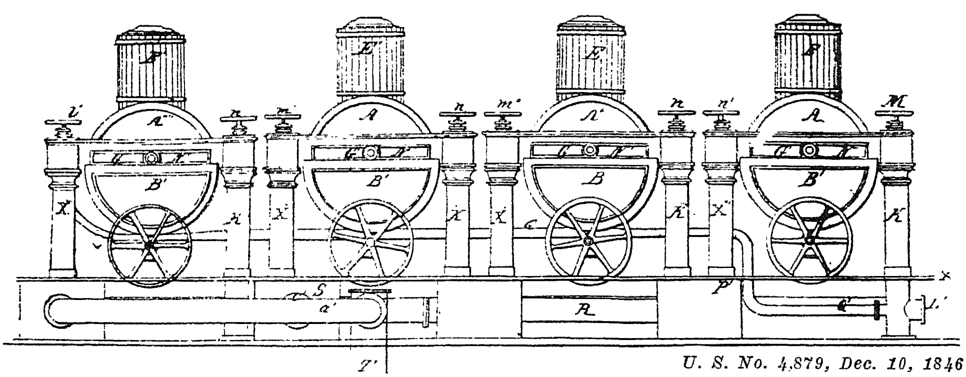
It is just 100 years since the first practical multiple-effect vacuum evaporator, known then as the Rillieux apparatus, was put into use in a sugar factory near New Orleans, and the centenary seems an appropriate time to bring together the facts concerning the inventor. Evaporation in multiple effect is now universally used throughout the sugar industry as well as in the manufacture of condensed milk, soap, gelatine, and glue, in the recovery of waste liquors in distilleries and paper factories, and in many other processes. The underlying principle on which these evaporators operate has not altered materially since Rillieux first designed his system, and a clear and simple description which applies to all such evaporators in use today is given in one of Rillieux's patents (U.S. 4,879, 1846):

> A series of vacuum pans, or partial vacuum pans, have been so combined together as to make use of the vapor of the evaporation of the juice in the first, to heat the juice in the second and the vapor from this to heat the juice in the third, which latter is in connection with a condenser, the degree of pressure in each successive one being less. . . . The number of sirup-pans may be increased or decreased at pleasure so long as the last of the series is in conjunction with the condenser.

The drawings accompanying the patent show that Rillieux's apparatus consisted of a series of horizontal pans, or bodies, with steam coils, also horizontal, each body having the general form of a locomotive boiler (Fig. 1).

The great scientific contribution which Rillieux made was in his recognition of the steam economies which can be effected by repeated use of the latent heat in the steam and vapors. The vapors resulting from the evaporation in the first body of the evaporator contain practically all the latent heat of the

[1] Special thanks are due Dr. Charles A. Browne, of the U. S. Department of Agriculture, for source material, illustrations, and suggestions in the preparation of this article. Mr. Alfred L. Webre, of the U. S. Pipe and Foundry Company, Burlington, N. J., also supplied valuable source material.



U. S. No. 4,879, Dec. 10, 1846

FIG. 1. RILLIEUX'S SECOND PATENT

FOR EVAPORATING SUGAR SOLUTIONS IN VACUO. AN ELEVATION OF A SERIES OF EVAPORATING PANS.

original steam, and these vapors are carried over to the steam chest of the second body where they give up their latent heat to the liquid in this body, which boils at a lower temperature because of the reduced pressure. The process is then repeated from the second to the third body and so on until the vapors from the last body go to the condenser.

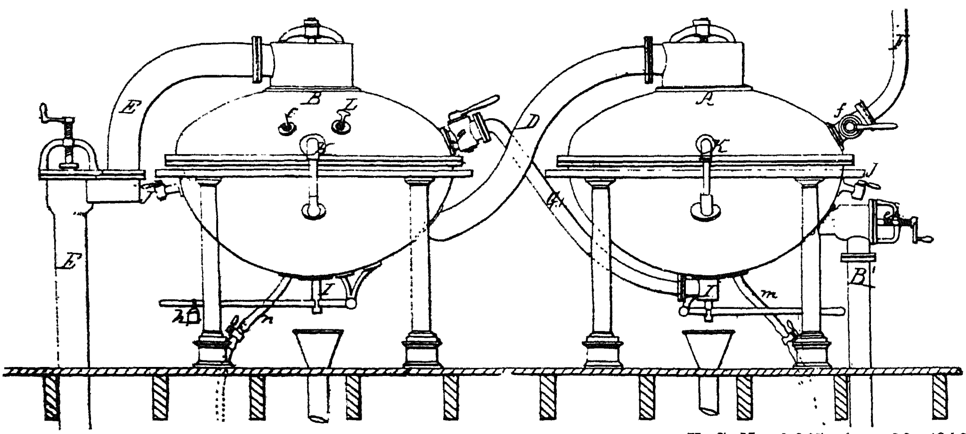
Theoretically, the multiple use of the latent heat may continue for any number of times, as is indicated in Rillieux's patent, but the practical application is limited by the need for sufficient temperature difference between the vapor and the liquid to be boiled. Quadruple effects are commonly used in the sugar industry, but higher combinations have been designed for special purposes. The multiple use of the latent heat results in economies proportional to the number of times that the transfer of latent heat is repeated, and the fuel savings which Rillieux's application of the principle have effected through the years are enormous.

THE birth record on file in the City Hall of New Orleans reads as follows: "Norbert Rillieux, quadroon libre, natural son of Vincent Rillieux and Constance Vivant, Born March 17, 1806. Baptized in St. Louis Cathedral by Pere Antoine."

It is not known whether the child was specifically freed or whether his mother was already free, but the latter is the more probable. The term quadroon was commonly used to mean any person who was more than half white, and the indications are that Norbert Rillieux was only one-eighth colored blood. The fact that the baptism took place in the cathedral and that the father's surname was given him and not the mother's may have been usual for such alliances at that time.

The father, Vincent Rillieux, was himself an engineer and inventor. A steam-operated cotton-baling press which was installed in a cotton warehouse on Poydras Street was one of his inventions of sufficient merit to be mentioned in the notes published at the time of his more distinguished son's death. The father recognized the boy's ability at an early date and sent Norbert to Paris to be educated. This was not unusual as many well-to-do Louisiana quadroons of that day were educated in France.

Most of the details of Rillieux's stu-



U. S. No. 3,237, Aug. 26, 1843

FIG. 2. RILLIEUX'S FIRST PATENT

FOR EVAPORATING SUGAR SOLUTIONS: TWO HOWARD VACUUM PANS CONNECTED IN MULTIPLE EFFECT.

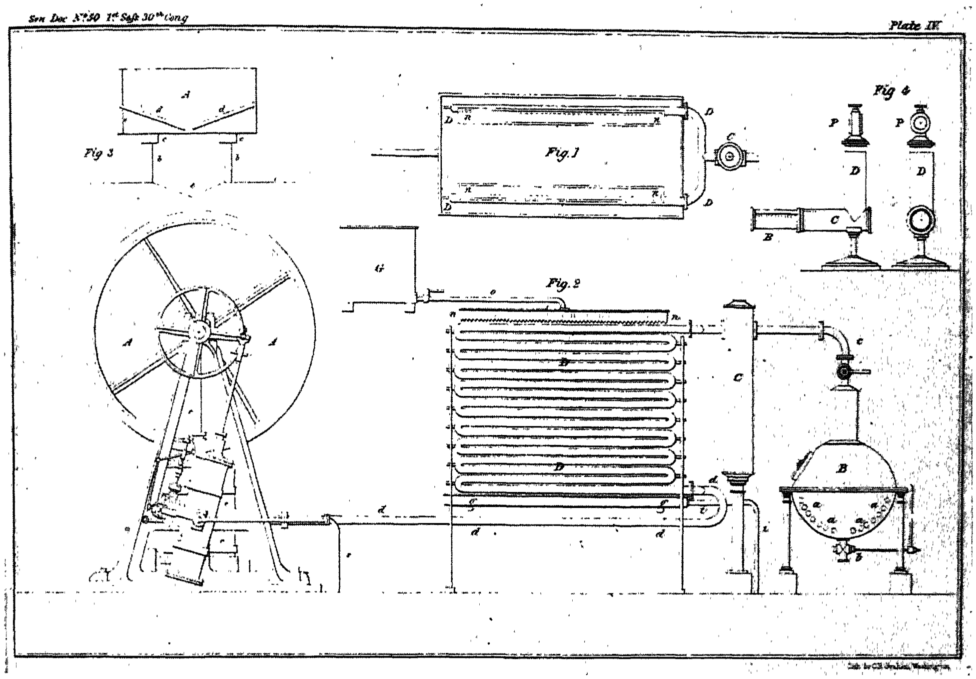


FIG. 3. CROSS SECTION OF A DEGRAND EVAPORATOR[2]
THE EVAPORATOR D STANDS BETWEEN VACUUM PAN B AND VACUUM PUMP A. SUGAR-CANE JUICE FROM G, TRICKLING OVER THE COILS OF D, CONDENSES THE VAPORS FROM B AND IS EVAPORATED TO A SIRUP WHICH IS THEN FURTHER CONCENTRATED IN B TO THE POINT OF CRYSTALLIZATION.

dent life in Paris come from the noted French sugar technologist and engineer Horsin-Deon, who was associated with the Louisianian in the latter part of the nineteenth century. Evidently the young Rillieux showed rare aptitude for engineering since at the age of twenty-four he was an instructor in applied mechanics at L'École Centrale in Paris. In 1830 he published a series of papers on steam engine work and steam economy, and, according to Horsin-Deon, it was at this time that he developed the theory of the multiple-effect evaporator. Unfortunately, none of these publications is now available but, according to French sources, they were of very high order.

[2] Plate IV of R. S. McCulloh's *Scientific Investigations in Relation to Sugar* and *Hydrometers,* Washington, 1848. Senate Document No. 50, 30th Congress, 1st Session.

The interest in multiple-effect evaporation was an outgrowth of Howard's invention in 1812 of the vacuum pan (Fig. 2) for boiling sugar solutions to grain. The advantage of vacuum evaporation for such a heat-sensitive material as sugar was so obvious that the "Howard saccharine evaporator," as it was called, was promptly adopted by the sugar refiners in England for crystallizing their sugar liquors. This was a "single-effect" and it is the prototype of all modern vacuum pans that crystallize the world's sugar. Many inventors turned their attention to extending Howard's invention so that it would make use of the latent heat of the vapors. The most promising of these attempts was the evaporator of the French inventor Degrand, who condensed the vapors from the vacuum pan in a series of horizontal coils whose outer

surfaces were cooled by a falling film of clarified cane juice. This evaporator (Fig. 3), as manufactured by Derosne and Cail of France, was installed in Louisiana, Cuba, and other countries to a limited extent, and for a time was a competitor of Rillieux's apparatus. The evaporation of juice thus accomplished, however, was an imperfect utilization of the latent heat and it remained for Rillieux, by a stroke of genius, to enclose the condensing coils in a vacuum chamber and to employ the vapor from this first condensing chamber for evaporating the juice in a second chamber under higher vacuum. It was thus that young Rillieux established for the first time the principles that have "laid the foundation for all modern industrial evaporation." He tried without success to interest French machinery manufacturers in his invention, and nearly twenty years elapsed before a beet sugar factory in France installed a multiple-effect evaporator.

The reputation of the young inventor must have reached Louisiana, as he was called to New Orleans by Edmund Forstall to be chief engineer of a sugar refinery which Forstall was building. The arrangement lasted only a short while, as Rillieux abandoned his position to avoid displeasing his father, who had some disagreement with Forstall.

The first attempt at a practical evaporator was in 1834 on the plantation of Zenon Ramon, where Rillieux and two colleagues constructed a triple effect which is frequently referred to as the first multiple evaporator. Following this doubtful venture Rillieux plunged into speculation on lands and made "an enormous fortune" which was lost through a bank failure in the financial crisis of 1837. Another unsuccessful attempt to operate a triple effect was made in 1841, but the reasons for the failure are again vague.

Finally, in 1843, Theodore Packwood, who owned a plantation below New Orleans now known as "Myrtle Grove," interested himself in the invention and encouraged Rillieux to install a triple effect made according to Rillieux's designs by the manufacturing firm of Merrick & Towne of Philadelphia. This operated with complete success in 1845, and it is agreed by all authorities that this was the first factory-scale multiple-effect vacuum evaporator. Although Rillieux's first patent of 1843 (No. 3,237) consists simply of two Howard vacuum pans connected so as to work in double effect (Fig. 2), the Packwood installation is always referred to as a triple effect. The evidence is that this conformed closely to the design (Fig. 1) given in Rillieux's second patent of 1846 (No. 4,879). A study of the text of these patents shows that the inventor had a thorough grasp of both the theory and practice of multiple-effect evaporation as understood today.

The acclaim was immediate and widespread, and the Rillieux apparatus was promptly recognized as revolutionizing the manufacture and refining of sugar. In 1846 several other factories in Louisiana installed the new evaporating system which made a superior sugar at greatly reduced costs. First and second prizes for the best sugar were awarded that year to Packwood and to Packwood and Benjamin, respectively, both cited as having "Rillieux's patent sugar boiling apparatus." The picturesque but wasteful "Jamaica train," in which the juice was evaporated in a series of open kettles, gave way rapidly not only in Louisiana but in Cuba (Fig. 4) and Mexico. The progressive factory owners of Louisiana were proud of their new equipment, and the financial reports carried the notation "Rillieux system" after the production statistics of the factories so equipped. Thirteen sugar-houses were thus designated in DeBow's crop reports of 1849.

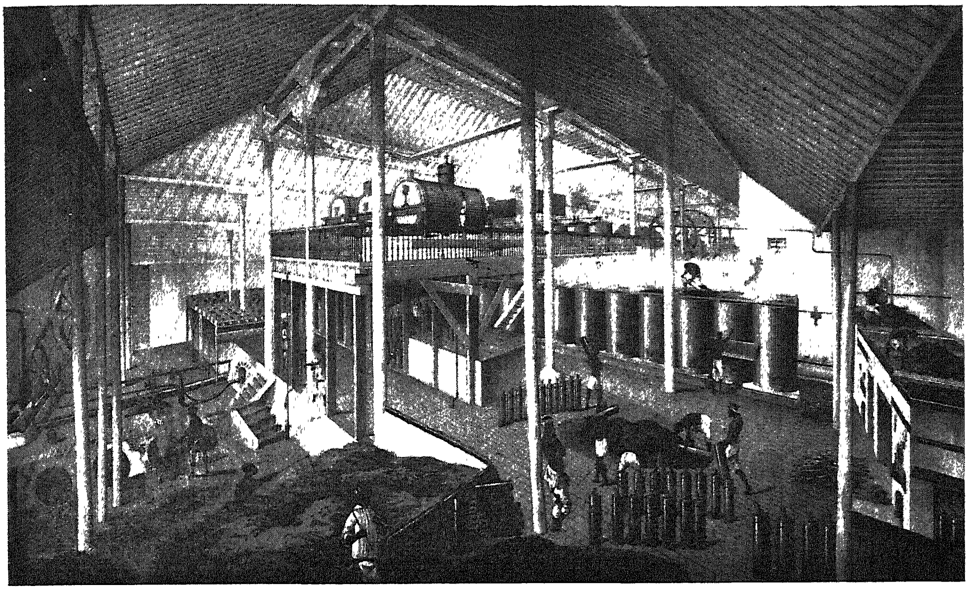


FIG. 4. INTERIOR OF A CUBAN SUGAR FACTORY[3]
INGENIO ASUNCIÓN, ABOUT 1855, SHOWING A RILLIEUX TRIPLE-EFFECT EVAPORATOR IN THE GALLERY.

The system as generally installed at that time included not only the multiple-effect evaporator for concentrating the cane juice to a sirup but also a vapor-heated vacuum pan for boiling the sirup to grain. The vapor for the pan was drawn from the first body of the multiple evaporator, and the boiling to grain was therefore done in multiple effect. This vapor vacuum-pan idea antedated by seventy-five years a similar arrangement of vapor pans in the large modern factory at Clewiston, Fla., which is looked upon as an innovation by present-day sugar factory engineers.

The years from 1845 to 1855 were years of triumph for Rillieux, or at least for his ideas. The new evaporating equipment was in sharp contrast to the older methods in which Negro slaves transferred the boiling juice from one steaming open kettle to the next by means of long ladles. One workman manipulating a few valves operated the completely enclosed Rillieux apparatus. In addition to the savings in labor and steam, the lower boiling temperatures *in vacuo* resulted in greatly reduced losses of sugar in the process. Possibly no such revolutionary change had taken place in any industry up to that time. This was not merely a change from a manual operation to a mechanical one; it was a complete overturn in theory, practice, and method from a process that had changed little through the centuries to one which was fundamentally the same as is used in the boiling-house of all sugar factories today.

FEW details of Rillieux's social status as a free man of color have come down to us. According to one story, he was housed in the slave quarters on some plantations that his work required him to visit, but this appears to be an exaggeration. Direct evidence from a man whose father knew Rillieux and employed him on his plantation as an engi-

[3] Copy of a lithograph by E. Laplante in J. G. Cantero's *Los Ingenios de la Isla de Cuba*, Havana, 1857.

neer indicates that the color problem was met by providing a special house with slave servants for the inventor on his visits as a consultant. According to this source, Rillieux was "the most sought after engineer in Louisiana," but because of his colored blood he could not be entertained at the owner's house or in the home of any white person.

Rillieux's own reminiscences, as transcribed through Horsin-Deon, do not refer to injustices because of his color, but there can be no doubt that he was subjected to restrictions and possibly indignities. Free persons of color were more and more restrained with the approach of the Civil War, although they never reached the status of slaves. Among other prerogatives, they had property rights; they could (and did) own slaves; and they were subject to taxation, even though they were deprived of the use of the New Orleans public school system. But by 1855 a free person of color could not move about the streets of New Orleans without permission, nor might he stop in the city without first presenting the guarantee of some white man. Failure to leave the city when ordered meant years of imprisonment at hard labor.

These restrictions probably contributed toward Rillieux's decision to return to France to live, although another argument for such a move would have been the rapid decline of the sugar industry in Louisiana during the war. The year of his leaving is in doubt. Horsin-Deon says that he left America "after the war, exhausted and asking for nothing but rest," but others refer to his living in Paris in 1861.

His engineering work while in Louisiana was by no means limited to the sugar industry. The drainage of the lowlands of New Orleans was one of the problems which he undertook, and a successful plan was worked out. There are conflicting stories about this enterprise. Horsin-Deon, who quotes Rillieux in much factual detail, says that the deal was blocked in the state legislature by Forstall, "his sworn enemy." It is not clear from this account whether the project was completed, or even attempted. Rousseve, in *The Negro in Louisiana* (New Orleans, 1937), says that the plan was a city sewerage system "which local authorities refused to accept" because "sentiment against free people of color had become sufficiently acute to prohibit the bestowing of such an honor upon a member of this group." The race question may have had some bearing, but this writer's source material seems to have been faulty as he credits Rillieux with inventing a "vacuum cup" and says further that "after a time he must have moved to France, for he is mentioned as head of the École Centrale of Paris."

Strangely enough, his patent troubles all occurred in Europe, where no race discrimination was involved. The United States patents are in the inventor's own name and were fully and profitably exploited by him during his last fifteen years in Louisiana. A fixed price was charged for the apparatus and in addition a percentage of the amount saved in fuel, which resulted in large returns to the inventor. A German who was working for the Philadelphia firm which constructed the first triple effect for Louisiana copied the drawings and took them to a factory in Magdeburg, Germany. From these pilfered designs, the first multiple-effect evaporator in France (and evidently anywhere abroad) was installed in 1852 in a beet sugar factory at Cuincy (Nord). This installation and several others made during subsequent years operated so poorly that the nickname *triste effet* became common among French sugar engineers. Authorities agree that the difficulty lay in a complete misunderstanding of Rillieux's designs, proving that the reason for the

success of the evaporators erected in America by Rillieux himself was his knowledge of the underlying scientific principles.

The first ten or fifteen years of his life after the return to Paris are obscured by lack of detail. He apparently lost all interest in sugar machinery and took up the study of Egyptology, working for at least a decade with the Champollions, who were noted specialists on the subject. According to an editorial in the *Louisiana Planter* at the time of Rillieux's death, Duncan Keener, one of the leading sugar planters of Rillieux's day in Louisiana, on a trip to Paris in 1880 looked up the inventor and was surprised to find him assiduously deciphering hieroglyphics at the Bibliotheque Nationale. This is further evidence of Rillieux's studious mind and scholarly attainments.

When Rillieux was nearing his seventy-fifth year he "returned from the pyramids," as one French commentator put it, and again vigorously concentrated on the problems of evaporation and sugar machinery. In 1881 he patented a system for heating juice with vapors in multiple effect which is now universal practice in cane and beet sugar factories. This innovation which Rillieux made at such an advanced age was credited with reducing fuel consumption in French beet sugarhouses to one-half that before its introduction. In collaboration with Horsin-Deon, Sr., a cane sugarhouse was constructed in Egypt on the Rillieux system, using a quintuple-effect evaporator for juice, a double-effect vapor vacuum pan for boiling sugar to grain, and a triple-effect for juice and other heating. This factory employed the diffusion process, which gives very thin juices, and therefore fuel economies were of great importance. Many sugarhouse engineers of today do not realize that such an extensive use of steam in multiple effect was fully developed over sixty years ago. This combined process of juice heating, vapor boiling to grain, and multiple-effect evaporation seems to be the French process patent which Rillieux lost because "experts were unwilling to recognize his invention." He was then eighty-five years old but of active temperament and in full possession of his faculties. The loss of his process seems to have been too much for the old man, however, and he ceased to devote himself to this work to which his genius had contributed so much. He died in his eighty-ninth year and, according to his unflagging friend Horsin-Deon, the "end came more from a broken heart than from the weight of years." He was buried in a vault in the churchyard Pere La Chaise with the following inscription on his grave:

Ici reposent
Norbert Rillieux
ingénieur civil a la Nouvelle Orleans
18 Mars 1806
décédé a Paris le 8 Obre 1894

Emily Cuckow, Veuve Rillieux
1827–1912

Little is known of Madame Rillieux except the dates on this headstone and that she was in comfortable circumstances during the final years of her long life.

Rillieux's character and appearance have been variously described, principally by French commentators. An excellent photograph taken in his later years in Paris shows him as a rather imposing figure of Caucasian appearance, with a high forehead, luxuriant white hair, and a mustache and full beard trimmed in French style. A vigorous and intense disposition, forceful to the verge of impatience, was evidently his, as the editor of a technical paper in Paris speaks of Rillieux's criticisms of his competitors as "formulated with a vivacity quite in character with

Norbert Rillieux

the irritable inventor.'' Apparently the term *caractère difficile* was frequently applied to him, since the younger Horsin-Deon denies that this is correct but adds that he was a *combatif* throughout his life, excessively frank, saying exactly what he thought, and never countenancing either duplicity or injustice.

THE recognition which Rillieux has received is surprisingly contradictory. On the negative side, there is no reference card bearing his name in either the New York Public Library or the New Orleans Public Library. None of the encyclopedias carries anything about him. The *Dictionary of American Biography,* which includes the names of Western bad men, gamblers, baseball players, football coaches, and relatively obscure inventors, does not mention this man whose apparatus revolutionized the sugar industry.

Most writers who have devoted themselves to the study of Negro life and Negro history afford him very little space. Rousseve (*The Negro in Louisiana*) gives only one paragraph to Norbert Rillieux, most of which is devoted to the story of the sewerage system already mentioned, but at least three pages are devoted to Victor Ernest Rillieux, a poet, whose work is little known. Thomas C. Fuller in the *Pictorial History of the American Negro* (Memphis, 1933) says ''some authorities give Norbert Rillieux credit for inventing an evaporator pan by which the refining of sugar was completely revolutionized.'' Carter Woodson in *The Negro in Our History* (Washington, 1922) gives a full-page drawing of Rillieux's apparatus which is a facsimile of a page from the patent of December 10, 1846, but the only reference in the text is that ''Norbert Rillieux, a man of color in Louisiana, patented an evaporating pan by which the refining of sugar was revolutionized.'' Weatherford does not mention Rillieux in *The Negro from Africa to America,* although this is the most frequently consulted work on Negro progress and achievements, according to the New Orleans Public Library.

The positive recognition is impressive. The only one of his race who seems to have accorded Rillieux full credit is R. L. Desdunes, himself a quadroon, in *Nos Hommes et Notre Histoire* (Montreal, Canada, 1911). After saying that the vacuum evaporating apparatus which he invented revolutionized the sugar industry (a line which all the other writers on Negro history seem to have copied), Desdunes states: ''We have had our heroes, writers, musicians, painters, sculptors, and architects, but Rillieux himself was a scientific genius.''

This appears to be the earliest printed reference to Rillieux's Negro extraction, aside from the birth record, although his racial background was common knowledge while he was living in Louisiana.

At the time of his death in 1894 the *Louisiana Planter and Sugar Manufacturer,* organ of the cane sugar industry for more than half a century, gave generous space to Rillieux's life and accomplishments. The editor and founder was John Dymond, a sugar planter, who operated one of Rillieux's original evaporating systems in his Belair sugarhouse until the factory was destroyed by fire in 1908. This apparatus was unquestionably the last of the early Rillieux equipment to operate in this country. The *Planter* ran a column-and-a-half editorial in the issue of November 3, 1894, and a two-column translation of an article which appeared in *Le Journal des Fabricants de Sucre* of Paris. The issue of November 24 carries the long letter from Horsin-Deon, Sr., in both French and English, from which much of the data here given have been taken. None of these articles makes any reference to Rillieux's Negro blood.

The editorial says: "It is almost startling to see the clearness with which Mr. Rillieux worked out his conclusions. There were so few people who appreciated what he was doing that it became necessary to accept his apparatus, we may say, blindly, its efficient working being accepted by many planters who had no comprehension of its *modus operandi.*" He was referred to as "one of the most distinguished engineers that has ever been identified with the sugar industry," and as "grasping this problem" of multiple-effect evaporation "with his superior intelligence." The editorial says further that "he went into the matter [of the repeated use of latent heat] so long in advance of our modern scientists such as John Tyndall, Lord Kelvin, and others that his success was a far more wonderful thing than can be grasped by those of this generation." These statements are not quotations from friendly French sources but the considered opinion of the editor who was operating Rillieux's equipment on his own plantation and who knew scores of Louisiana sugar men who had seen the pioneer development of the multiple-effect evaporator.

Sugar technologists have generally been generous to Rillieux. One of the earlier writers, J. G. McIntosh, in his *Technology of Sugar* (London, 1903), gives five pages to the development, principle, and advantages of the Rillieux patents. "This is the system which constitutes the basis of all the saving in fuel hitherto effected in sugar factories. . . . Rillieux may, therefore, with all justice, be regarded as one of the greatest benefactors of the sugar industry." McIntosh also credits him with originating many engineering accessories now in use, such as the catchall to prevent the carrying over of sugar from one body to another by droplets in the vapors, the sight glass, or lunette, for watching the progress of the evaporation in the vacuum apparatus, and the substitution of cast-iron vessels for the costly copper which was previously thought to be essential for sugar evaporation.

This extensive recognition by McIntosh is especially noteworthy because some European writers on beet sugar manufacture have considered only the first faulty evaporators in Europe which, though they did not actually bear Rillieux's name, were copies of his successfully operating models in America.

Among American authorities, Charles A. Browne, eminent sugar chemist of the U. S. Department of Agriculture, who has supplied much of the source material for this article, says: "I have always

held that Rillieux's invention is the greatest in the history of American chemical engineering and I know of no other invention that has brought so great a saving to all branches of chemical engineering."

Alfred L. Webre, a specialist in the field of evaporation, is convinced that Rillieux was the originator of the basic theory and practice and that he was fifty to seventy-five years ahead of his time with many of his ideas. W. L. Badger, of the University of Michigan, who is another recognized authority, says: "There is no question that the first multiple-effect evaporator to be actually built and commercially operated was built by Norbert Rillieux in Louisiana in 1843, covered by U. S. Patent No. 3,237." (The first year of operation was 1845.) Edward Koppeschaar, a Dutch specialist who published a treatise on evaporation in 1914, was so impressed with Rillieux's claim to distinction in this field that he inaugurated a movement about 1930 which resulted in the crowning recognition for Rillieux. Koppeschaar enlisted the support of the great Dutch sugar expert, H. C. Prinsen-Geerligs, then president of the International Society of Sugar Cane Technologists, for erecting a memorial to the Louisianian, and together they canvassed the sugar interests of the world for contributions. The response was almost unanimous and the only detractors were a few German scientists interested in beet sugar, who argued that Rillieux's claims as an originator were not fully established. The list of thirty-eight contributors covers the globe and includes organizations representing every cane and beet sugar producing country or territory.

A bronze plaque was designed and cast in Amsterdam, and arrangements were made through Dr. Browne to have this placed in the Louisiana State Museum, which is housed in the old *Cabildo* in New Orleans. The tablet shows a bust of the inventor in bas-relief with "Norbert Rillieux, 1806–1894" cast in the bronze. In a frame of tiles, also made in Holland, is the inscription:

To honor and commemorate
Norbert Rillieux
born at New Orleans, La., March 18, 1806, and
died at Paris, France, October 8, 1894
Inventor of Multiple Evaporation and its
Application into the Sugar Industry
This tablet was dedicated in 1934 by
Corporations representing the Sugar Industry
all over the world

The date of his birth on his grave and on this tablet does not correspond to Rillieux's birth record in New Orleans. The use of the word "corporations" in the inscription is somewhat misleading as the contributors were scientific and technological organizations and not commercial corporations.

A comparison with George Washington Carver will undoubtedly suggest itself. While it is not necessary to dim the luster of Carver's name in order to enhance the standing of Rillieux, it should be said that Carver's color and picturesque background undoubtedly added to his fame, whereas Rillieux has received little recognition from the general public, and this is in part, at least, due to his Negro extraction.

COAL BALLS—A KEY TO THE PAST

By HENRY N. ANDREWS
MISSOURI BOTANICAL GARDEN, ST. LOUIS

MORE than 200 million years ago vast areas of the United States, from what is now Massachusetts to Kansas, were covered with low humid swamps—a "Great Dismal Swamp" on a magnificent scale. There is no sound basis for the belief that it was necessarily a steaming tropical jungle as we are so often led to believe, but it certainly was composed of a lush growth of plants most of which would appear weird and strange to a modern field botanist.

In the stagnant swamp waters there accumulated a vast quantity of plant debris, most of which partially decayed and became compressed to form the plant-mineral we call coal. A microscopic examination of this all-important natural resource rarely reveals any distinguishable plant structure other than spores and pollen grains, yet by indirect methods we have learned a great deal about the vegetation that was responsible for it. This information has been gleaned very largely from *coal balls,* aggregations of petrified plants found in the coal seams of Midwestern mines.

Through southern Illinois, to select a typical and productive hunting ground, the coal seams often lie within 20 to 50 feet of the surface of the ground, and if they are thick enough it is profitable to employ the open-pit, or "stripping," method of mining. Gigantic electric shovels, scooping up as much as 25 to 30 cubic yards of earth and rock at a time, clear away the overburden, laying bare the coal below. In exploring such an exposed surface we might, with a little luck, chance upon characteristic rounded knobs projecting a few inches above the level of the seam as a whole. Upon digging down into the coal a few inches we find these knobs to be brownish ball-shaped masses of petrified plants.

These petrifactions are called coal balls because of their more or less rounded shape and because they are found in the coal itself. They contain a heterogeneous assemblage of petrified plant remains—stems, seeds, leaves, and other plant organs, often in a nearly perfect state of preservation. They are representative fragments of the millions of tons of forest debris that served as the raw material for coal. Our knowledge of the origin of these fossils is by no means complete, but the essential steps in their formation seem clear—at least in a general way. During Upper Carboniferous times streams of water heavily charged with minerals seeped through the swamps. Scattered here and there fragments of plants served as a nucleus for the deposition of the minerals, and the resulting petrifaction prevented their being crushed and altered into coal.

The coal balls are local in occurrence and as sporadically distributed. One may wander along an exposed seam for half a mile or more and not find one, then patches often covering many square yards will turn up. Where there are any at all there are hundreds of pounds, or even tons. In some mines they are never found and in others they are the usual thing. Incidentally, they are a bane to miners, for in addition to dulling drills they will not burn and must be screened out before the coal is loaded for market. In size they vary from specimens smaller than a thimble to huge ones almost too heavy to lift.

It is fitting to point out that this mode of fossilization is quite different from

that of the famous nodules of northern Illinois, discussed in "Fossil Plant Miniatures of Mazon Creek" by Raymond E. Janssen (THE SCIENTIFIC MONTHLY, March 1945). Although both are concretionary in nature—that is, built up by continued deposition of mineral matter about a central nucleus—the Mazon Creek concretions are found in the shales above the coal and contain as a rule but one compressed plant fragment. On the other hand, the coal balls occur in the coal itself and are continuous masses of plant parts preserved in cellular detail. They offer a special problem for study, but yield correspondingly greater botanical knowledge.

Since these are a unique type of fossil, as well as one that has added an amazing chapter to our knowledge of the Coal Age forests, it may be of interest to outline briefly the procedure that the paleobotanist follows in order to extract from them their secrets of the past (Fig. 1).

The coal balls are cut in half with a diamond-impregnated saw, and with a specimen 3 to 4 inches in diameter this is a matter of as many minutes. Next, the cut surface is smoothed with carborundum abrasives of #100 and #400 and then dipped into dilute hydrochloric acid. The acid will dissolve out the mineral matter (chiefly calcium and magnesium carbonate), leaving a very thin layer of the unaffected plant tissues standing in relief. This etching time varies with the strength of acid em-

FIG. 1. COAL BALLS FROM AN ILLINOIS MINE
A FRAGMENT OF A LARGE SHEET OF SPECIMENS SHOWING THEM EMBEDDED IN COAL. NATURAL SIZE.

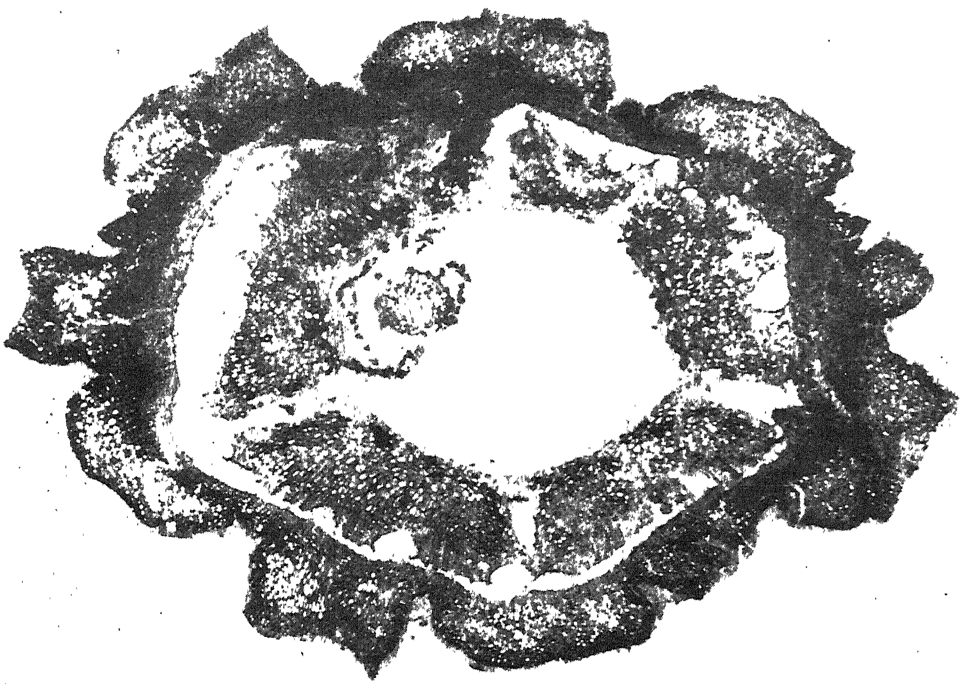


FIG. 2. A SMALL TWIG OF LEPIDODENDRON
A CROSS SECTION SHOWING PERIPHERAL LEAF BASES AND CENTRAL CYLINDER. MAGNIFIED 15 TIMES.

ployed and the relative amount of mineral matter present. After the etched surface is washed and dried a solution of *parlodion* is poured over it and allowed to harden overnight. In the morning the resulting film is peeled off, using a razor, and with it comes a thin section of whatever plants have been exposed.

FROM the great open-pit Pyramid Mine south of Pinckneyville, Ill., we have collected tons of coal balls during the past five years. These have revealed a wide variety of plants, although a species of *Lepidodendron* is by far the most abundant (Figs. 2, 3). In fact 90 percent or more of the petrified vegetable debris of the coal balls consists of the stems, roots, leaves, and reproductive structures of this plant, suggesting that it probably composed a nearly pure stand in the forests of that region. The Lepidodendrons, although related to the modern diminutive Club-mosses of our eastern woodlands, were trees of respectable size, attaining a height of 60 to 70 feet and bearing a profusion of small needle-like leaves. Their superficial appearance was similar to that of a fir or spruce. The stem anatomy reveals some of the distinctive features of these trees. In proportion to the general size, the stem possessed little wood, depending for additional support on a tremendous growth of cork tissue. Since this cork composed a considerable bulk of the stem and was less susceptible to decay than most of the other tissues, we may conclude that it is the chief constituent of the coal of this region, and this conclusion is substantiated in other ways.

The quality of preservation in the coal balls from any mine varies a good deal, depending on the extent to which the plants decomposed prior to fossilization,

Magnified 1000 times

FIG. 3. SPORES FROM A LEPIDODENDRON CONE

on the degree of replacement by the mineral, and on the nature of the mineral itself. The chief curse of the coal ball hunter is iron sulphide. Some specimens are composed almost entirely of it and in such cases they are of little or no botanical value. Specimens containing small quantities of iron sulphide are workable and may contain well-preserved plants, but in general they are distinctly inferior to those petrified by calcium and magnesium carbonate only. The "sulphur balls," as they are referred to by the miners, are excessively heavy and readily distinguished in the field in this way.

The perfection of preservation of relatively delicate plant tissues is a never-ceasing marvel to the uninitiated and to the professional paleobotanist alike. One of the most striking cases we have encountered is the presence of beautifully preserved fungus mycelium in the cortical cells of a fern stem (Fig. 4). The vegetative filaments (mycelium) of fungi are of common occurrence in petrified plants, but in this instance the mycelium had profusely invaded many of the host cells in a way strongly suggesting the mycorrhizal relationship in living plants. A vast number of modern perennial plants are now known to be dependent upon such a fungal association for certain phases of their nutrition, and it is of interest to find that this association must be one of great antiquity.

Plant structures as delicate as root hairs have turned up on at least one occasion in the Illinois coal balls. The roots, stems, and leaves of plants assigned to the Cordaitales, ancestors of present-day conifers, occur frequently enough to suggest that they were second in numerical importance only to the

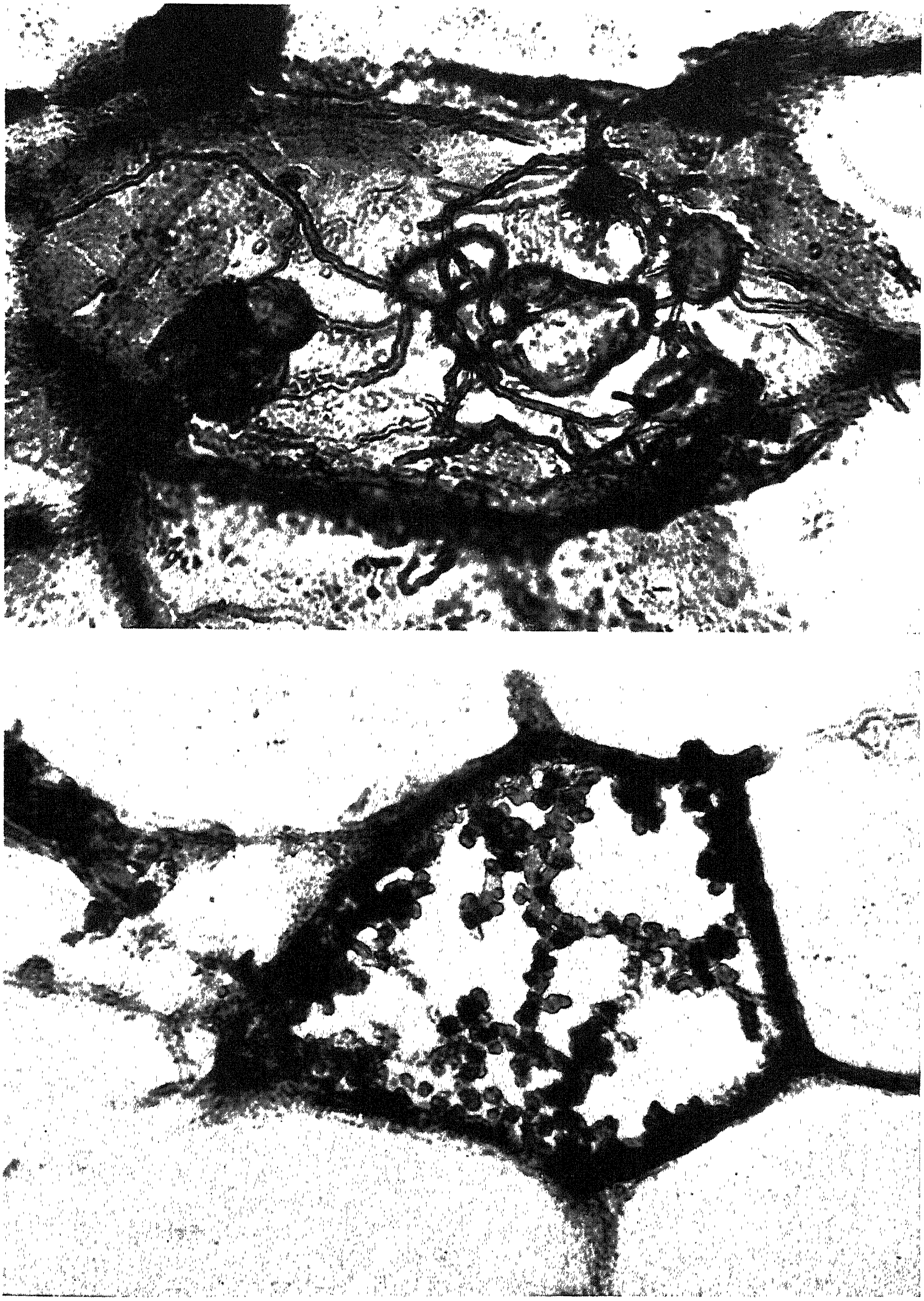


FIG. 4. FOSSIL FUNGUS MYCELIUM

SEEN IN CORTICAL CELLS OF A FERN FROM AN ILLINOIS COAL BALL. MAGNIFIED ABOUT 500 TIMES.

FIG. 5. A SEED-FERN STEM FROM IOWA
THIS CROSS SECTION OF *Medullosa Thompsonii* SHOWS THREE STELES COMPOSING THE CENTRAL CONDUCTING SYSTEM OF THE PLANT AND A LEAFSTALK DEPARTING AT THE RIGHT. MAGNIFIED 1.7 TIMES.

Lepidodendrons. They were trees that attained a height of 80 to 100 feet and bore long strap-shaped leaves, presenting a close superficial similarity to the foliage of iris or corn. In one of our earlier collections from the Pyramid Mine a stem was found with roots in organic connection, and some of the smaller rootlets retained their epidermal root hairs sufficiently preserved that they might well be used to demonstrate the salient structural features to a student in an elementary botany course.

Contrary to what might be expected, the more fragile plant tissues are often better preserved than the more resistant ones. The integument, or coat, of a seed, likewise belonging to the cordaitean group, found recently, is typical. It consists of three distinct layers: an outer one of large thin-walled cells, probably quite fleshy in life, and two inner layers, the cells of which had walls that were thick to the point of being stony, much like the "grit" tissue of a pear. The outer tissue was very well preserved whereas the inner two had become rather badly decomposed prior to fossilization. This is by no means an isolated case, and the explanation seems clear when one reviews the probable sequence of events. The mineral-bearing waters were able to penetrate the thin-walled cells much more rapidly than the sclerotic ones, thus insuring their petrifaction before bacterial and chemical decomposition set in to any appreciable extent.

As might be expected, the flora of the coal balls varies somewhat from place to place. For example, specimens gathered in the vicinity of Des Moines, Ia., reveal a different Carboniferous landscape from the Illinois mines a few hundred miles to the southeast. In Iowa, seed plants seem to have been dominant, including members of the Cordaitales, and a considerable variety of species belonging to that most fascinating of all extinct fossil groups, the Seed-ferns, or Pteridospermeae (Fig. 5). This does not imply, however, that the plants composing the two floras were entirely different, for in nearly all Upper Carboniferous deposits from Kansas and Iowa, east to China, there occur certain common genera of Lycopods and Calamites and a wide variety of fernlike foliage. Some of the latter we know belonged to true ferns, a few have proven to be Seed-fern fronds, and the affinities of a great many remain in question.

The abundance of this fernlike foliage

that is found almost everywhere in the shales that overlie coal deposits of Pennsylvanian age has led to numerous learned monographs. Unfortunately much of it is sterile, offering no recognizable clues as to its natural relationships. A suggestion began to grow in the minds of paleobotanists during the latter part of the eighteenth century that these apparent ferns may have borne seeds. The evidence, however, was not forthcoming until about 40 years ago, and in more recent decades a number of these "ferns" have been found to be Seed-ferns, presenting a multitude of distinctive characters, especially in the anatomy of their seeds and stems.

In the Iowa coal balls we have found a number of new and unique Seed-ferns, as well as specimens the same as, or closely related to, previously described European species. Of particular interest is the genus *Medullosa,* distinguished by having more than one woody cylinder, or "stele," composing the central part of the stem. And like the Lepidodendrons their wood-producing ability was not sufficient to support a great weight. In lieu of this the outer cortex developed a stout layer of tissue composed of anastomosing, vertically aligned strands of fibrous cells. The seeds of these plants are similar in certain respects to those of modern cycads, having a well-developed pollen chamber near the apex of the seed, in which the pollen grains apparently germinated and produced their male sex cells (Fig. 6).

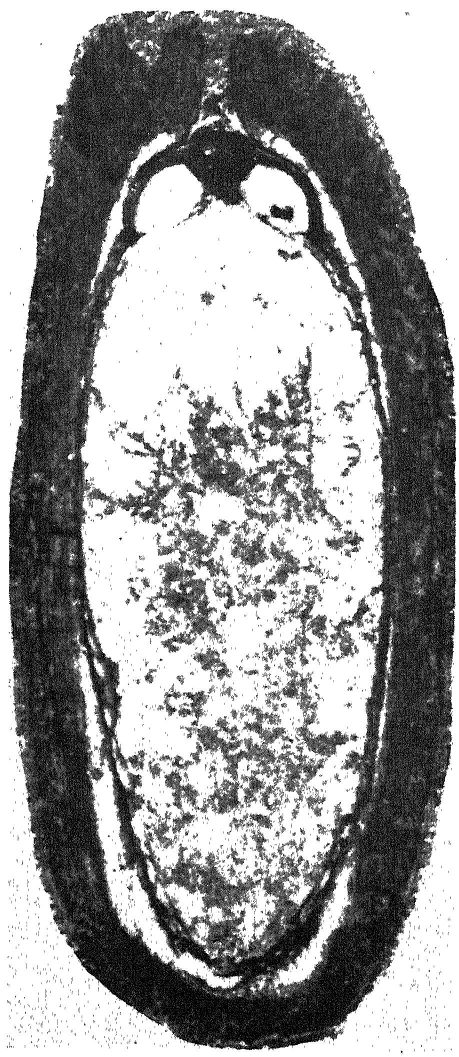


FIG. 6. A FOSSIL SEED

IN THIS LONGITUDINAL SECTION NOTICE THE POLLEN IN THE TOP CHAMBER. MAGNIFIED 25 TIMES.

THESE calcareous concretions have been known for nearly a century from the coal fields of northern England. The earliest authors who gave a clear account of the petrified plants found within them were Joseph D. Hooker and E. W. Binney in 1854. Later in the century the works of W. C. Williamson began to reveal the extent of the prolific treasure of ancient plant life contained in coal balls, and his work was carried on by the late D. H. Scott, to whom we are indebted for a rich series of contributions on Paleozoic botany as well as an encyclopedic, yet very readable, text on the anatomy of fossil plants. In more recent years these petrifactions have been found in the Dutch, Belgian, and Russian coal fields.

In this country coal balls were discovered in a mine near Des Moines, Ia., as long ago as 1894. They apparently were not then brought to the attention of paleobotanists and consequently remained as dormant as before until years later. About 25 years ago the late A. C. Noé, of the University of Chicago, initiated the first productive interest in coal balls in America through their discovery in open-pit mines through central and southern Illinois. They have since turned up in a number of adjacent states, including Indiana, Kentucky, and Kansas, but the better preserved and largest collections to date have been obtained from Iowa and Illinois. We still lag considerably behind the great contributions of the British paleobotanists. There is now, however, a great deal of active interest in these American deposits which have already yielded such an abundance of beautifully preserved plants of the past.

THE SECOND LAW OF THERMODYNAMICS

A mountain range worn down may rise again,
Rivers and winds reverse their wonted flow,
Seas surge and sink, the glaciers come and go,
The distant mighty Cepheids wax and wane;
All these are local transitory show
To which the transient mind so fondly clings,
But entropy and time are endless things,
Immutable and changeless is their flow.

The Law predicts a dismal final state
The doom of this bright universe of ours,
Slowly downhill it drifts to meet its fate,
Reprieve nor hope is found in all its powers,
Darkness and death for all things surely wait
When change shall cease and no star marks the hours.

THOMSON KING

LEONARDO DID IT FIRST

By HARVEY N. DAVIS
PRESIDENT, STEVENS INSTITUTE OF TECHNOLOGY

"LEONARDO did it first" is a phrase familiar to scientists in many fields. Few of them may realize, however, that an interesting and comprehensive collection of Vinciana is assembled at Stevens Institute of Technology, where it is available for reference to scientists and scholars everywhere.

The Lieb Memorial Collection consists of about 2,400 items, including a complete set of the Leonardo manuscripts in facsimile. The few originals, of course, are preserved in the great museums of the world. Many of the facsimiles, issued in limited editions, have been long out of print and are almost unobtainable. In addition, an attempt has been made to secure every worth-while book or pamphlet on Leonardo in every language. This is a "scholar's collection, made by a scholar," accessible to all those interested in investigating the life, mind, and work of Leonardo da Vinci.

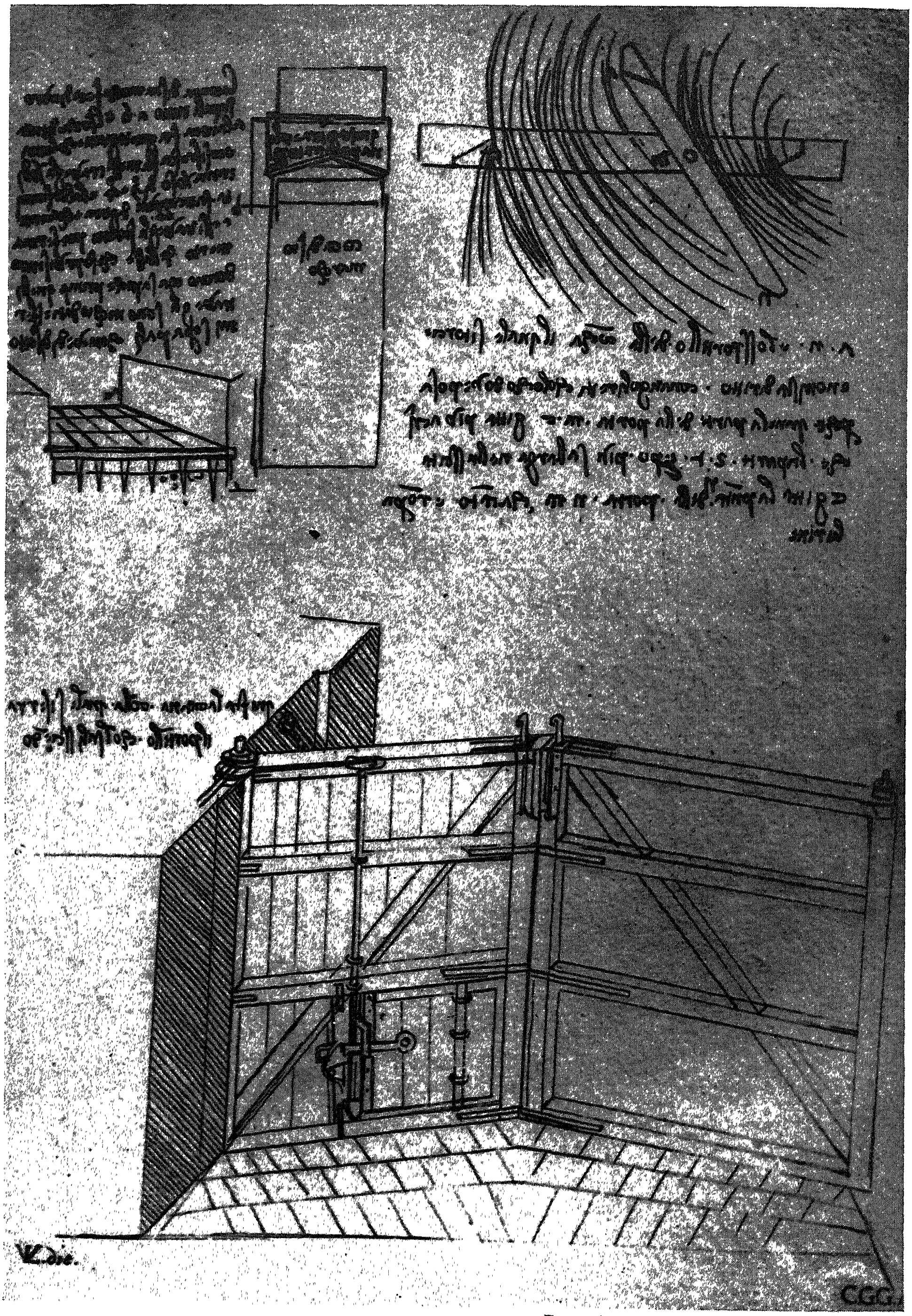
John W. Lieb, alumnus and trustee of Stevens, never knew that his collection was to have a permanent home in his own Alma Mater. There is no doubt that he would have been gratified to have it so. He thought of the collection as a nucleus of the history of science and art and wrote, a short time before his death, "I am very anxious that the collection should find lodgment in some public institution where it could be studied by engineers, scientists, artists, and professional men."

Lieb graduated from Stevens in 1880 and, after experimenting with the arc light in Cleveland for a year, became associated with Thomas A. Edison in 1881. Edison sent him to Italy to install the first Edison central station in that country. Lieb's assignment to Italy seems almost to have been predestined. In addition to doing a great job of engineering there himself, he was to rediscover a much older engineer and to make it possible for students all over the world to become better acquainted with that many-sided genius.

It was in 1890 that the Italian Edison Company, through Mr. Lieb, obtained a franchise from the Italian Government for the construction of a hydroelectric plant at Paderno on the River Addo. It was necessary, under the terms of the concession, to assume in perpetuity the maintenance of the Martesana Canal. This canal was to serve as the intake for the hydroelectric plant while serving as part of the extensive canal and irrigation system covering the plains of Lombardy.

When the bed of the canal was drained, it was found to be in perfect condition, and the locks at either end had been built in accordance with the best modern practice in hydraulic engineering. An examination showed that the original of these locks, identical with those then in use, had been built about the year 1500. The designer of these original locks was Leonardo da Vinci, who probably also laid out the plans for the canal itself. Amazed by the perfection of the system, Lieb devoted all his spare time to further research on da Vinci. In a remarkable way, he relived the life of his famous predecessor.

The result of his labors is the collection at Stevens. The Lieb Memorial consists of two research rooms and an exhibition room housed in the Stevens Library building, a square, brick, ivy-clad edifice erected in 1918, with a dignified inscription in stone, "The Lieb Me-



From Stevens collection of reproductions

LEONARDO'S LOCKS ON THE MARTESANA CANAL

morial,'' on the west door. The collection was purchased two years after Lieb's death and presented to the college in 1932 on a day fittingly chosen because it was the birthday of his lifelong associate, Thomas A. Edison—February 11. Fireproof rooms for the collection were provided by a fund raised by the late William S. Barstow, long a trustee of Stevens, and president of the Thomas Alva Edison Association. Besides the Leonardo collection the rooms house a historical collection of some 9,000 Stevens papers and a reference library on the work of Frederick W. Taylor (Stevens '83), originator of scientific management.

Although three rooms constitute the Lieb Memorial, one is dedicated principally to a library containing Lieb's collection of Vinciana. It is a small book-lined room, with filing cabinets at one end and a large desk in the middle. One-fourth of the collection pertaining to Leonardo constitutes the scientific material, which is concentrated at one end. The shelves are lined with books in English, French, German, and Italian. The famous ''facsimilae'' are bound in appropriate bindings and are large and impressive volumes. Reproductions of Leonardo's famous *Note Books* are kept in ivory-colored oversized folios, next to the maroon leather-covered *Codex Atlantico.*

In the large glass exhibition case in the middle room are some typical facsimiles of Leonardo's drawings in detail. There are a few choice reproductions of his paintings on the walls of the library room, including the famous self-portrait done in crayon.

In charge of the collection, as well as of the library proper, is Enid May Hawkins, who came to the college thirty-eight years ago as librarian, and who has had an influential part in the growth of the library and of its special treasures.

The entire collection is heavily insured, since it can never be replaced or duplicated. Many of the European houses that published the works went out of existence after the war began.

The oldest book of the collection, in its original binding of ivory-colored vellum, is the *Divina Proportione* by Paciolo Luca, published during the life of Leonardo and illustrated by him. A half-dozen scrapbooks containing clippings and magazine articles about Leonardo are kept in the files. The whole collection has been ably catalogued by Maureen Cobb Mabbott, a well-known Leonardo scholar, who says that this is probably the only collection of Vinciana made by an engineer with emphasis on Leonardo's mechanical genius.

He was ardently interested in every branch of theoretical or applied science in which any beginning had been made in his age (1452–1519) as well as in those in which he was the pioneer. He was full of new ideas concerning both the laws and applications of mechanical forces.

Only recently has the work which Leonardo did as a pioneer of physiological anatomy been appreciated. Copies of his anatomical sketches in splendid facsimile reproductions are to be found in the collection at Stevens. These consist not only of his beautiful and accurate drawings of the human figure, but also detailed studies of the internal structures: heart, lungs, and respiratory system.

Leonardo left over 6,000 pages of manuscript, almost all of it in ''mirror-writing,'' each sheet covered with sketches. The small details of his work are fascinating to the engineer. For instance, he made a complete study of mills—arrangements for all sorts of power: wind, water, horse, mule, cranks, and various pumps. His drawings, no matter how technical, show that he was primarily an artist. They are often accompanied by minute drawings of hu-

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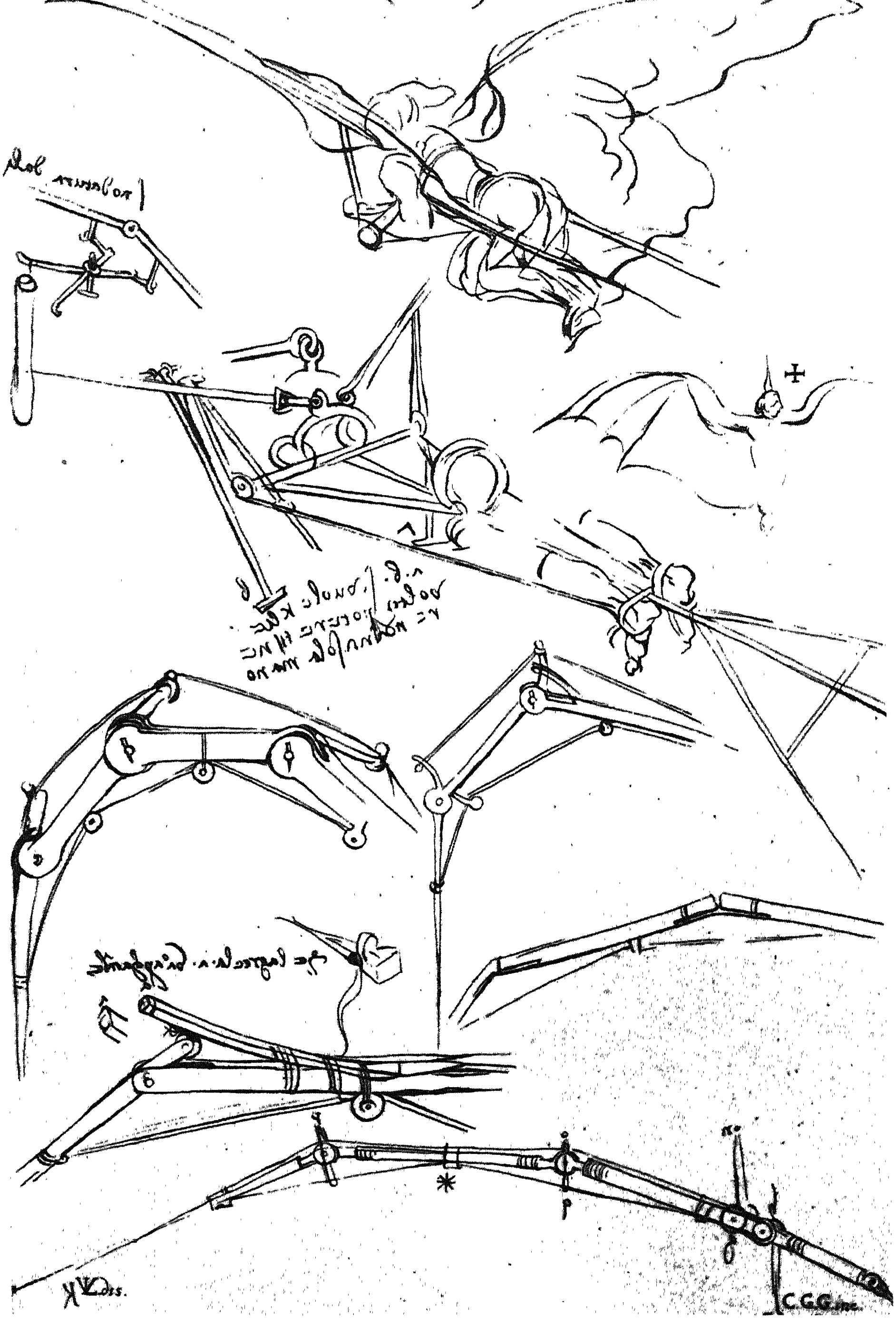
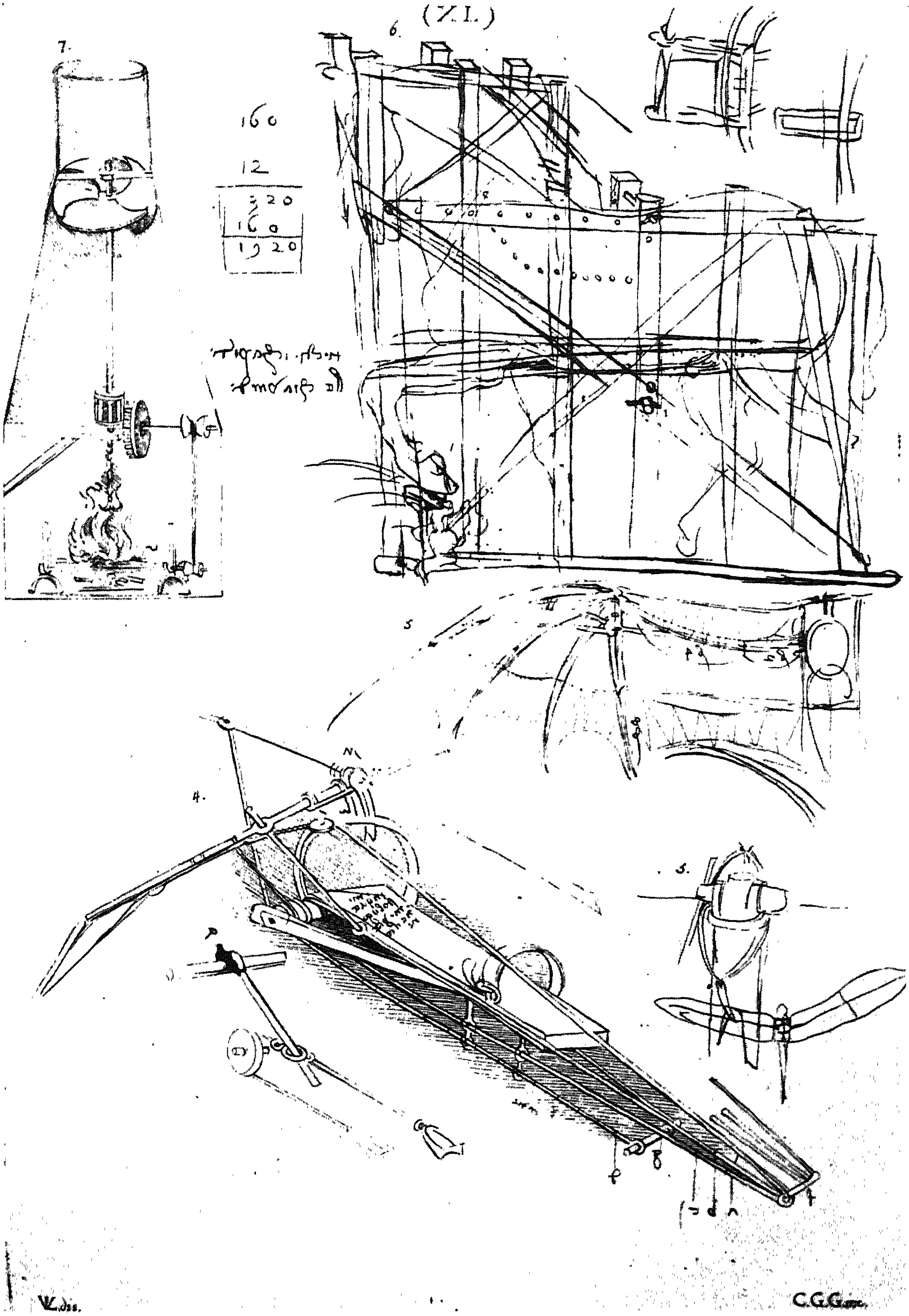


From Stevens collection of reproductions

LEONARDO'S MECHANICAL ANGEL



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GADGETS OF THE RENAISSANCE

man beings, so real and anatomically perfect that they could serve as models in any drawing class.

To the scientist, however, his genius for mechanical invention is most important, and because of our recent concern with the instruments of war, it may be particularly interesting to recall his ventures in that field. At the age of thirty, Leonardo wrote a letter to the Duke of Milan, applying for work. He said, in part:

> I can construct bridges which are very light, strong, and very portable, with which to pursue and defeat the enemy. [This would certainly suggest the Bailey Bridge, of recent use.]
>
> In case of siege I can cut off water from the trenches and make scaling ladders and other similar contrivances.
>
> I can also make a kind of cannon which is light and easy of transport, with which to hurl small stones like hail. . . .
>
> I can noiselessly construct subterranean passages, either straight or winding, passing if necessary underneath trenches or a river.
>
> In times of peace I believe that I can give you as complete satisfaction as anyone else in the construction of buildings, both public and private, and in conducting water from one place to another.

Needless to say, he got the job! And kept it for sixteen years!

That Leonardo knew the principle of the submarine is evident in other notes. He contemplated an invention "to smash ships in the keel and sink their crews" and mentioned the existence of a machine by which many people could stay some time under water. Recently, when the moral issue relating to the use of the atomic bomb was raised, it was recalled that Leonardo had said, in respect to the submarine, that he would not "describe the method of it because the evil nature of man is such that it would be used as a means of destruction, namely, by assassinations at the bottom of the sea."

In spite of his many explorations into almost every conceivable field, Leonardo was haunted all his life by one great idea—the theory and practice of flight. "The Great Bird," he said, "will fly." His *Treatise on Birds,* one of the sources in the Stevens Library, has been an endlessly rewarding book to students of aviation.

Ivor B. Hart, in his *Mechanical Investigations on Leonardo da Vinci,* says: "Although he owed nothing to his contemporaries or those who lived before his day, careful studies of his manuscripts make it clear that Leonardo was not only a pioneer of flight, but the first pioneer." He also understood the theory of balloon flight but, as Hart says, "In the balloon there is neither life nor control." In "The Great Bird" of Leonardo's imagination there were both.

According to the same authority, Leonardo may well be credited with the first helicopter, and certainly with the first parachute. In Leonardo's own words, "If a man carry a dome of starched material eighteen feet wide and eighteen feet long he will be able to throw himself from any great height without fear of danger."

These are typical of what a student may uncover in the library at Stevens. What is more significant, he may learn that Leonardo's discoveries or inventions were always based on the scientific approach.

THE SUN MAKES THE WEATHER

II. WEATHER EFFECTS OF SOLAR VARIATION*

By C. G. ABBOT
RESEARCH ASSOCIATE, SMITHSONIAN INSTITUTION

Everybody who undertakes to play with long programs of observing, seeking to know the effect of some supposed cause, likes to clear away from the figures he is using the effects of all well-known causes. Then his own supposed cause will have the field to itself. I therefore wish to clear the reports of temperatures I am about to use from the effect of night and day, and from the effect of summer and winter, before looking for the effects on temperature of the variation of the sun. Fortunately, the Weather Bureau has done this for me, for it publishes daily the average temperature for each 24 hours. That clears out the night-and-day effect. It has also computed, from the daily records of about 75 years, the expected normal temperature of each station for each day of the year. For example, at Washington, on Washington's Birthday, the normal average temperature is 36° F. On February 22, 1944, the actual average temperature was 44°. The difference was 8°. This +8° the Weather Bureau calls the "departure from normal temperature" for that day. Every month it publishes all of these "departures" for the individual days. It also publishes monthly average departures. In February 1944 it was +2.8°. As for precipitation, it publishes the amount of rain or snowfall from midnight to midnight for each day. At the end of the month it publishes the total precipitation for that month and the normal to be expected, from the average of records for that month for about 75 years. Thus in February 1944, 2.48 inches of precipitation fell. The normal for February is 3.37 inches. So the precentage of normal in February 1944 was 74.

Of the nearly 500 cases of short-interval changes of the sun's heat noted prior to the year 1940, about half were cases of *rise* and the other half of *fall*. They usually lasted about 3 days, and their average range was about 0.75 percent. As their effects would almost surely be different for different times of the year, I examined the temperature effects of these solar fluctuations in groups separately, month by month. In January of the years 1924 to 1939, I found 21 cases of rising, and 14 cases of falling, solar heat intensity. Corresponding to each rising case I wrote in a line the 20 Weather Bureau temperature departures for Washington, beginning 5 days before the sun's heat began to rise and continuing to 14 days thereafter. In this way I soon had a table of 21 lines with 20 columns of temperatures. I then took the average of each column. I did similarly for the 14 falling cases.

Most readers are familiar with the curves that newspapers publish so often, showing how stocks rise and fall, how prices of all sorts of goods change, how the population is meeting influences of many kinds, and, in short, how a multitude of things vary with time. Since this method is so well known, I use it in Figure 1, to show results for the months of January, February, and March. We find that small solar changes produce from 10° to 20° change in our Washington temperatures. The temperature changes for rising solar activ-

* Continued from page 210 of preceding issue.

ity are opposite to those for falling activity. The largest effects occur some 10 days after the solar changes seem to begin. The effects last through about 17 days, from 3 days *before* our measurements begin to show solar changes till 14 days *after*. I do not know why the weather effects start before our measurements begin to show solar change, but we do not yet know all that happens in the sun.

The other months give similar results, though they are not identical from month to month. All other stations behave as Washington does, except that the details of the effects differ from station to station. Similar details come a few days earlier at western stations, for weather, as we all know, drifts from west to east.

Our scientific critics, who "are from Missouri," have hitherto been reluctant to credit fully the day-to-day solar changes we have published. They praise the accuracy of our work, but doubt if it is accurate enough to show solar changes averaging only 0.75 percent, or even smaller. Hence I found it necessary to cite other people's observations of other solar phenomena in order to support these new and very important conclusions as to the weather effects of small day-to-day fluctuations of the sun's activity. I have used two such sources, both of which confirm our results as splendidly as one could wish.

One of these concerns the chemical element calcium, which exists in a very rarefied gaseous form in cloudlike masses high up in the atmosphere of the sun. These calcium clouds, named "flocculi" by the late Dr. George E. Hale, cannot be observed by telescope. They are revealed and photographed by an instrument called the "spectroheliograph," which Hale invented. The monks of the Jesuit solar observatory at Ebro have made such photographs, measured the areas of the flocculi, and published the results since 1910 for every day on which observation could be made. I computed from their measurements the changes indicated by the area of the flocculi and have used them as a measure of change in the sun's activity.

As another measure for changes in solar activity, I have used the measurements of the Carnegie Institution on ionization, made in Peru and Australia beginning in 1938. These were graciously furnished to me from manuscript by Dr. John A. Fleming, Director of the Carnegie Department of Terrestrial Magnetism. Electric ions are unit electric charges. They are shot out abundantly from sunspots and, traversing without hindrance the intervening space of 93,000,000 miles, they enter our atmosphere, where they multiply by breaking up, or "ionizing," the molecules of the gases of our upper air. They do not penetrate through the atmosphere to the earth to any great extent, but form layers of ions high above us. These are called, after two famous scientists, the Kennelly-Heaviside layers. It is these layers that reflect radio waves around the world. If they did not exist radio waves would go unhindered into space, and we would get no long distance programs.

The intensity of ionization in the principal ionic atmospheric layer, which scientists have named F_e, is measured every hour of every day at the two Carnegie observatories in Peru and Australia. During the night the values are small. As the sun comes up they greatly increase and remain high until sunset. I have taken averages of the values found in the daylight hours for every day available from 1938 to 1944. Fluctuations of my averages were at once apparent. They too, like the solar constant measurements and like the Ebro flocculi measurements, are in almost exactly the same relationship to the temperature departures at Washington

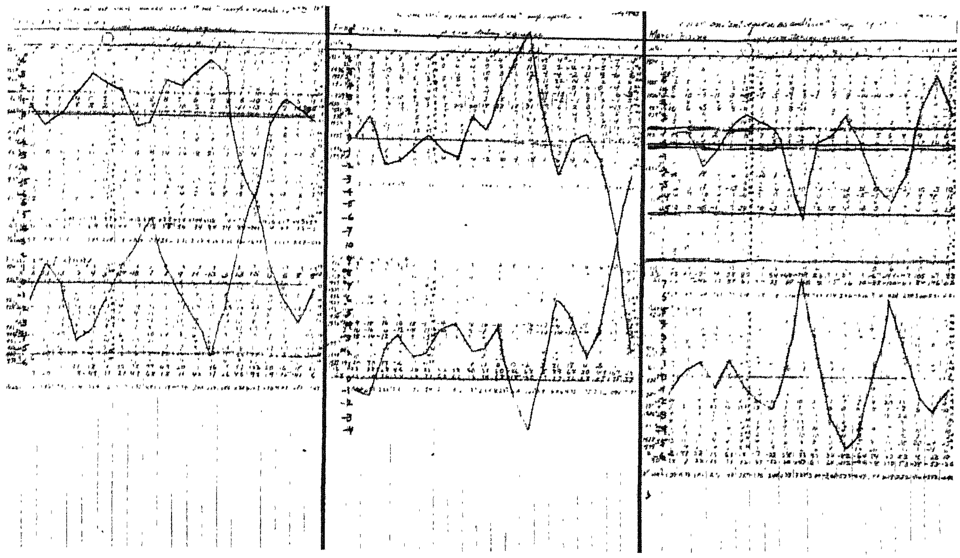

FIG. 1. SOLAR CONSTANT SEQUENCES AND TEMPERATURE DEPARTURES
AUTHOR'S WORKSHEET OF TEMPERATURE DEPARTURES IN WASHINGTON, D. C., DURING JANUARY, FEBRUARY, AND MARCH, SHOWING THAT RISING AND FALLING SOLAR ACTIVITY AFFECTS WASHINGTON TEMPERATURES OPPOSITELY AND STRONGLY FOR MANY DAYS. THE UPPER CURVES REPRESENT MEAN TEMPERATURE DEPARTURES ASSOCIATED WITH RISING SOLAR ACTIVITY; LOWER CURVES FOR FALLING ACTIVITY.

already described. So our work on short-interval solar variation and weather is confirmed from two quite independent sources, involving other years, other methods, other observers, other countries, and other phenomena.

And now comes the most suggestive result of all. Since these temperature changes are of importance until at least 14 days after the solar changes have been observed, there is in this new discovery ground to hope that certain weather features may be predicted for at least 10 days in advance. Ionization observations can be made on every day, rain or shine. Hence we must probably look to them for the groundwork of measurements on which to take this new important step in weather forecasting. I have, however, made a preliminary test of this new method, employing Smithsonian solar constant basic curves, such as those of curves 1 and 2 of Figure 1, and using Ebro photographs of solar flocculi to yield the dates when solar changes occurred. In order that I might immediately test the forecasts, I used months in former years, when the actual Washington temperatures were already available for comparison with expectation.

In Figure 2 solar predictions of Washington temperature departures for the months of September and October 1935, are shown by dotted lines, and the corresponding actually observed temperature departures by solid lines. The prediction was made by noting from Ebro flocculi records the dates when ups and downs in solar activity seemed to begin and translating them backwards 2 days in each case to suit heat observations. I then wrote down in several successive columns the marches of temperature each of these solar changes portended. By adding the values in these several successive columns, the total expected temperature effect of all these solar changes was summed up. These results are plotted in the dotted line of

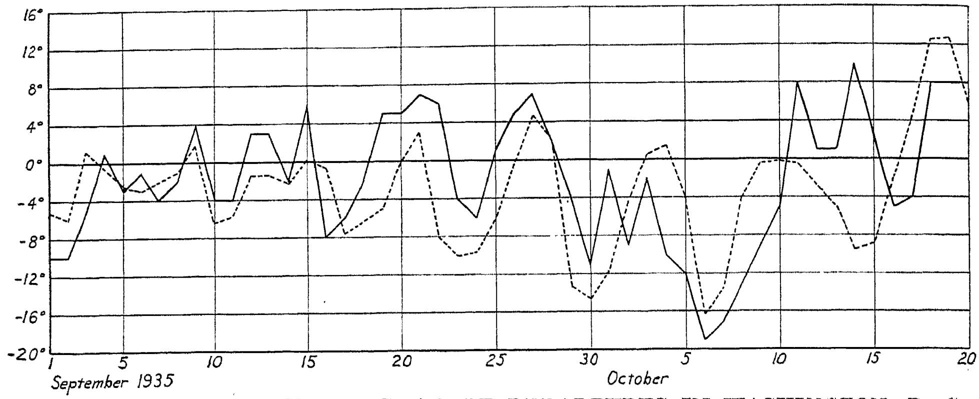

FIG. 2. PREDICTION OF TEMPERATURE DEPARTURES IN WASHINGTON, D. C. THE DOTTED LINE REPRESENTS TEMPERATURES PREDICTED FOR TEN DAYS IN ADVANCE FROM SOLAR CHANGES. THE SOLID LINE SHOWS THE TEMPERATURES THAT WERE ACTUALLY OBSERVED IN 1935.

Figure 2. It will be seen that the average difference between predicted and observed daily temperatures in this test prediction was but 4.9° F., and that the ups and downs of predicted and observed curves occurred nearly always on the same day. The actually observed range, as expected, exceeded the predicted range, for terrestrial influences were excluded from this purely solar prediction. Ordinary predictions for only 24 hours in advance are seldom more successful, either as regards the times of changes of temperature or the magnitudes of changes, than this long-range solar forecast.

I have predicted similarly for several other intervals. A summary of results is given in Table 1. On the whole I believe the method gives great promise. It can probably be improved in the hands of meteorologists skilled in knowledge of terrestrial influences. It can also be improved by combining all the evidence available from Smithsonian, Carnegie, and Ebro sources, so as to

TABLE 1

GENERAL SUMMARY OF A PURELY SOLAR PREDICTION OF THE DEPARTURES FROM NORMAL TEMPERATURE AT WASHINGTON, COVERING THE MONTHS MARCH AND APRIL OF 1911 AND 1915, AND SEPTEMBER AND OCTOBER OF 1917 AND 1935

			Percentages
Total days predicted		201	
Observed and predicted of same signs		139	
" " " " opposite signs		62	
Observed departures plus		65	
Predicted " "		64	
Observed departures minus		136	
Predicted " "		137	
Differences, Obs.-Pred.	0° to 2°	66	32.7
" " "	3° to 4°	43	21.3
" " "	5° to 6°	32	15.8
" " "	7° to 10°	33	16.3
" " "	11° to 15°	20	10.0
" " " greater than 15°		8	4.0
" " " less than 6°			69.8
" " " more than 6°			30.3
" " " , general mean			5°35

Correlation coefficient 56.9 ± 3.2 percent.

yield all the dates when solar changes appear to have occurred in the years from 1910 to 1944. For then the basic curves for any desired station will rest on many times more cases than those combined in the Smithsonian curves alone.

If the method comes to be officially used and is based on ionization measures, the combined results of some half-dozen observatories will be available. Combining these will eliminate spurious apparent dates of solar changes and will thus promote accuracy of prediction. I greatly hope that in this way excellent temperature forecasts for 10 days in advance may soon be made possible for many stations in the world.

Is this method also available for barometric pressure and precipitation? I have already made some studies of solar changes as related to barometric pressure and find the relationships are, as might be supposed, not so close as in the case of temperature. However, it seems that forecasts of barometric pressure may be of considerable value. As regards precipitation, the prospect for 10-day predictions is more doubtful. A combination of forecasts of temperature and barometric pressure might indeed give some grounds for a prediction of precipitation, but I should hardly anticipate a high average of success. Doubtless it will be found, when more extensive studies have gone forward, that the solar influence is more potent at some stations than at others. Some stations, indeed, may be found to be so strongly influenced by unpredictable terrestrial influences, that solar predictions at these stations will have little value.

Monthly averages of solar constant measurements, while apparently fluctuating in a perfectly haphazard way, really follow the pattern produced by the combination of 16 regular periods of different lengths, operating simultaneously. These periods, ranging from $8\frac{1}{3}$ to 273 months, are all close to being integral fractions of 273 months, which, besides, is close to being double the length of the famous sunspot cycle of $11\frac{1}{3}$ years. If these periodic solar changes are of importance in weather, we should therefore expect that the principal details of weather changes, reckoned as "departures from the normal," would tend to repeat themselves at intervals of nearly 23 years. For at the conclusion of this master period all the solar changes would begin to repeat in approximately the same order as at its beginning. There are one or two reservations I have made to this statement in my more technical publications, but by and large it is true.

I have in my office the records of departures from the normals in temperature and precipitation for many stations in all parts of the world, arranged in cycles of 23 years. As the monthly values jump about too much to display plainly what one wishes to look for, I have used 5-month smoothed running means of the monthly values of departures. Thus for Washington in 1944 the monthly departures from normal temperature in the months January to May were as follows:

Jan.	Feb.	Mar.	Apr.	May
+4.0	+2.8	+0.4	+0.5	+7.9

Their algebraic sum is +15.6°. Dividing by 5, I get, as the 5-month smoothed running mean for March 1944, the departure +3.3°. Similar procedure is followed for all other months.

When I compare the smoothed departures in successive cycles of 23 years each, I find for many stations a considerable similarity of principal features. I have reduced this circumstance to a rough-and-ready rule-of-thumb method of forecasting. I use it for months and even years in advance. My rule is this: As the master cycle is a little less than 23 years, I add alge-

braically the smoothed departure from normal of April, 46 years ago, to that of February, 23 years ago. Dividing by 2, I use the algebraic half as the departure for January this year, and so on for other months. I will give two illustrations of the degree of success sometimes attained by this rough method of forecasting.

Several years ago an army engineer, in charge of some work in the Tennessee Valley Authority, asked me to predict for him the percentage of normal precipitation to be expected in that area for the 3 months, November, December, and January. I warned him not to depend on my forecast too much, chose 10 stations to represent the area, and gave him for each one the forecasted precipitation according to the method above described. The average result for the 10 stations seemed to indicate between 84 and 87 percent of normal for the total precipitation of the 3 months. In February he informed me that it actually turned out to be 87 percent.

From 1934 to 1940 I had a letter each winter from a farmer in South Dakota, living about 100 miles from Bismarck. In 1934 he drew a dark picture of the great drought and begged me to tell him what the prospects were. I based my replies on what had happened at Bismarck 46 years earlier. I have found that for precipitation there is apt to be a greater similarity of events at intervals of 2 and 4 times the 23-year master cycle than in the 23-year intervals themselves. Unfortunately records 92 years back are seldom available. I myself was really astonished, however, by the approximate accuracy with which I forecasted each year the precipitation for the year to follow in the Bismarck region. After 6 years of this, my correspondent wrote in 1940: "For several years you have given us very accurate predictions of the season's weather, so accurate that I am constrained to solicit from you like guidance this year in the crops we may expect. I have great faith in your predictions."

While this is very flattering, I must discourage other similar inquirers. The fact is that it does not turn out so perfectly as this for every region. More compelling still, I have much important work in hand. If many farmers in the United States should apply to me, as they would be sure to do, and if I consented to answer their inquiries, I could not satisfy their demands even by giving up all the work I hope to finish in my few remaining years.

There is a better, but far more onerous, method for long-range solar forecasting than this rough-and-ready procedure I have described. Each of the 16 periods, integrally related to 273 months, has a measurable effect on temperature and precipitation. In illustration I may say that I have followed the effect of the $8\frac{1}{8}$-month solar period on temperature at Copenhagen, Vienna, and New Haven from 1800 up to 1940—140 years altogether. It appears that the sun must have varied perfectly regularly in this $8\frac{1}{8}$-month period throughout all that long interval. For at these 3 stations there was produced by this $8\frac{1}{8}$-month solar period average temperature ranges as follows: Copenhagen, 2.35°; Vienna, 2.02°; New Haven, 1.30°. Some of the other solar periods are even more effective. If, however, they were only on equality with the $8\frac{1}{8}$-month period they could bring about a swing of more than 20° in temperature between dates when they all tended together to raise, and dates when they all tended together to depress the temperature.

For several stations I have computed the average effect of each of the solar periods separately, by analysis of long temperature and precipitation records. Having thus determined their several effects, and the times when they severally occur, I have added them algebra-

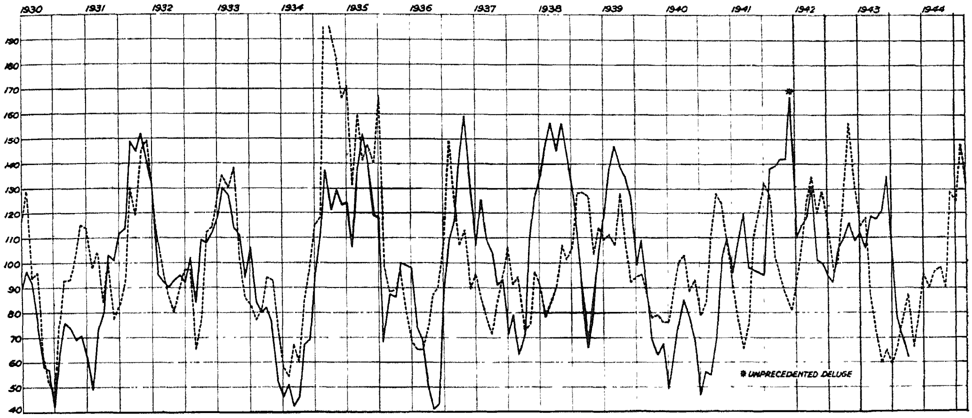

FIG. 3. PRECIPITATION AT PEORIA, ILL., 1930–1944

PREDICTED (DOTTED LINE) FOR FIFTEEN YEARS AND COMPARED WITH ACTUAL PRECIPITATION (SOLID LINE) DURING THAT PERIOD. THE VERTICAL SCALE GIVES PERCENTAGES OF NORMAL PRECIPITATION.

ically and have made predictions for years therefrom. One example is given in Figure 3. On the records of percentages of normal precipitation at Peoria, Ill., for every month from 1856 to 1929, inclusive, used as explained above in the form of 5-month running averages, I based a prediction for the years 1930 to 1944. In Figure 3 the solid line gives the actual event, as far as it was available when the graph was drawn. I feel pleased with the agreement between forecast and event for the years 1930 to 1937. A farmer who had the advantage of a forecast of this accuracy would know very well how to plan his crops. The general effect in 1938 and 1939 indeed is not bad, but in 1938 the features are not well predicted. The years 1939 and 1940 are again fairly satisfactory. Beyond that my prediction seems to get more and more in advance of the event, though both prediction and event show similar general trends upward from 1940 to 1942. It was doubtless too ambitious to make a forecast for 15 years in advance. One would have to be surer of the true lengths of the longer solar periods than our data can as yet show. If a new forecast had been started with 1940, putting forecast and event in coincidence then, possibly the succeeding 8 years might yield as good a fit between forecast and event as the years 1930 to 1937 did. I believe that these very long-range forecasts of temperature and precipitation from the periods of solar variation are well worth more investigation and in some localities will have great value. Such predictions, I regret to say, require many days of computation in each case.

In conclusion, I will draw attention to my earlier remark regarding the great depression of solar radiation in 1922, which was expected to recur in 1945, 23 years later. So very unusual an event in the fluctuations of solar radiation is sure to be accompanied by unusual conditions of weather. From a paper I wrote in 1923 regarding it I quote the following extracts from Weather Bureau statements:

> The record of December, 1922, shows unusual contrasts as to the temperature and precipitation in different parts of the country. . . .
>
> Like the preceding December, January 1923 was notable for the disturbed atmospheric conditions. . . .
>
> The outstanding feature of the weather . . . was the almost continuously high temperature . . . over much of the country. At the same

time, however, severe winter weather was the rule over New England and much of New York.

Precipitation occurred with unusual frequency . . . in northern districts west of the Continental Divide and from the Upper Lakes eastward. . . .

The disturbed atmospheric conditions, so persistent during the first two months of the present winter, continued into February. . . . The pressure distribution for the month as a whole showed marked variations from the conditions usually expected in February. . . .

The unseasonable warmth which had continued during most of the two preceding months of the winter save over the Northeastern States, terminated with the first few days of February, and the remainder of the month was distinctly cold. . . .

I wish I had had sufficient computing help to have analyzed and forecasted for various localities the weather implications of the solar situation expected to recur in 1945 and 1946, but I have been unable to do so. I must therefore content myself with a number of quotations that prove that already very unusual weather has occurred in various parts of the world.

By the United Press, Feb. 9, 1945: Eastern communities [of the United States] struggled from beneath new snowfalls today amid predictions of the worst floods in years on the seacoast and in the Mid West when the drifts begin to thaw.

At least two deaths were caused by the storm in New England, where transportation and communication was snarled. Boston streets were barely passable. Hundreds of war workers were stranded in Providence.

New York City suburbs had one of the heaviest snowfalls of the winter. Eight to 12 inches fell in eastern Massachusetts, Rhode Island, and Connecticut.

Along New England's coast northeast winds whistled at 55 to 65 miles an hour, reaching 75 in gusts.

Only light flurries hit northern and western New York State, where the railroads were winning their battle against snow and ice. Freight was rolling freely out of Buffalo.

Forecasters predicted an end to the storm today and a warm wave tomorrow that may thaw the deep snow and possibly flood ice-choked rivers throughout the Middle Atlantic area.

The Hudson was frozen to a depth of 24 inches in many stretches and the Chenango and Susquehanna rivers threatened trouble in a thaw.

New York City has a sea of slush. The city ordered 7,052 street workers on the job last night. LaGuardia Airport canceled 124 flights yesterday and reported that this winter has been the worst for flying since the terminal was built.

Disaster and relief agencies prepared for the worst flood year since 1936. The Coast Guard and Red Cross kept flood relief equipment ready for instant use.

Snows on the ground in the Ohio Valley, western Pennsylvania and the Connecticut River watershed are "much in excess" of the average for the last 20 years and snow coverage in the "feeder" areas of those districts also is "considerably more" than during the severe floods of 1936, William E. Hiatt, Mid West engineer for the Chicago Weather Bureau, reported.

Most serious threat was to Pittsburgh, where the Allegheny and Monongahela rivers converge to form the Ohio. The snowfall in that area has totaled 110 inches this winter with few thaws.

By the Associated Press: Canberra, Australia, Jan. 15, 1945. More than 2,000,000 wool-producing sheep have perished due to Australia's worst drought since being settled by whites and the country's most productive acres have been turned into huge dust-bowls. . . .

Meat production, it is estimated, will fall off 10 percent; the wheat farmers' income will be reduced by at least $50,000,000, and wool producers will lose an estimated $45,000,000. . . .

One of the gravest results will be that the United States forces in the Pacific will have to secure meat and flour from other sources, perhaps draining their home stocks.

Other such reports of weather conditions that have not been matched for a quarter of a century have appeared frequently in the press within the past two years. I look forward to making an extensive study in the immediate future of the variation of the sun and the changes of weather. I hope that results of a positive and easily verified nature may appear, so that a real contribution to meteorology may follow.

SCIENCE AND SOCIETY

By ERNEST CHERRINGTON, Jr.

THE abrupt conclusion of our war with Japan following closely the atomic blasting of Nagasaki has released a swirling torrent of opinion on the place of the scientist in society and his responsibilities thereto. A few critics have gone so far as to charge the atomic physicists with conspiracy to commit mass murder. Some men of science have retorted that it is not their responsibility that the greatest discovery of modern times has been adapted to purposes of destruction. It is scarcely surprising that such charges and countercharges should be hurled about during the present period of social confusion and intellectual hysteria. A stunned civilization gropes falteringly for means of escape from the fetid pit in which it finds itself after six years of unrestrained warfare. Scapegoats are quickly singled out in times like these, but a dispassionate analysis of the position against science finds it devoid of logic or justice. It is a question of how far indirect responsibility for death is to be traced, even in times of peace. Are the manufacturers of firearms guilty of murdering all who die by the gun? Are the hands of automobile designers stained with the blood of the 34,000 Americans killed each year by their machines? The victim of a traffic fatality is just as dead as the civilian blown to bits by a bomb dropped to destroy an enemy munitions plant, and the homicide is unintentional in both cases.

A less extreme point of view has been expressed by sociologist Joseph Schneider writing in THE SCIENTIFIC MONTHLY (November 1945). He holds that the unparalleled magnitude of destruction wrought by World War II is "of the scientist's own making." "There was a time," he continues, "when men of mind expressed only contempt for the vainglorious show of politics and war. But today the man of science has become a hireling, a willing subject in the service of the nation state; an indefatigable combatant in the righteous cause of a finite warrior god." It is implied that scientists have perpetrated a heinous crime against society to atone for which it now behooves them to get busy on useful discoveries and satisfy "the simplest needs of men." Professor Schneider is somewhat vague as to just whom he has charged with responsibility for creating the chaos under the accusation of being a scientist. If we adopt the broad definition of astronomer Harlow Shapley who writes that "we are nearly all scientists," then perhaps the indictment is general enough to include the guilty. The term "scientist" might be stretched to embrace those barons of international commerce who have fattened on the exploitation of colonial peoples and the control of raw materials, together with their colleagues, the politicians, who have created artificial trade barriers and have legalized a most inequitable distribution of the natural resources of the earth. In fixing the blame for the ravages of war let us not vent our wrath on the men who helped repel and defeat our attackers. Let us rather seek out those who caused the economic and social injustices that led to the slaughter.

Since to most of us the word scientist connotes an erudite individual concentrating on experiments in a secluded laboratory, let us now limit the term to include only those whose principal occupation is research or teaching in one of the branches of natural science.

The scientist too has raised his voice from time to time in regard to his position in the war effort. The declining Ph.D. rate of the past five years has been viewed with alarm, and the dire consequences of the present scarcity of technical specialists have been emphasized and re-emphasized. The induction of young scientists into the armed forces has been decried as shortsighted nationalistic planning. The implication has been that all those who enjoy the privilege of postgraduate scientific study are to become the future benefactors of mankind, and that they should, therefore, have been exempted from the draft. If the argument is valid, its application is broad. It should likewise have applied to those young men who were preparing for careers in education, literature, art, law, public administration, agriculture, industrial management—in short, any calling which in some way serves the needs of humanity. If it be granted that any one group of young men should be exempted from military service on grounds of exceptional value to society, the logical conclusion of such reasoning is that only the unintelligent and those who have been denied the opportunity of specialized education should be called upon to fight for our country in the hour of attack. In other words, those who are likely to benefit least from the American way of life should be detailed to defend it for the rest of us. Such a policy of selective service would be not only the antithesis of the democratic principle; it would be suicidal as a means of national security.

A great many scientists of varying ages and degrees of proficiency have devoted the past few years to war research under the direction of the Office of Scientific Research and Development. Almost all of these men and women have undertaken such work with the sincere desire to aid their country to the best of their ability in its time of need. A few were lured from the paths of pure science by the enticement of higher salary, and a few hurried into war research with the local draft board in close pursuit. But regardless of which entrance they used, once inside they have worked hard and have done a splendid job. Had the scientists of America resolved to hold themselves aloof from the revolting butchery of modern warfare, a course of action indicated by Professor Schneider as appropriate for "men of mind," one can only guess how many years longer World War II would have dragged on and how many hundreds of thousands of additional American lives it would have consumed. In that eventuality, how would the scientist's crime of omission have compared with his present alleged crime of commission?

The overwhelming majority of professional men have served our nation during the years of war emergency either as civilians or as members of the armed forces with no more complaint than is heard from the average soldier regarding his lot. However, attention to war research at the expense of the normal pursuit of pure science has been to a few scientists a cross they have borne noisily. Several articles have appeared in recent months lamenting the wasted years of brilliant specialists forced to develop military gadgets when they might have used their time and talent to discover new principles in the realm of abstract investigation. The situation is indeed regrettable, but, after all, who has not found his life and his purpose adversely affected by the war?

The scientist has usually been well-paid for his military investigations. He has usually worked in comfortable laboratories where every facility has been provided. He has usually been able to spend his spare time each day in the company of his family. Consider by comparison the young men of the armed forces snatched from their homes, torn

from new enterprises well begun, and shipped overseas to fight, to suffer, and often to die in a hideous maelstrom of destruction, separated by thousands of miles and years of time from their loved ones. Has the scientist been asked to make too great a sacrifice at a time when our nation faced disaster? Even one of the exponents of the "wasted-years" hypothesis grudgingly concedes that not quite all the work in the laboratory during the past five years has been fundamentally useless. Physicist I. I. Rabi, writing in the *Atlantic Monthly,* admits that "our advance in pure science, when we get back to it, may be greatly accelerated by the use of new techniques developed during the war, if," he adds gloomily, "those whose business it is to supply the funds will stand the expense and not insist upon calling the tune."

When the heat has gone out of the argument, and the smoke of the salvos has drifted away, probably it will be agreed that the role of the scientist in World War II has made of him neither a criminal nor a martyr. He had a job to do and he did it well. Never before in its history has science mustered so large an army of research experts dedicated to the satisfaction of the immediate, practical need of humanity. Radar, the variable timing fuse, the terrible atomic bomb, and thousands of other less spectacular devices were developed by our scientists and used with great effect by our armed forces to drive back and defeat a treacherous, fanatical, and inhuman enemy whose sworn purpose was the obliteration of the democratic way of life. With such a magnificent record of service to humanity just completed, why is it that certain intellectual and spiritual leaders rise to denounce the scientist in bitter terms? Why, also, do some men of science feel that they have wasted their time? Is humanity ungrateful toward those who have contributed so liberally to her salvation? Do the latter question the value of saving the former? Such questions lead us below the surface issues to an unwholesome situation that has developed gradually over a long period of time. The sensational and violent events of World War II have not created the condition but only accentuated it. The fact of the matter is that there exists today a broad chasm between the professional scientist on the one hand and his fellow-men on the other. Long before the war began this chasm had widened to the stage of misunderstanding. During the chaotic years of war its jagged edges have receded to embrace both distrust and fear.

Just as the Grand Canyon of the Colorado River may be traced upstream to a small crevice high in the Rocky Mountains, the present yawning abyss between science and society may be traced back through years of progress to a tiny fissure. That fissure is marked by the concept of atomic structure. So long as the atom remained but an infinitesimal billiard ball, chemistry, physics, and astronomy were studies any intelligent person could explore thoroughly and understand with a reasonable amount of intellectual effort, either as a student in the classroom or as an amateur scholar in his hours of leisure. The entire accumulation of knowledge could be treated adequately in a small volume for each subject. Scientists had time to write such books, and laymen had time to read them. With the breakdown of the atom, the scientist parted company with his fellow-men. Since then he has pursued a lonely course farther and farther into the realm of abstraction on the other side of the widening canyon. His life has become so filled with technicalities that he has scarcely had time to be a human being. The paths of investigation opened up have been limitless, and an ever increasing number of technicians have joined the chase, each pressing on

breathlessly lest his colleagues beat him to the nebulous treasures of knowledge that lie somewhere ahead. Each has had to devote six, eight, or more years of his life to intensive specialized study in order to skim over the important discoveries and the endless minutiae which have already been added to the total of information in his special branch of a general division of the science he has chosen. Having at last reached the frontier, he plunges into the underbrush and loses thereafter all contact with society.

ALMOST every intelligent, civilized person has an inherent interest in things scientific. This desire for knowledge of how things work has brought him again and again to the scientist requesting instruction. It had become all too common in the years preceding the war for the scientist rudely to brush aside such requests with the excuse that he was too busy to explain the matter or that the subject was too technical to be understood by the nonspecialist. Thus science has turned away from mankind and has forged upward, building a vast Tower of Babel in the shape of an inverted pyramid, the main entrance of which was marked years ago with a conspicuous sign "For Members Only." From its small foundation consisting of the subatomic concept, the strange structure has towered aloft into the clouds themselves, from which its high priests have recently cast down the thunderbolt of subatomic energy. Is it surprising, then, that intelligent members of society, forced to remain in ignorance of what has gone on in the awful temple of science, have risen to demand that the scientist give an accounting of what he has done and of what he proposes to do about the terrible forces he has unleashed?

Some will assert that this picture is unfair. Reference will be made to the books, the magazine articles, and the popular lectures on scientific subjects that have been presented continuously and in large numbers during the years of peace. True, there has been considerable popularization of science, but who has performed it and of what has it consisted? In answer to the first question, it will be found that almost all of the material released for public consumption has been prepared by interpreters who belong to one of two groups: first, the outstanding scientists who fully recognize their responsibility to society; and, second, the science news writers, whose profession it is to storm the walls of the citadel and bring out to the public descriptions of what they have seen inside. As to the content of the reports to the layman, a great deal of it has been old, pre-atom stuff. The intelligent nonspecialist has been patronized and given only what it was thought he could understand. Let the skeptical reader ask himself how many scientists of his own acquaintance devote even a small portion of their time to reporting the progress of their current research to their fellow-men.

Most of the leaders of science today are doing their best to remedy this evil, but unfortunately their number is small compared with the rank and file of professional scientists. Far too many of the latter live out their lives at public expense and contribute little or nothing to the welfare or intellectual development of society. These individuals recognize, perhaps unconsciously, that they are little better than parasites on the human race and, refusing to admit the fact even to themselves, they subconsciously set up a defense mechanism consisting of an attitude of superiority. As intellectual snobs they retire permanently to ivory laboratories and observatories where they continue their investigations of minutiae, emerging only occasionally to flay for their nig-

gardliness those on whose gratuities they exist. Of course they report the results of their research, but all their papers are written in equations and technical jargon. They publish exclusively in professional journals, which reach only their fellow-specialists and the shelves of the larger libraries. It often happens that much of such a publication consists of material previously printed in foreign journals or even more obscure periodicals—old data regrouped and rehashed with just enough new measurements added to change the conclusions of the original writer. Among these gentlemen of the self-appointed intellectual hierarchy there are two schools of thought as to what constitutes the highest evidence of scholarship. One contends that it is the number of titles on his list of published papers while the other argues that it is the total number of printed pages that really counts.

Let it be reiterated that the above description is not intended to be a caricature of all scientists or even of the typical scientist. Nevertheless it applies to an important minority of the class—important not so much for their number as for the damage the cause of science suffers at their hands. It was perhaps not Confucius who said that one dead horse may command greater public attention than thirteen live ones, but the illustration is apropos.

It has never been the custom of society to support a privileged class of individuals from which it receives little or nothing in return when that galling course of action could be avoided. During the past two centuries the people of the civilized nations have eliminated one such class after another. Does the professional scientist face the possibility of a similar fate as a result of the antisocial attitude of a group of his colleagues? Perhaps not, but the seriousness of the situation merits the deep concern of the more responsible members of the profession. "If those whose business it is to furnish the funds will stand the expense and not insist upon calling the tune"—that phrase frankly expresses the feeling of many scientists toward their patrons. Why is it someone's business to furnish the funds regardless of value received? We live today in a hard, business world, and if the man on the street ceases to produce either articles or services that society can use, he soon finds himself without income. Why should the scientist be an exception to this rule? Spoiled by the apparently limitless funds the federal government poured out for war research, many scientists will return to their former positions with demands for new facilities far in excess of any they dared hope for in years before the war. Research foundations both large and small will probably call upon their sponsors for increased appropriations at a time when the enthusiasm of society for scientific investigation seems to have reached its lowest ebb. The outcome looks rather obvious unless the scientist takes steps at once to regain the respect and confidence of his fellow-men.

Professor Rabi argues vigorously that science can advance only when research is carried out on a high plane of pure theory and experimentation with no thought of deriving results of practical value to humanity. One certainly need not search far to find numerous outstanding contradictions to such a view. Was not the foundation of chemistry laid by the ancient alchemists whose motives were the highly practical ones of making gold from cheaper metals and of discovering the elixir of life? A number of the fundamental laws of astronomy were discovered by medieval observers who were seeking astrological relationships which they thought would enable them to forecast the future—a most practical, if futile, purpose. Even today the pure-motive hypothesis is

questionable. An analysis of the current technical literature of chemistry and physics will show that a great many important principles and fundamental relationships have been discovered by scientists working in industrial laboratories who began with general practical ends in mind and who climaxed their investigations with the development of new and valuable industrial materials and commercial devices. It is certainly not my contention that scientists must invent practical gadgets in order to benefit mankind. The point to be made is that science should be able to live and flourish outside the artificial atmosphere of unlimited financial resources and unlimited social irresponsibility. If genius is so nearly dead that it can exist only in an oxygen tent, we can scarcely expect to postpone the funeral much longer.

There are two ways in which the professional scientist can serve humanity. The first method is by the development of "better things for better living." The second method is by contributing to the education of society as a whole in order that the lives of men may be enriched through understanding of an appreciation for the physical universe and all it contains—from stellar galaxies down to subatomic configurations. Path number one, the focus of the present controversy, has been discussed sufficiently. The second path may be pursued either by enthusiastic teaching in an educational institution or by popular writing and public lecturing on current research. The scientist who refuses to follow one or the other course of action is unworthy of his salt.

It is time that each man of science sat down at his desk and examined the book of his account with society. The debit side of the ledger will be crowded with entries. There he will find everything he knows—the entire cost of his general and specialized education—all he has learned from the experience of the past and everything he has discovered in his own investigations. There he will also find his position in the community, his home, every comfort he enjoys, everything he owns, and every facility to which he has access. But what about the credit side of the ledger? If a teacher, does he make an honest effort to disseminate effectively his knowledge among his fellow-men, or does he regard teaching as an odious task necessary to secure his pay check? Does he report his researches to the public in understandable language, or does he write exclusively for technical journals of small circulation and lecture only before meetings of his professional society? Does he graciously admit interested laymen to his place of research, or does he keep the door locked in order not to be bothered? Does he try to explain what goes on at the frontier of his science, or does he save himself the trouble by assuming that the average man is too stupid to understand such matters? Does he improvise and use to the utmost the research facilities available to him, or does he sulk in inactivity complaining that his apparatus is antiquated and useless? When he needs new equipment does he go out and really sell his program, or does he curtly demand that "those whose business it is to supply the funds" do their duty? Is his research enlightening and inspiring, or has it degenerated into the humdrum process of assembling, sorting, and cataloguing minutiae?

It is through frank answers to questions such as these that the scientist may trace the origin of the storm of public criticism that swirls about his laboratory today. Let him heed well the fact that the storm is of his own brewing. Let him revise his program better to meet the needs of the postwar world. Let him do so at once before a disillusioned society decides the matter for him and again demands that science take a holiday—this time a long one.

THE STRANGE TRINITY CALLED MAN

By A. BOYAJIAN

THAT human nature is a highly complex mixture of good and bad is a very old story, but that it is a trinity—a genuine three-persons-in-one trinity—is a discovery of psychoanalysis.

The fundamental difference between conventional psychology and psychoanalysis is this: while the former has always concerned itself with the consciousness, the latter has recognized that human personality is like an iceberg. A small portion of it is exposed to view above the surface, in the conscious mind; the bulk of it is concealed from view below the surface, in the subconscious, or in the depths of the unconscious. All strange behavior, even insane behavior, becomes explicable when its motivation is traced into these lower levels. The differentiation of the three partners in the trinity is visible enough above the surface of consciousness after it is pointed out, but it is particularly conspicuous at levels below the surface of the consciousness when these depths are probed by psychoanalytic methods.

The so-called "unconscious" is not unconscious by any means; it is not a blind machine. It knows full well what it wants, and makes plans for it sometimes more shrewdly than the conscious mind: it is unconscious only in the sense that our waking consciousness is not directly aware of all that goes on in it.

The popular misconception of psychoanalysis as the psychology of sex originates in the fact that sex may play a more conspicuous role in the unconscious than in the conscious mind, and therefore it occupies a more prominent place in the psychoanalytic literature. The primary object of study of psychoanalysis, however, is the unconscious—the study of sex is only incidental.

When the three persons of the trinity function harmoniously, they may appear as different faculties or tendencies of the same person, and the casual observer may dismiss the theory of three persons in one as pedantry. However, when the three begin to work at cross-purposes, especially when discord breaks out among them so that one persecutes another, or worse yet, when one murders another (incidentally killing all three), there cannot be much doubt left as to whether human personality is more significantly interpreted as a trinity or merely as one person having a number of faculties and tendencies.

That there has been some recognition of multiplicity in human nature is not to be denied. Who has not heard of the "body-mind-and-soul" theory of man? More often, however, human nature has been pronounced duplex. St. Paul was emphatic on the distinction between the "carnal" and the "spiritual" man. The "Dr.-Jekyll-and-Mr.-Hyde" picture is classic. While these contain much truth, they are very inadequate. They are not only fragmentary but also unscientific in conception and erroneous in some of their important implications. For this very reason, it has been necessary to give new names to the three persons distinguished by psychoanalysis, so as to avoid the notions that cling to old words.

Let us identify the three personalities briefly and then review some of their famous conflicts in history and fiction.

First comes the *Ego*, a well-enough known fellow. He is the personification of reason, and the nominal head and mouthpiece of the trinity. As such, he generally poses and passes for the whole personality. Our "company manners"

are largely those of the Ego. His major attributes are common sense, prudence, and expedience. In some respects, the Ego is the most decent member of the trinity, but he is also the weakest and is easily corrupted by the others. He counsels his partners, furnishes them expert knowledge, and acts as their executive; but frequently the others fool him or simply compel him to carry out their dictates against his own better judgment. He writes scientific treatises, develops philosophical systems, makes inventions, and in matters of abstract truth, such as mathematics, he talks with an absolute authority as if he were the voice of God. And yet, the amount of stupidity the Ego has advocated through the centuries is simply astounding. And in spite of all his talents, the Ego plays only a minor role in art and religion.

The psychoanalytic name of the second member of the trinity is *Id.* This name may sound Chinese, but the Id is *us* more intimately than the other two members of the trinity, and far more important than they because he represents the *life instincts,* or *vital drives,* in us. Although these instincts, or drives, are many, they are so well united in the long evolutionary process that in the normal person they do not ordinarily exhibit cleavage into separate personalities. The Id likes sex and is libidinous, but he is so only because he is the source of all emotion, and hence of love in all its manifestations, and also of much hate.

The Id is the creative member of the trinity. He surely is more than animal, as he is the fountainhead of all artistic inspiration. The Ego can write verse, but only the Id can put into it the spirit that elevates verse to the rank of poetry. The Ego can furnish correct harmony (after learning it from the Id), but only the Id can furnish an enduring melody. The Ego can draw and color, but only the Id can furnish the ingredient that makes the difference between a colored drawing and an artistic painting. The Ego can write excellent history, but only the Id can write good fiction. Our gentlest emotions spring from the Id; and the tender grace of parenthood is his exclusive gift. The Ego strains his intellect and calls God "the Infinite," which means "bigger than you can imagine"—a sorry compliment. Intuitively, the Id calls him "Father" and infuses human life with divinity.

The Ego's workshop is in the cortex, that of the Id extends down to the thalamus, and therefore much of the latter's activity is below the surface of consciousness. Thus, when communications reach the Ego from the Id, they may appear as sudden inspirations.

In the unconscious, the Id preoccupies himself particularly with those interests of his that are repressed by the other members of the trinity. Sex is perhaps the most important one of those interests, but there are others, many of them arising from infantile and childhood experiences. The manifestations of the subconscious workings of sexual energy so impressed Freud that he christened them the Libido. So, in the early days of the new science, the human personality was double (even though not officially so stated)—the Ego and the Libido. Some psychoanalysts now use the word Libido more broadly as synonymous with Id. In this article it will be so used because it is a more familiar word, with more color and potential—although a certain amount of odium also attaches to it, from which the colorless word Id is free.

The Id, or Libido, is the dynamo of the trinity: he furnishes both objectives and energy to the Ego; he is perpetually bent on achieving some desired goal and would destroy anything that stands in his way, except for the restraining hands of the other two persons of the trinity—the rational Ego and the third person.

And who is this third person?

Conscience is his popular name, *Super-*

ego his psychoanalytic name. That conscience speaks to us as if it were the voice of another person—perhaps as the voice of God—will be readily granted, and to that extent at least, the multiplicity of human personality may be conceded. The fundamental difference between the psychoanalytic concept of the Superego and the popular notion of conscience is that the Superego is larger than the conscience and includes it. Conscience is the Superego as the latter appears in consciousness, but the Superego works also in the unconscious, as the *conscience of the unconscious,* and does some of its most important work there. Of the latter, we are not directly aware, but we have a great deal of indirect positive knowledge of it.

Conscience is sometimes thought of as primarily interested in religion, but the Supergo is seen to enforce all tribal and social taboos, political loyalties, the dictates of fashion, the codes of professional conduct, the amenities of the club, the rules of the shop, the rules of sport, the traffic regulations, and even the unwritten codes of the gangsters. At the subconscious level, it keeps guard over the Libido and the bodily functions, even when the Ego sleeps. For instance, it guards the sphincter muscle to keep the bedding dry and clean; keeps dreams within bounds; and wakes us up in the morning at the right time.

Censorship of the Libido is, of course, also one of the functions of the Ego. Perhaps because routine repetitive censorship would be an unnecessary burden on the Ego, this duty has evolved into a separate function delegated to the Superego. As this type of work does not require much intelligence, the Superego is a dullard compared with his partners. He is easily won over by all kinds of established authority and is *the great ally of conservatism,* although he also spurs an occasional crusader. The very fact that it is the function of the Superego to *check* the Libido carries with it the danger that a hypertrophied, or otherwise malfunctioning Superego, may tend to *choke* the Libido.

Human personality, then, consists of these three persons: Ego, Superego, and Libido; or, in the more familiar terms, Reason, Conscience, and Desire. In the normal individual, they so complement and balance each other that the resultant is an apparently single personality with a certain characteristic complexion, depending on their proportionate strength.

Under the influence of alcohol, the equilibrium within the personalities of our trinity is altered, and hence the complexion of our composite personality changes. The Ego and Superego are dulled more than the Libido, and therefore the latter gets freer rein. Thus his joy in living and loving comes to the fore more conspicuously. While it is generally recognized that alcohol "releases tensions," it is not so generally understood that these "tensions" are conflicts within the trinity and are eased by dulling the *censorious* partners and "releasing" the Libido.

At night the Ego sleeps much more deeply than the other two, and the complexion of our sleeping personality is considerably different from that of our waking one. This shows itself clearly in dreams. The Libido takes advantage of the drowsy Ego and enacts its hopes, wishes, and fears in dramatic scenes on the stage of the lower levels of consciousness. The Ego takes these dreams as reality because he knows he is not imagining them and, being drowsy, he does not suspect that the Libido may be concocting them. In every dream there are three real persons, namely, the partners in the trinity. All others are puppets, mostly the creations of the Libido. Concealed in the subconscious, the Libido pulls the strings and speaks for the puppets as a master-ventriloquist. The Ego watches as an innocent bystander or fre-

quently enters into the play, like a spectator walking up to a magician's platform. As the Libido is rigidly controlled during waking hours, now it would like to do a lot of wishful thinking without arousing its censorious partners. To accomplish this, it resorts to symbolism, and that makes dreams seem fantastic; but dreams are no more so than code messages. When decoded, both make sense, sometimes fearful sense. Thus, dreams provide a clue as to what is going on in the subconscious, and that is why the psychoanalyst is very much interested in them. Many a symbolic murder is committed by the Libido in dreams, without the knowledge of the Ego or the Superego. The person remembers the dream but does not understand it and may not believe the interpretation of the psychoanalyst. Conflicts in the unconscious, between the Superego and the Libido, lead to symbolic nightmares, although we may well grant a nightmare may also be caused by a physiological discomfort. Even in the latter case, it is possible that the physiological discomfort initiates the mental conflict. ✓

The Libido uses skillful camouflage to dodge the censorship of the Ego and Superego, even in our waking hours. At the New York World's Fair, millions of men and women innocently gazed at and admired the "theme" of the Fair—the Trylon and Perisphere—while their unconscious drank in the lightly disguised phallic symbolism. We may grant that the architect's Ego also was innocent in this matter. He says the Trylon was the logical answer to the requirement of visibility, and the Perisphere was needed for an interior display of the nighttime view of the heavens and the countryside. How clever of the Libido, and how naive and innocent of the Ego! The architect never explained why the Trylon and the Perisphere had to be adjacent to each other; what need there was for such deceptive words as Trylon and Perisphere; and why no elevator was ever placed in the Trylon to enable visitors to get an adequate view of the Fair. Artists put things in their paintings unaware, and it is common knowledge that composers do not always understand the music welling up from the unconscious. So they call their compositions "Symphony No. 1," "Concerto No. 2," etc. It was some time after the composition of his *Fifth Symphony* that Beethoven interpreted it as "Thus Fate Knocks at the Door." In daily life, much of our motivation is unconscious—from either the Libido or the Superego—and our "reasons" for our behavior frequently are only rationalizations.

The Superego not only restrains but may also punish. "He that diggeth a pit shall fall into it," says the Good Book, and centuries of human experience show that it is true. Who or what could be the agency for such divine justice? Why look beyond the Superego? While the Libido and the Ego have turned a deaf ear to him and are digging a pit for the innocent, the Superego is muttering, "Wait and see if I don't push you two into that pit when you're unwary."

Just as Reason's function is to be an unerring guide to truth (though he is frequently guilty of much stupidity), so Conscience's function is to be an unerring guide to ideal conduct (though he is frequently guilty of much crime). Often, too, he is neither the voice of God nor of reason but of some tradition, prejudice, or some other false authority. Right or wrong, he acts as judge, jury, and executioner and punishes disobedience, sometimes with much cruelty. He is impartial, too, punishing his partners sometimes worse than others. The horrors of the inquisitions perpetrated by cruel Superegoes are well known. Witness the following letter written by the minister of a Boston church in the year of our Lord 1682:

TO YE AGED AND BELOVED, MR. JOHN HIGGINSON:

There be now at sea a ship called Welcome, which has on board 100 or more of the heretics and malignants called Quakers, with W. Penn, who is the chief scamp, at the head of them. The General Court has accordingly given sacred orders to Master Malachi Huscott, of the brig Porpoise, to waylay the said Welcome slyly as near the Cape of Cod as may be, and make captive the said Penn and his ungodly crew, so that the Lord may be glorified and not mocked on the soil of this new country with the heathen worship of these people. Much spoil can be made of selling the whole lot to Barbadoes, where slaves fetch good prices in rum and sugar, and we shall not only do the Lord great good by punishing the wickeds, but we shall make great good for His Minister.

Yours in the bowels of Christ,

COTTON MATHER

If such a Superego should turn against its partners, would it hesitate at killing them "so that the Lord may be glorified"? The ascetic self-tortures of the early centuries represent a cruel Superego torturing the Libido with the assent of a mistaken Ego, based on a misunderstanding of the gospel of Jesus Christ. Such asceticism is nearly extinct now, but other forms of masochism are in style.

Let us consider now some classical cases of murderous conflict within this unholy trinity. For the purposes of this article we shall define as *true suicide* only those cases of self-destruction in which the three persons of the trinity agree that death is better than life, but as *murder* those cases resulting from a conflict among them. Sometimes we can tell which of the trinity killed which, with malice aforethought, with full knowledge that killing the other will be the end of all three, and reconciled to it: a suicidal murder, but murder just the same. Although the end effect is the same in both cases—death for the trinity—yet in the first case the motive is suicide, in the second, murder.

It is a surprising conclusion of some psychoanalysts that true suicides are rare. Frequently, what appears as a suicide in the coroner's report is murder within the trinity. For instance:

In Victor Hugo's immortal novel, *Les Misérables,* we see the police officer Javert standing on the brink of the river Seine in Paris, in the dead of the night. He plunges in and is drowned.

Suicide or murder?

Murder of an implacable Superego by a Libido and Ego in desperation. Javert's conscience, as the agent of the law, was making life miserable for Jean Valjean, with whom Javert's Ego and Libido sympathized and suffered and revolted. Death alone would silence that unreasonable conscience of his, so death was dealt to him.

That story features also conflict within the personality of the hero—Jean Valjean—his Superego hounding him all through the story, demanding surrender to the law with great humiliation to his Ego and great privation to his Id, and these two resisting the Superego successfully as long as no one else is concerned. But the moment the interests of another person are endangered, the Superego is aroused to greater effort and obtains the surrender of the other two. When Jean Valjean's son-in-law learned of his painful past (and present) and offered to secure pardon for him through political influence, he answered that what he needed was pardon from his conscience! But that could not be secured through political influence and was not secured even by all his good deeds, and so his whole life was one of agony and martyrdom to his conscience.

The story of Dr. Jekyll and Mr. Hyde ends with what the coroner would call suicide. The screen version recognized murder in it and ended with words like "Mr. Hyde has killed Dr. Jekyll." Undoubtedly, Hyde's scandalous conduct led to the tragic end, and therefore the Libido may be said to have killed the Ego. But what is lacking is explicit recognition of the trinity, of the part played by the Superego. In the long-drawn-out conflict between the Superego

and the Libido, the latter had the upper hand until the situation arose in which life's appeal to the Libido was greatly weakened. The Superego, now more infuriated than ever, ordered the Ego to kill and punish the Libido. The order was carried out because the Libido was now too indifferent to life to offer serious resistance.

A fourteen-year old apprentice of a cobbler was very much impressed by an adult conversation about a certain customer having been blinded for blasphemy. Soon after that, his eyes began to bother him, and an oculist advised him to change his occupation to something less exacting to the eyes. His eyes, however, steadily grew worse, and on occasions his retina would bleed. Oculists were unable to help, and the boy (by this time a man) consulted the psychiatrist Georg Groddeck, telling him his story. For brevity, the diagnosis and subsequent events are condensed here into a short dialogue.

Doctor. How do you account for the bleeding of your eyes?

Patient. It happens invariably in the fall, in October, and also at other irregular times. In the fall, nature is dying, and I feel sad. I cannot account for the other times, or the present case except that my little girl accidently hit me in the eye.

D. Did anything serious happen to you in the fall?

P. No.

D. Give me a number. Any number.

P. Eight.

D. Did anything serious happen to you when you were eight?

P. No.

(Previous reference to the blind man came to the doctor's mind.)

D. Did you ever blaspheme God?

P. (laughing.) I used to be a pious child but for many years I have not gone to church. I don't believe in those things any more.

Suddenly the man grew pale, and fainted away. When he came to himself again, he broke down in weeping confession.

P. Doctor, you are right. I am a blasphemer. It was in October, I was eight years old, and with a bunch of other boys, I was throwing stones at a border crucifix. My stone hit the mark, and the image of Christ fell down and broke into pieces. We fled, terrified.

D. When did this last bleeding start?

P. Yesterday, at five, while I was waiting for a car at a certain corner.

D. Go back there now, look all around, and report to me what you see.

The patient returned with a statement that he saw a large crucifix!

The retinal hemorrhage was punishment administered by an ill-advised superego, aroused by the sight of the crucifix. With the reeducation of the patient's mind, his eyes steadily improved, and in time he secured and held a job as a bookkeeper, a most severe task for eyes.

Consider St. Paul's case. His stern Superego, energetic Libido, and mistaken Ego conspired to persecute the Christians. He helped stone Stephen, and drove others into dark dungeons. He probably threatened also to blind them so they would be eternally in a dark dungeon. But as his contact with the Christians grew, his Ego saw that these were not bad people, that it was a crime to persecute them. The Superego then relented, but the Libido continued the persecution. On the way to Damascus, the Superego revolted against the Libido and turned on it with a terrific thrust: It's you that deserves to be blinded, not those Christians, and you're going to get it! The subconscious explosion sounded like thunder and lightning to his Ego. He was knocked down blind and helpless. "Saul, Saul, . . . it is hard for thee to kick against the goads [of thy conscience]." When Ananias gave him the authoritative word of forgiveness for his sin, Paul's Superego relented, and his Libido regained his sight.

The Master-Healer knew intuitively the need of many of his patients and applied the correct remedy to them, saying, though not to all, "Son, thy sins are forgiven thee." These people went home happy, not only because their physical ailments had been relieved, but also because they felt a joy and peace in their hearts transcending their understanding. The magic words of the Master had relieved the unconscious conflicts gnawing at their vitals.

The flowering of Christian love in

Paul's Libido never quite succeeded in completely humanizing his intolerant Conscience. Listen to him write to the Galatians, who have had a doctrinal row: "This simply means that certain individuals are unsettling you; they want to distort the Gospel of Christ. Now even though it were myself or some angel from heaven, whoever preaches a gospel that contradicts the gospel I preached to you, God's curse be on him! I have said it before and I now repeat it; God's curse be on him!" (Gal. 1:7–9) Compare this with the spirit of the Master: ". . . they went, and entered into a village of the Samaritans . . . and they [the Samaritans] did not receive him. . . . And when his disciples, James and John, saw this, they said, Lord, wilt thou that we command fire to come down from heaven, and consume them, even as Elias did? But he turned and rebuked them, and said, Ye know not what manner of spirit ye are of." (Luke 9:52–55).

Paul's theology also was profoundly influenced by his stern Superego. The following words of James probably represented also the temper of Paul's conscience: "For whoever shall keep the whole law, and yet stumble in one point, he is become guilty of all." (That is, if you pass a red traffic light, you thereby become guilty under all the provisions of the penal code.) Such a stern conscience could not be appeased with anything less than the most precious blood, and Paul had the inspiration to offer the blood of Jesus Christ in his defense. That was one of the most momentous events in history—for ages to come, humanity suffering at heart as Paul did will find salvation and joy as he did.

Like astronomy, psychoanalysis is neither religious nor irreligious: it only reveals the mechanism through which man, God, or the Devil may work. As an example of the Devil's work, the Ammonites sacrificed their first-born to Moloch. Whom were they appeasing? A stone statue? More likely, the Moloch of a Superego in their unconscious. Superstition? Of course, but that only because in all ages people have fashioned their gods in their own image. The Hebrews solved the conflict in the human personality by a token sacrifice of sex in the ritual of circumcision, supplemented by the shedding of the blood of an animal.

Although generally the conflict is between the Superego and the Libido, sometimes it is between the Superego and the Ego. Here is an example that recently appeared in the papers:

MINISTER'S SON KILLS SELF

A minister's 16-year-old son, who left notes indicating that he debated with himself questions of belief and nonbelief, was found dead in a L———— College chemical laboratory today. Sixteen gas jets in the room located in Gayley Hall were open, according to police, who said a note had been fastened to the laboratory door, warning other students against entering because of the fumes. The boy was J———— B. R————, 16, a freshman, whose father is the Rev. M———— R————, pastor of C———— Presbyterian Church, Staten Island, N. Y. R———— was on the dean's list and was a member of the college choir.

Although death from conflict between theological dogma and reason are unusual, milder cases of such conflict are quite common among scientific people brought up in fundamentalist families. A *modus vivendi* is generally established by going to church on Sunday, to the laboratory on Monday, and by never discussing doctrinal matters or even religion.

Violently murderous conflict within the human personality is of course rare, but a greater amount of harm is done by the less spectacular cases because they are so prevalent.

An oversimplified picture of the development of an internal conflict between the Superego and the Libido in the adult may be described as follows: An antagonistic social environment, more powerful than the individual, arouses aggressive impulses in the Id, and these are repressed jointly by the Ego and the

Superego, the former counseling prudence, the latter demanding obedience to authority. The repressed resentment festers in the subconscious into more seriously aggressive impulses, and this excites the Superego into more drastic controls against the Id. Thus, tension begins to build up and may explode sometime. Surely, "Blessed are the meek." Blessed are also those who are not exactly meek but have mastered the art of diverting into constructive activities those pugnacious energies that may be mobilized in them by external opposition.

Human unhappiness arising from internal conflict is deeper than that from external conflict, though the two react on each other; and the establishment of internal peace may be a prerequisite to the establishment of a durable world peace. If we could but see the internal conflicts of men like Hitler engaged in creating a world conflict! About the general causes of war, perhaps the ostensible ones, like the Versailles Treaty, etc., are merely excuses, the real causes being the conflicts within the personalities of the leaders. James said many centuries ago: "Whence come wars and whence come fightings among you? Come they not hence, even of your passions, that war in your members?"

In *The Art of Happiness,* John Cowper Powys says:

> I myself have discovered, from examining the behavior of my own mind, that there is a cruel demon in it that derives sadistic pleasure from trying to force me to think of the very things that especially make me shudder. And the happier I feel, . . . , the more energetically does this demon under my own helmet call to my attention what I particularly loathe to think about.

That there is in all of us a will-to-life is universally recognized, and self-preservation is said to be the first law of nature. Some philosophers have also observed that in all of us there is also a will-to-death, as if self-destruction might be a second law of nature. This is stated very effectively by Dorothea Brande in *Wake Up and Live:* "With the time and energy we spend in making failure a certainty, we might have certain success. . . . *It takes energy to fail.* . . . Then why do we *work hard* at failure? Because, besides being creatures subject to the Will-to-Live and the Will-to-Power, we are driven by another will, the Will-to-Fail, or Die."

This condition may be responsible for certain slow, unconscious suicides, producing either organic or functional disorders. People who do not spare themselves, those who take unnecessary chances, may well be suspected as ailing with a mild case of will-to-death.

If this is true, it must have a profound biological significance. It has: when functioning properly, it is an instrumentality for *race*-preservation as against *self*-preservation. This is evident at the lowest level of the unconscious in the senile degeneration by which the body eventually commits suicide. It is evident also at the highest level of consciousness in the role of the conscience as the champion of the interests of the race at the expense of the individual harboring him! Although psychiatrists are not unanimous, I am convinced that the Superego is present in the lower animals also, as an instinctive conscience guarding the safety of the herd. Trinitarian psychology explains nicely how the Superego gone wrong may torture the individual with no benefit to the race.

As the subconscious is a master of disguise, the will-to-death may take forms that will fool all but the experienced psychiatrist. Here is an agitator talking about his superior race and blood, his unconquerable will, and his irresistible determination to conquer the world or go down in the attempt, pulling down the rest of the world with him. Is he driven by the will-to-life-and-power? Or is not such reckless gambling with life sufficient evidence in itself that the

man is driven by the will-to-death, disguised as will-to-power, so as to evade whatever conservative controls may have been left in his personality? When one reads about the kind of will-to-power that Nietzsche visualized as the highest good and just what he would do with it after he got the power, one cannot help but conclude that he was driven by a disguised, insane will-to-spectacular-death. Many people in Germany seem to be afflicted with such a neurosis. They even have a name for it—*Götterdämmerung*—twilight of the gods. It is not unreasonable to assume that Wagner's Id sensed the ailment all around him and expressed it in *Der Ring der Niebelungen.* No wonder many Germans, especially the nazis, are extremely fond of that music—it expresses sonorously their subconscious longing for an epic end.

Not all self-mutilations are due to an internal conflict. One large class of them are instruments of warfare against others—passive in appearance, but very aggressive in intent and sometimes very effective. Children cry so as to have their way with their parents; women cry and even become sick so as to have their way with their husbands; the jilted lover threatens to shoot himself unless the young lady will marry him; Gandhi goes on a hunger strike to fight the British Government; and the Gold Coast native hangs himself at the door of his powerful enemy. The success of this type of warfare or aggression depends on driving a wedge between the Superego and the rest of the opponent's personality; or, failing that, driving a wedge between the opponent and the public conscience. The argument is "You drove me to this, my blood shall be on your head [conscience]." Or, "He drove me to this; regard him as a criminal."

In the light of all the foregoing, one can see an unsuspected psychoanalytic reason for the ancient friendly admonition, "Be good to yourself."

THE PARENTAL DEVOTION OF BIRDS

By ALEXANDER F. SKUTCH
SAN ISIDRO DEL GENERAL, COSTA RICA

Most of us who have given even casual attention to the ways of wild creatures have from time to time witnessed acts of courage in the defense of home or young that have made our pulses throb with admiration. It may have been a mother raccoon returning heroically to rescue a cub who had lagged behind when danger threatened; or a parent bird defending her nestlings from a cat or a snake; or else we ourselves were the object of attack, when our well-intentioned visit to the nest was misinterpreted. We marvel at such dauntless heroism, and question whether we would possess the valor to confront enemies so many times greater and more powerful than ourselves, even in the defense of those we most love.

Of all my adventures among wild things, no recollections are more grateful to memory than those of their heroic acts in the defense of their homes. For nothing so warms the heart, even of the timid, as the sight of courage put forth in a good cause. Most of these recollections center about birds. Once, while bathing in a tropical river, my attention was drawn to a sudden commotion among the bushes overhanging the channel. A Song Tanager (*Ramphocelus costaricensis*) had her nest there. A slender green snake, about two feet in length, had surprised her by a stealthy advance and was devouring her spotted blue eggs. The tanager, crying loudly in her nasal voice, was fluttering about, trying to fight off the serpent. If she did not actually peck the lustrous green body, she certainly came within an inch of touching it. I fear that I was more the partizan than the disinterested scientific observer, for I no sooner became aware of what was happening than I began to look for a weapon—which, as usual, was not to be found when most urgently needed. As soon as I could lay hands upon a stick that brought the snake within reach, I rushed to the bird's assistance and knocked her green enemy into the water, just too late to save her eggs.

Meanwhile, the tanager had been hovering around her doomed nest, very nearly, if not actually, in contact with its assailant. This display of courageous determination surprised me, because where man is concerned the female Song Tanager is one of the most timid of birds at her nest. The green serpent was to the tanager in size as a boa or python from fifteen to twenty feet in length to a man. (The boa which the intrepid Waterton engaged in a wrestling bout amid the Guiana forests was, if I rightly recall, of far inferior proportions; and few men, I imagine, would care to repeat his exploit.) The fact that the tanager's defense against the snake was ineffective did not prove that she had failed to inflict punishment upon it; for I once watched a serpent of another kind, a mica, mortally wounded by bullets, continue to gorge upon the contents of the nests in a colony of Lawrence's Caciques. A ravening snake appears to be insensitive to pain and almost insensible to danger.

Often I have myself been the object of attack by too-zealous parents unable to understand my benevolent curiosity. One of the vivid recollections of my boyhood is of an adventure with a parent Bob-white Quail. Riding along a winding country road on my bicycle, I saw her cross with her covey of downy

chicks ahead of me, and dismounted just in time to catch one of the stragglers. But the old bird, noticing the fate of her chick, turned and dashed toward me so belligerently that I dropped my tiny captive and retreated. An angry barnyard hen will sometimes make as fierce an attack in defense of her chicks.

Years later, a pair of Catbirds (*Dumetella carolinensis*), nesting in a thorny barberry hedge between two houses in the suburbs of a great city, gave a fine display of courage and cooperation in the defense of their nest. During the female's frequent absences from incubating her three blue eggs, her mate would almost invariably perch in a little hawthorn tree hard by the nest, where he could keep watch over it. Here he stood guard in silence; but upon the return of the female he went off to a more distant ash tree to resume his sweet music. Whenever I came near his treasures, he advanced very close to me, tail spread and wings quivering, hopped excitedly about, and uttered loud mews. If I held my hand above the nest, the female Catbird would strike it with her feet. After the eggs hatched, both grew bolder in the defense of the nestlings. The mother bird would now not only strike my hand, but actually alight upon the back of it and peck me vigorously with her slightly deformed bill. The blows were rather painful, and at times drew a few drops of blood. If I turned the palm of my hand upward, she dared not rest upon it, but pecked from the side. Meanwhile, the father would flutter about my head, sometimes striking me. All in all, this was the most vigorous mauling I have ever received from any pair of birds.

The human hand, with its five long, mobile, grasping fingers, must seem a terrible instrument to creatures with less versatile forelimbs. I imagine that they look upon it with the same degree of aversion and dread as we do the tentacles of an octopus, the fangs of a serpent, or the sting of a scorpion. The Catbird, who doubtless had enjoyed no previous opportunity to study the mode of action of a hand, seemed instinctively to know which way the thing worked, and made her attacks with due caution. While taming a filly, I found that she would take food from my hand, and nuzzle my face, long before she would willingly submit to being touched with my fingers. She appeared to have the same dread of them as the Catbird.

In the forests of Panama, a male Slaty Antshrike (*Thamnophilus punctatus*), a bird somewhat smaller than the Catbird, defended his nest with equal valor. He was standing guard beside the little moss-trimmed cup when I attempted to lift out one of the nestlings. Fluttering his spread wings, expanding his tail into a black fan tipped with white, raising the blackish feathers of his crown, and spreading the slate-colored plumage of his back to reveal the white patch usually concealed in its center, he resembled a wild Indian in full panoply of war. Then, darting forward, he bit the tip of a finger—I suppose with all the might of frenzied parental devotion, yet so lightly that I scarcely felt the nip. Twice he repeated his daring assault, then retreated a little way. Meanwhile his mate, creeping over the ground close by, tried to lure me away by the familiar "broken-wing" ruse.

Many other birds have displayed signal courage in defending their nests from my well-meant but probably terrifying intrusion. I recall a male Groove-billed Ani who forcefully buffeted the back of my head whenever I visited his nestlings in the thorny orange tree. A Verreaux's Dove, who could not easily be frightened from its overhead nest in a coffee plantation, gave a stinging wing-blow to the mirror I held up to see the contents of the shallow cup—stinging, I fear, only to its wing. Then

it dropped to the ground and gave one of the best exhibitions of broken-winged helplessness I have ever witnessed. A Short-legged Wood Pewee, one of the smallest and weakest of the flycatchers, made most spirited demonstrations whenever I visited his nest, dashing close by my ears with angry snaps of his little bill. Trespassing birds far larger than himself were treated in the same fashion.

The literature of natural history, both dryly scientific and that intended to make more exciting reading, is full of accounts of the reckless courage which birds and mammals at times display in the defense of their young. Many an egg collector has had cause to rue his climb to a hawk's eyrie defended by parents strong of talon and dauntless of heart.

Some birds display their fortitude by clinging staunchly to their nests in the face of danger, rather than by bold attack. Chiefly, these are of kinds whose coloration renders them inconspicuous; to move would be to cancel the value of their protective coloration and to catch every watchful eye. Hence they remain sitting motionless in the face of an enemy until it is almost upon them, as though hoping against hope that they had not been seen. Yet it is not always the neutrally colored bird who remains faithfully at her post of duty: a Robin and a glittering hummingbird have permitted me to touch them lightly as they sat. But the bravest and most devoted of all the incubating birds I have ever known was a tiny female Yellow-thighed Manakin (*Pipra mentalis*). Clad in dull olive-green, quite lacking the brilliant scarlet that adorns the head of the black male, she was a most inconspicuous object as she sat in her shallow nest, hardly bigger than a watch glass, in the undergrowth of the tropical forest. She clung stubbornly to her nest, and allowed me to smooth the feathers of her back, touch her breast and bill; even shaking the branch that supported the nest would not cause her to forsake it. Since she could not easily be made to leave, when I wished to learn whether her eggs had hatched, I was obliged to push my fingers beneath her breast and feel. I found her clinging to the little cup with her feet. She provided a unique object for photography; she was the only mature bird I have ever known who would allow herself to be gently pushed about on the nest into just the pose the photographer desired. Yet away from their nests, these manakins are not devoid of a wholesome caution in the presence of man.

Birds that breed in burrows often remain on their nests with incredible disregard of their personal safety. This is especially true when the nestlings have just hatched, for it is then that parental devotion is at its most fervent pitch. I have opened the burrow of a Blue-throated Green Motmot, and of a Green Kingfisher, and lifted the parent bird unresisting from its newly hatched nestlings. A big Ringed Kingfisher remained upon her eggs with equal bravery; but her powerful beak forbade familiarities. Yet all of these birds were free to escape, since I had opened their burrows at the rear, and they might have slipped down the entrance tunnel to the outer air.

But despite all the steadfast devotion of birds to their nest, their zeal in its defense, their reluctance to forsake it even in the face of enemies far larger and more powerful than themselves, less than half of all bird nests remain inviolate until the young are fledged. Studies have been made of the nest-losses of many kinds of birds in the most varied regions. Only occasionally, where sufficient nests to be statistically significant have been followed through, has the number of losses been much below 50 percent. In the North Temperate Zone,

the premature loss of from 50 to 60 percent of all open nests seems to be the rule; often the destruction is considerably greater. In the tropical forest, the loss of nests is sometimes as high as six in seven. This will not surprise us, when we remember that the enemies of birds are numerous, while the birds themselves—for all their zeal—are for the most part harmless creatures whose only defense lies in flight.

But what does seem incompatible with the parent birds' manifestation of devotion and concern is the fact that they nearly always survive the loss of their eggs or nestlings. Very infrequently one finds evidence, in the form of feathers scattered about and below the nest, that the parent bird has shared the fate of its offspring. And even this telltale sign of disaster may be misleading, for I have known parent birds to leave many feathers behind, yet survive to build another nest. Nearly always, the despoiled nest is followed by another and another, until at last a brood is fledged, or the breeding season draws to a close. I have known a pair of birds to build four, five, or even six nests in a season, yet not raise a single nestling until it was fledged.

Are we to conclude from this that the solicitude of parent birds—their cries of distress, their attacks and feints of attack, their steadfast clinging to the nest in the face of danger—is all a sham and a bluff; that in their boldest resistance to the intruder they are acting a part, just as when they flutter away over the ground feigning a broken wing? Can it be that they have no real feeling in the matter, and not the least intention to sacrifice their lives in the defense of their offspring?

Birds do in fact at times give up their lives in the defense of their nests. If Audubon's famous painting of the Mockingbird in the rattlesnake's mouth is based upon actual observation, we have here a case in point. In the defense of its nest the British Corncrake is said to attack a dog or fight a rat until too crippled to escape. I personally have never witnessed a grown bird fall a victim to the assailant of its nest; but then it is not often that the watcher is at hand at the moment the calamity occurs. Yet many a bird, defending its nest or merely remaining steadfast upon its eggs, has placed itself within my power to kill it with a stick or even my hands, had I been so inclined.

We have seen that with most species of birds the eggs or nestlings are destroyed in half or more of all nests. At times the destruction is effected by some small marauder, weaker than the owner of the nest, during an interval when both parents are absent. But usually the destroyer is more powerful than the parent birds. Were these to offer a determined resistance, the outcome of the conflict could hardly remain in doubt: the parents must die along with their offspring. Even if they succeeded in beating off the assailant at the expense of mortal wounds, the nestlings would succumb from lack of attention. In many species, if only the male died, the female might care for her family alone; and I have known male birds of several kinds to be successful in raising nestlings whose mother disappeared after they were feathered. But with most species of birds, the economy of the nest is such that if the female be lost before the nestlings are covered with plumage, the eggs must spoil or the nestlings die of exposure. Even making allowance for exceptional cases, I believe that by a policy of "resistance unto death," nearly half of the breeding adults would each year not only fail to reproduce themselves, but perish in the attempt to reproduce. The remaining half of the breeding birds would doubtless not suffice to maintain the population at a constant level; for many a fledgling that

leaves an undespoiled nest fails to reach maturity; and the adults themselves face many dangers.

Thus it appears that natural selection must itself set a limit to parental devotion. For the parents to sacrifice their lives in vain attempts to preserve their offspring, or even in the successful defense of their young, would be a heroic but futile gesture. The struggle for existence among wild creatures is too severe to allow such chivalrous renunciation of self. Among highly social animals, such as man, the social Hymenoptera, and perhaps even beavers, Brown Jays, and a few other vertebrates, the sacrifice of an adult in the defense of the young may be of positive biological value; but certainly not among creatures with which the single individual or pair is alone responsible for the care of the offspring—for they have no orphanages, and adoption is rare or unknown.

In an economy under which all or nearly all of those individuals who lose their lives in an attempt to shield their offspring fail to leave progeny, the heroic race cannot long survive. I believe that the elimination of individuals inclined to take too great risks in the defense of their offspring is going on, on a small scale, all the time in many species. Thus it would be impossible for any species or race to arise, through natural selection, in which all or the majority of the individuals fight to the last breath to preserve their eggs or young. It is not impossible that whole species may have become extinct through no other cause. This is especially apt to occur when a new predator, with whose prowess and activity the native birds are unfamiliar, invades their territory. Natural selection certainly cannot forestall the development of strong parental solicitude, nor the manifestation of that solicitude in demonstrations of anger or attempts at defense when the young are endangered, but it sets a strict limit to those demonstrations and that defense. The parents must not often endanger their own lives, nor habitually enter into conflicts of which the issue is in doubt.

Often, when we visit a bird's nest, we witness behavior which is a compromise between parental devotion and the instinct of self-preservation. Anguished parenthood would—and sometimes does—throw itself recklessly upon the intruder. But more often it is withheld by that prudence without which its kind would cease to exist. The bird may dash boldly toward us—only to swerve harmlessly aside when nearly within reach. It may hover in the air above us and utter shrill screams of distress. Or else it may shower upon some inanimate, or even animate, substitute the pecks it dare not inflict upon us. A female Great-tailed Grackle, whose nest in a coconut palm I used to visit, would alight upon one of the great fronds and give it angry pecks while I looked at her nestlings close by. When I climbed to a nest of Brown Jays in a willow tree beside a tropical lagoon, the most zealous of the attendants would administer punishment to the surrounding boughs, or else alight on a banana leaf close by and tear it to shreds with its powerful bill. Blue Jays will also hammer vigorously at a branch while a human intruder is at their nest. Under similar circumstances, Ravens behave in even more spectacular fashion. "When the young are in danger, you often see the old birds work themselves into such a temper that they pitch down and tear grass up by the roots, looking a very picture of impotent rage. On one occasion, under such conditions, we saw a bird settle on a sheep's back and tear the wool off it, to everyone's great amusement except the sheep's" (Gilbert and Brook, *Secrets of Bird Life,* London, 1924). Prairie Falcons, when their nests are disturbed by men, sometimes strike and even kill other birds in the vicinity, making them

the innocent victims of outraged parental devotion (Bent, *Life Histories of North American Birds of Prey,* Washington, 1938).

IT APPEARS to be generally true that wild creatures are instinctively aware of the strength and prowess of their hereditary natural enemies, and avoid risking their lives in the defense of home or offspring against such enemies as are likely to overcome them. This principle might be briefly designated the "Law of Prudence." It applies not only to birds and mammals, but to cold-blooded animals as well. Once I watched a Red-throated Caracara (*Ibycter americanus*), a big black hawk with a white belly, attack the corrugated carton nest of a kind of large black wasp with a formidable sting. The caracara subsists largely upon the larvae of wasps, and is a master of the art of opening their hives. Attracted to the scene of impending conflict by the bird's hoarse, raucous calls, I prepared to witness a thrilling and hotly contested battle between hawk and wasps. But in this I was disappointed. From the first, the wasps seemed to recognize the mastery of the caracara. They hovered about it in a black swarm. A few apparently attached themselves to its plumage, for it shook its head, scratched its body with a foot, and appeared to pluck one or two from its feathers. But on the whole it took little account of the insects, which soon ceased their attacks and flew about in helpless anger; while the caracara, hanging back downward, proceeded to tear away the corrugated gray envelope of the nest and to devour at its leisure the contents of the brood combs.

These wasps, although they lost all their young brood, lived to establish a new nest on the wall of my house—behavior biologically more sound, if less inspiringly heroic, than losing their lives in a vain attack upon the caracara. Not long after their new corrugated fabric was completed, the house was invaded by a horde of brown army ants. These incursions were repeated at brief intervals during the course of several weeks. When the ants discovered the nest of the big black wasps, they filed in long, hurrying columns to the attack. The wasps merely lined up around the single inch-wide orifice at the top of their nest, their bodies entirely inside, their heads in a ring just within the rim. Whenever an ant passed over the edge of the doorway, it was easily pushed back by the defenders. The brown hordes picked out each weak spot in the corrugated carton walls and attempted to open a breach. But whenever a small gap was made, it was defended from within in the same manner as the permanent entrance. For two hours I watched an exciting siege, an attack upon Troy in miniature, right on the outer wall of my dining room. At length the Greeks admitted defeat and drew off their forces, leaving the Trojans in unshaken possession of their citadel. Neither attackers nor defenders, so far as I could learn, suffered a single casualty. The wasps knew, instinctively, when prudence was the better part of valor, as with caracara, and when they could put up a successful resistance.

Of all the numerous kinds of curious wasps' nests attached to my walls and rafters, none other escaped destruction that eventful afternoon; for either their occupants were smaller and weaker than the black *guitarrónes,* or else their houses were more open. And in every instance, apparently, these less powerful wasps fled without a real attempt at defense, although a few were trapped in their nests and fell prey to the Echiton ants. The chief booty, however, consisted of the soft white larvae, which in long, victorious columns the ants bore down the walls and out through the grass in the yard. In many cases, after the departure of the ants, the wasps returned

to their desolated homes, and began to fill the empty brood cells with eggs once more.

The army ants made several other afternoon attacks upon the big gray nest, but always with the same lack of success. The corrugated fortress was clearly impregnable by day. Finally, the Echitons attempted a nocturnal attack, with a very different outcome. The wasps, big-eyed, diurnal creatures, had clearly lost advantage in the face of their nearly eyeless opponents, to which night or day seem to be as one. Many fled the doomed nest and settled upon the walls of the house, or else flew in to the lamplight through the open door, and so gave me notice of what was taking place. Others were trapped inside and overpowered, and their lifeless bodies dragged out along with the larvae, each by a whole squad of toiling ants. But next day, the numerous survivors returned to their childless home, and began to fill the brood-combs anew. Here, again, the instincts of the wasps led to the maintenance of a breeding population of adults rather than to the preservation of the young brood. Biologically, an adult in the reproductive period of life is far more important than an immature individual; for, under natural conditions, the chances of any newborn animal's reaching reproductive maturity are not high. The breeding adults are a selected, and therefore more valuable, group than the infants of the species.

My experience with the wasps and ants led me to introduce a modification to the "Law of Prudence," which is that wild creatures as a rule avoid risking their lives in the defense of home and offspring against an enemy powerful enough to overcome them *under the conditions of the attack.* For it is evident that while adults may put up an effective defense under certain conditions, with altered circumstances they flee from the same enemy. Many birds' nests are despoiled during the night, possibly by enemies that the birds, which are helpless in the dark, would be able to drive away in the daylight.

These responses to attack, whether by flight or defense, are instinctive rather than rational, or based upon individual experience. The same wasps that appear to know so well what course to pursue when attacked by a hereditary natural enemy, are most stupidly blind where man is concerned. When a swarm has settled down to build a nest on a house, where they would be a menace to the inmates, it frequently happens that there is no way to discourage them short of extermination. If a few are killed at a time, the survivors return; and they must be attacked again and again until all are dead. They have not yet learned the ways of man, and appear to be incapable of learning them from individual experience. Similarly, birds upon a desert island fall easy prey to cruel, avaricious men.

The "Law of Prudence" applies to the procuring of food no less than to the defense of offspring. Just as a wild creature's chances of rearing its young to maturity are at best so small that nature cannot afford to let it lose its own life in defending them against a more powerful assailant, so its natural enemies, whether larger animals or parasites, are so numerous that it would jeopardize the existence of its kind if it habitually preyed upon animals whose strength or weapons of defense made them a doubtful quarry. The squirrel and the jay enjoy an occasional egg, but will not attempt to steal one from a hawk's nest while the owner is present; the eagle hungers for the newborn antelope, but seeks to surprise it away from its mother rather than brave her sharp prongs. I believe that no wild animal normally obtains its food in a manner so dangerous as, let us say, the occupation of a deep-sea fisherman in

northern seas before the mechanization of the industry. Those animals who appear to win their food, or that of their offspring, in a most desperate and hazardous fashion, upon closer study are found to be so expert at their trade, or so well-provided with bodily defenses, that actually they run little risk. The caracara, which subsists largely upon the larvae of wasps, would seem to be doomed to a life of cruel punishment and distress; yet I saw that in attacking a hive of one of the fiercest wasps of this region, it had things all its own way.

Snake-eating birds, which prey upon venomous as well as nonvenomous species, would appear to lead a precarious existence; but a little reflection will show that this cannot be so. A bird like the Laughing Hawk (*Herpetotheres cachinnans*)—the *Guaco* of Central America—whose principal, if not sole, diet is snakes, needs on a conservative estimate 365 of these reptiles a year for its own consumption. At a nest I watched, the male brought a snake every morning and evening for his mate and downy nestling; whence I conclude that a breeding male must capture upwards of five hundred serpents in a year. In order to make so many successful attacks upon snakes, he must be a master of his art, not an experimenter or a bungler. He must know to a nicety which snake he can safely seize, and which is too powerful for him. Although little is known of the rate of reproduction of these hawks, it appears to be slow, indicating that the adults are long-lived. The poets delight in describing long-continued, spectacular conflicts between the eagle and the serpent it has grasped in its talons—none more thrilling than that which opens the first canto of Shelley's "Revolt of Islam." It seems a pity to cast doubt upon the factual accuracy of such stirring passages in poesy; yet in the interest of truth I must record that I have seen hawks and eagles sailing through the air with snakes in their talons more times than I can recall, and always the serpent, far from endangering the life of its captor, hung a helpless victim in his talons. We may recall that in some of the great serpentaria where antivenins are prepared, the attendant who allows himself to be struck by one of the snakes under his charge is deemed guilty of unpardonable carelessness, and suffers a pecuniary fine in addition to the painful treatment for snake-poisoning.

Those wasps which store up paralyzed tarantulas or other great spiders as provender for their larvae appear also to engage in a desperate business; but in reality, as the studies of Petrunkevitch and others have shown, the spiders are terror-stricken at the approach of these skillful huntresses, and fall almost unresisting victims. Perhaps the terror of the rabbit when cornered by the weasel, and of small birds when overtaken far from cover by a hawk, is typical of all animals when face to face with the creatures that habitually prey upon them.

But the most skilled experts at times make mistakes at their trade; so these hunters of large or dangerous creatures sometimes misjudge their prey and pay dearly for the error. An example of this is the Osprey who sinks his talons into the back of a fish heavier than he can lift, and is drawn under water and drowned by the intended meal. Once I saw a Laughing Hawk pounce down upon a large black-and-yellow mica crawling through a pasture. The snake reared up; the hawk, evidently surprised by its length, at once took flight and did not repeat the attack, not caring to risk its neck in an encounter of uncertain outcome. Such mistakes appear to be rare in the lives of hunting animals. Their slow rate of reproduction, as compared with that of the creatures upon which they prey, is proof of this.

Last October, at the edge of the forest beside my house, I saw the act of the

Laughing Hawk and the mica repeated in all its essential details, but with actors of far smaller size. A female Tyrannine Antbird (*Cercomacra tyrannina*), an insectivorous bird about the size of a sparrow, had discovered a big, brown, hairy spider in its lair between two leaves it had bound together with cobweb. When threatened by the antbird, the spider raised its four front legs, keeping the two on each side together except at their tips, where they were slightly separated, so that the legs jointly bore considerable resemblance to the pincers of a scorpion. Whenever the bird drew near, the spider waved these raised and threatening appendages, and then she would draw back. It was clear that the little antbird was tempted by this large and succulent morsel, and it was just as plain that she feared it. Perching at a respectful distance, she stretched forward her neck to seize it in the tip of her bill, but she did not stretch quite far enough. Finally prudence overweighed appetite, and the bird flew off in search of less formidable game. The spider measured possibly two inches across the spread legs, and was a big morsel for a bird only five inches long.

The prudence of the foraging Wren-tit (*Chamaea fasciata*) is described by Dr. Erickson in her monograph of this little bird of western North America:

> The attack on larger prey, such as centipedes two or three inches long, as seen in caged birds, is a prolonged process which combines dexterity and speed. The lashing centipede, to a wren-tit, is formidable and inspires obvious fear. It is circled and watched at length, and often succeeds in disappearing into the leaves. Otherwise, the bird finally recognizes an opening, there is a quick thrust, grab, and toss, the centipede lands several inches away, and the tactics are resumed. With each attack the vigor of the prey is reduced. Finally, the wren-tit dares hold it with foot or bill. . . . Once, a captive wren-tit pulled to pieces and ate in the course of half an hour a very active four-inch angleworm, an operation which was not repeated though the opportunity was often provided. It was accomplished with extreme awkwardness, both because of the obvious fear of the bird for the twisting worm and because when a grip was secured the slimy length pulled through the grasping foot.

Further evidence of the habitual prudence of birds in choosing their prey is found in the eyemarks of many caterpillars, notably those of the swallow-tailed butterflies (*Papilio*), their habit of raising their foreparts or lashing from side to side when threatened, and similar markings and attitudinizings of small creatures that are perfectly harmless and apparently quite good to eat. It would be difficult to account for the evolutionary development of such devices were they not at times effective in frightening away hungry but timid birds and perhaps other insect-eaters as well. Several observers have described the startled behavior of small birds that suddenly came face to face with the wide-eyed, serpent-like mask of the caterpillars of the Tiger Swallowtail and related species.

In times of scarcity and famine, a different story must be told. Then, half-famished animals are driven to take unequal chances to procure the means of continued life. Wolves attack villages, braving the deadly weapons of men; animals ordinarily cautious are lured into traps. But these are exceptional circumstances.

Yet occasionally, even when there appears to be no dearth of food, creatures of two species will engage in long, fierce conflicts of which food is the coveted prize. Social insects, especially ants and bees, seem most frequently to wage such deadly battles. At times bees of two kinds join in hottest warfare, one seeking to carry off the sweet stores of the other. The dead and dying bestrew the ground, opponents locked together in deathgrips. Or else ants attack a hive of bees, which valiantly defend their home, slaying many of their adversaries, but paying several lives for one. Such

struggles appear at first sight to present exceptions to the "Law of Prudence," but the exceptions are more apparent than real. In all the battles of this sort that I have witnessed, the opposing hosts were rather evenly matched; and if one lost more heavily than the other, it had more soldiers to spare. Each side had a prospect of victory. If the attackers won and carried off the booty, they paid for it with many lives. But then the hive's stores of nectar are gathered only at the price of the workers which wear out their lives in quest of it. Perhaps, after all, a full storehouse won by a victory in which many lives are sacrificed, costs less per drop than nectar slowly gathered from its primary sources in pacific fashion. And if the defenders are victorious and save their hive at the expense of many of its inhabitants, the survivors carry on the work of rearing the young brood. Their situation is quite different from that of more solitary animals; comparable to that of man.

THERE are two reasons, then, why we should not expect to see birds engage often in life-and-death struggles in the defense of their nests and young. First, were they to resist habitual predators more powerful than themselves, the existence of their kind would be placed in jeopardy. Second, they are not often called upon to do more than threaten weaker animals that approach the nest, for these would jeopardize the existence of their species if they attacked stronger creatures in their quest for food. So long as more certain prey is available, animals prefer not to attack others that are an even match for themselves. Sometimes, of course, a weak marauder attempts to rob a nest in the absence of its owners, and is put to ignominious flight by their sudden return.

Some years ago, I spent many hours watching a nest of a pair of Laughing Hawks, situated a hundred feet above the ground in a cavity in the trunk of a huge tree standing at the edge of the forest. Through field glasses, I could sometimes see a single downy nestling, buff-colored with a black mask like that of the adults, as it tumbled about in its lofty nursery. The mother hawk passed most of the day sitting in the cavity beside the nestling, and during her brief absences went no farther than a tree in front of the doorway, whence she could keep an eye upon her youngster while she preened, scratched, and stretched her wings. The male hawk brought all the food, arriving morning and evening with a snake from which he had already bitten the head. Alighting upon a bough in front of the nest, he would call his mate to come out and receive the prey. Then the two would celebrate the event with a long-continued duet of loud, far-carrying cries.

One afternoon, while the female hawk rested in the tree in front of the nest, I suddenly became aware that a long, black tayra—a larger relative of the weasels—was climbing squirrel-like up the great trunk toward the eyrie. He advanced toward the cavity in a direct, unhesitating fashion, as though he had already spotted it from the ground. The Laughing Hawk failed to notice the beast, or at least gave no outward indication that she had seen him, until he was almost within reach of the wide doorway of the eyrie. I desired intensely to complete my study of the only Laughing Hawk's nest I had ever seen; but the situation was so charged with possibilities of intense, dramatic action that, waiting breathlessly for the outcome, I made no move to frighten the advancing tayra. How effective was this unremitting guard the mother hawk kept over her offspring? Only when the tayra was on the verge of entering the hole did the hawk bestir herself. Then, uttering a low cry, she darted directly toward him. But he snarled, baring

strong fangs; and she dropped away, to return to her former perch.

The hawk's resistance was so weak and ineffective that it did not give me time to prepare for my own next move. I had expected that she would at least retard the tayra's progress toward the eyrie, but he hardly altered his pace on her account. When she returned tamely to her watching post, leaving the beast at the very doorway of her nest, I felt in my situation almost as helpless as the little hawk in the eyrie. I could think of nothing to do except to shout and wave my arms. By continued noise and gesticulations, I succeeded in driving the tayra to the ground, but not before he had reached into the cavity and killed the nestling by a stroke of his forepaw.

I was disappointed in the mother hawk's fainthearted defense of her nestling. I had expected something more heroic from such watchful motherhood. Such attentive, unremitting guard, to be capped by so slight a gesture of defense! But pondering over the event at a later date, I saw clearly that the outcome could not have been other than it was. The Laughing Hawk, intrepid snake-eater though she be, is slow of flight and weak of talon, not to be compared in prowess with such winged furies as the Peregrine and the Goshawk. A marauding, voracious toucan or a squirrel might have been put to flight, but she was clearly no match for the strong-fanged, catlike tayra. She could have done nothing more than offer herself a sacrifice upon the altar of parenthood; and the offering would have been garnished by the tender body of her nestling. Nature could not permit such a sacrifice without jeopardizing the existence of the whole race of Laughing Hawks—whose loss would result in a widespread upsetting of her balance, with snakes increasing and each year destroying a larger number of birds' nests, while birds declined in numbers. It was more important for the Laughing Hawk to continue to live that she might make another attempt to increase her kind, than that she should die heroically, a beautiful sacrifice which could profit only her enemy the tayra. Among Laughing Hawks there are no bards to immortalize a glorious death for the inspiration of future generations of hawks. Natural selection had set a limit to her parental devotion.

BOOK REVIEWS

A MEDICOPHARMACEUTICAL CLASSIC

Pharmacopoeia Londinensis of 1618. Reproduced in Facsimile with a Historical Introduction by George Urdang. vii, 299 pp. $12.00. 1944. State Historical Society of Wisconsin, Madison.

In 1914 a prominent druggist of Madison, Wis., Albert Henry Hollister, and his wife Kittie E. V. Hollister bequeathed to the Wisconsin State Historical Society a sum of money to be known as the Hollister Pharmaceutical Fund. It was decided by the Society, with the consent of the executor of the will, to use half of the income from the fund to reprint "rare and valuable works in the field of pharmaceutical history and development."

The first reprint to be issued with the aid of this fund was the *Pharmacopoeia Augustana of 1564,* which appeared in 1927. Now the exceedingly rare *Pharmacopoeia Londinensis* has been issued in a magnificent facsimile edition. This by itself would entitle the Wisconsin Historical Society to receive the thanks of the learned world. But this facsimile is more than a reproduction of a rare old book. Preceding the text is an introductory essay of eighty-one pages entitled "The History of the *Pharmacopoeia Londinensis*" written by the brilliant and scholarly George Urdang, Director of the American Institute of the History of Pharmacy in Madison. In masterly fashion Dr. Urdang has provided an analysis of the text, relating it not only to works that preceded and succeeded it elsewhere, but placing the work in its proper setting in the social, economic, and political atmosphere of its time.

The London *Pharmacopoeia* has a strange and interesting history. The idea for such a work seems to have first been recorded in the *Annals* of the Royal College of Physicians on June 25, 1585. But it was not until May 7, 1618, that it was finally issued. It had been eagerly awaited not only by the physicians and apothecaries of England but also by the representatives of medicine and pharmacy in western Europe. This was not because it was the first pharmacopoeia (there had been many earlier works) but because it was the first such work to be held as authoritative for a whole country, i.e., England. The many European compends that had preceded it were gotten up for municipalities or city-states.

Then a curious thing happened. The issue of May 7, 1618, of the London *Pharmacopoeia* was suppressed, and a new version was made public on December 7, 1618. Even a cursory examination of the two issues shows that the difference between them was more fundamental than the mere substitution of one title page for another.

Although much has been written about, and many scholars have labored over, the problem posed by the two issues of the first edition of the London *Pharmacopoeia,* it remained for George Urdang to provide what will almost certainly come to be regarded as a complete, correct, and satisfying solution to the mystery. This solution cannot be given here because it is too complicated to compress into a small space, but I am convinced of its soundness.

The first and suppressed issue of May 7, 1618, seems to have been printed in a small edition, and only six copies are known to have survived. The facsimile, beautifully reproduced in an edition of five hundred copies, is from the copy now in the British Museum.

I can do no better in concluding my review of this noteworthy contribution

to pharmaceutical history than to quote from the final paragraph of Urdang's introduction:

> Well-nigh forgotten and lost, deliberately misrepresented, this first issue once more makes its appearance before the medico-pharmaceutical world. It has an interesting history. What is more, it is interesting in itself as the first Anglo-Saxon attempt at an official formulary. Moreover, it is a reliable witness of what the learned medical eclectics of the seventeenth century regarded as the necessary armament of the average physician and apothecary of their period.

MORRIS C. LEIKIND
LIBRARY OF CONGRESS
WASHINGTON, D. C.

TWO PLANT COLLECTORS IN BRAZIL

Brazil, Orchid of the Tropics. Mulford and Racine Foster. 314 pp. 1945. $3.00. The Jaques Cattell Press, Lancaster, Pa.

THIS recent book dealing with plant collecting in Brazil might appropriately have been given a less general title. The authors are well-known horticulturists, who in the past few years have specialized on the family Bromeliaceae, of which the best-known representative is the pineapple. Their primary purpose in collecting in Brazil was to gather live specimens of plants of this family for their Florida garden, although plants of other families, especially orchids, were collected also. The expedition was eminently successful, even though some of the choicest plants died during the long sea voyage or during fumigation at the time of their entry into the United States. Fortunately, however, the authors were wise enough to make herbarium specimens. In this way they have made a worth-while contribution to science, for they discovered more than forty new species and varieties of Bromeliaceae. This is not wholly surprising, for general botanical collectors have been apt to neglect these plants, because, like the cacti and the palms, they are among the most difficult to preserve properly. They are hard to dry, and, although they are often very large, it is necessary to collect the whole plant. The resulting prepared specimens are therefore bulky and awkward to transport.

The authors began their collecting in the state of Bahia, and visited especially Jacobina and Ilhéos, one of the classic collecting grounds of the great botanist Martius. This is a region of rain forests, and in such habitats the Bromeliaceae are mostly epiphytes, or "air plants." The natives usually call them parasites, but they are not, for they use the trees only as footholds. They grow so high in the trees that getting them is frequently a problem, even though some of the natives are expert tree climbers.

After a return to Rio de Janeiro, an expedition was made to the little coastal state of Espírito Santo. Here, near a gigantic shaft of granite known locally as the "Dedo de Deus," or "Finger of God," the authors discovered a spectacular new species, which Dr. L. B. Smith has named *Aechmea orlandiana,* after the Fosters' home city of Orlando.

This first trip to Brazil ended with an automobile excursion to Paraná, a region in southern Brazil floristically distinct from the more tropical sections. The following year (1940) the authors returned to Brazil and made trips to the states of Minas Geraes, Espírito Santo, and to the large far-western state of Matto Grosso.

Plants are foremost in the minds of the authors, but passing observations are made on insects, birds, frogs, and monkeys. The comments reveal also their high esteem for the people of Brazil, a natural consequence of the friendly fashion in which they were everywhere received. The accommodations were rather primitive in some instances, but the best hospitality available was always offered, despite the fact that many Brazilians were unable to comprehend the authors' interest in "parasites."

The literary style of the book leaves much to be desired, and the numerous solecisms are disconcerting to the reader, as are the often misspelled scientific names. The attempts at philosophizing and "fancy writing" are frequently overdone, as for instance: "In Jungle, the fecund mother of plant life, we find a peace and contentment that never knows the fear of storybooks. We were embraced by great arms of chlorophyll full of the love of life, the constancy of life, and the nothingness of death." However, much can be forgiven the authors by reason of their obvious enthusiasm for their subject. The book is beautifully illustrated by 137 photographs and four colored plates, as well as by several clever drawings by Mr. Foster.

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OF INTEREST TO STUDENTS OF SNAKES

An Annotated Checklist and Key to the Snakes of Mexico. Hobart M. Smith and Edward H. Taylor. 240 pp. 1945. 50 cents. U. S. National Museum Bulletin 187, Washington.

THE title of this book might remind one of the man who was reading an encycylopedia and when asked how he enjoyed it replied that it was interesting reading, but that the plot was somewhat broken.

Smith and Taylor have lavished an immense amount of work on what they state is a temporary steppingstone in a rapidly developing field of research. Their effort will not be as ephemeral as they believe because it is built on a rock of sound common sense, as indicated by the eleven-page introduction. The book may well serve as a model for future checklists as it contains many improvements over those with which the herpetologist now struggles.

Remembering that their book is for the convenience of the student, the authors have thoughtfully arranged genera and species alphabetically. They adhere to specific names unless intergradation is known to occur. A "species" is defined as: any population isolated reproductively, geographically, or morphologically, or a total ensemble of characters in which at least one character always distinguishes the population from all others.

The writers explain their use of the terms "form," "race," "group," and "section." Unfortunately, every authority has a slightly different meaning for some of these terms, and it would be much simpler to abandon them altogether. The stand on primary and secondary homonyms is what all herpetologists have long desired—the suppression of only primary homonyms. The reason given for not suppressing secondary homonyms is that careful examination would be required, including the poorest and least known portion of literature and educational or semipopular works, and sanctioning the activities of the "professional name-giver."

That Mexico probably has a richer fauna than any other area of the globe of equal size is their claim, based on K. P. Schmidt's statement that Mexico acted as a very important faunal paleopeninsula and to a lesser extent as a faunal neopeninsula!

The body of the book is excellently arranged. A student who is familiar with the genera can most easily locate the page by a glance at the table of contents; others can run down the genus in the first key. Thereafter each genus has its own species key. An innovation of interest is the inclusion of species under the genus heading, giving the total number of species and the number known from Mexico.

Specifically, parentheses for authors' names are used only where a species has been transferred from another genus. The synonymies include a reference to

the original description, and original descriptions to all synonyms having type localities in Mexico; references to known illustrations; citation of the first appearance of the name-combination used and reference to any other work considered pertinent. The type number, locality, depository, and range is given for each species.

Except for a complete index the book closes with a list of species by states. This list discloses that only one species has been recorded from Aguascalientes, 4 from Tlaxcala, and 5 from Queretaro, as against an average of about 40 or more for the remaining 28 states. One immediately wants to set off for Aguascalientes, until a glance at the map shows that all the three poorly represented states are surrounded by states with goodly lists of species. This illustrates the unfortunate necessity of using political subdivisions instead of ecological areas. If Aguascalientes had perchance been included as part of one or more of the contiguous states, nothing out of the ordinary would appear; and, on the other hand, there are doubtless many areas larger than Aguascalientes that have never been herpetologized at all.

The book is a "must" for any student of Mexican snakes and it will doubtless remain the standard book of reference for many years to come.

CHAPMAN GRANT

SAN DIEGO, CAL.

A SAILOR'S HANDBOOK AND A WOODSMAN'S GUIDE

Science of the Seven Seas. Henry Stommel. 208 pp. 1945. $2.50. Cornell Maritime Press, New York.

THIS is a handy and concise popularization of the essential facts of physical oceanography and meteorology, and, considered from those branches of science alone, a successful work. However, the treatment of Darwin's coral reef theory and Wegener's continental drift hypothesis are worse than inadequate, since mere mention of them without stating opposing views leads to the impression that they are the accepted theories for the phenomena concerned. While admitting that he is not a biologist, the author appends a collection of photographs of marine life. This part of the book would have been improved by a better selection of illustrations—the combination of dingy views of motheaten museum groups with Ewing's interesting deepwater photographs is strange indeed—and the omission of such misleading commentaries as "Penguins are inhabitants of antarctic regions. Birds only in name, they have lost their ability to fly. They are very social and intelligent animals." The bibliography is barely a lick and a promise, and the addition of half a dozen or so titles would have enhanced the value of the whole book, for many do not have access to a library catalogue and are unable to follow the author's casual advice to consult one.

J. W. HEDGPETH

ROCKPORT, TEXAS

The Lost Woods. Edwin Way Teale. 326 pp. (with over 200 photographs). 1945. $4.00. Dodd, Mead & Co., New York.

EDWIN WAY TEALE's fine photographs are familiar to all readers of the natural history magazines, and his writing is commanding a growing circle of readers. This book, essentially an expanded notebook of field trips and of observations in his own backyard, is something of a miscellaneous gathering, with abrupt transitions from Indiana to Long Island beach and Florida to Walden Pond. Such casual treatment, when reinforced by personal observations and impressions, has a charm of its own, although it can be easily overdone. Mr. Teale, however, keeps his prose within bounds, avoiding purple passages, and manages to convey to the reader something of the

fresh wonder of his own observations. He realizes, as some writers do not, that no matter how well known something may be to the professionals, it is new to everyone who sees it for himself for the first time. This attitude is essential for all popularization of science and natural history, but unfortunately it seems rarest among professional biologists in this country. However, such enthusiastic amateurs as Mr. Teale are doing very well indeed.

J. W. HEDGPETH

ROCKPORT, TEXAS

ENCYCLOPEDIA MALESIANA

Science and Scientists in the Netherlands Indies. Pieter Honig and Frans Verdoorn, editors. 491 pp. Illus. 1945. $4.00. Board for the Netherlands Indies, Surinam and Curaçao, New York.

"I DO not understand how any one can be content until he has experienced the wonder of the tropics." That was the impression of David Fairchild upon first reaching Padang in 1895. And many have been the traveling scientists who have felt likewise. Quite naturally the tropics fall into two broad divisions: the Neotropics and the Old World tropics. A large and important fraction of the Old World tropics is the East Indies. Like the West Indies, the East Indies have been perhaps more unified in the realm of the Three Kingdoms than in the rule of their governing Kings! But from the galleon days of spice trading, the East Indies have been thought of, predominantly, as the "Dutch East Indies"—those "garden islands of the Great East"—and now we have a 491-page manual to answer queries, mark off the frontiers of knowledge, and tingle travel nerves.

This encyclopedia for the reference shelf is organized in a way that will beguile you into wider reading when you seek some particular reference, that is, if you permit yourself the inefficiency of browsing. Seventy-five authors have contributed to the manual, which consists of (1) original articles prepared expressly for the volume; (2) reprinted articles dealing with the development and status of various branches of science in the Netherlands Indies; (3) travel accounts and sketches or impressions, both early and recent; (4) *serta malesiana:* short articles, biographies, reviews, bibliographies, and commentaries; and (5) a directory of scientific institutions, societies, and workers in the Netherlands Indies at the time of the Japanese invasion. The reprinted and original contents are in the proportion of almost exactly 2:1. Yet the reprinted articles are commonly ones which up to now have not been generally accessible, having appeared in regional periodicals or in the Dutch language. The reprinted articles and translations have, furthermore, often been critically edited. Some doubt may be expressed as to the value of reprinting seventy-two pages of Wallace's *Malay Archipelago.* Much of that section is printed in small type, conducive to eyestrain if read very continuously, and the general availability of the book renders it unnecessary for the ordinary user to consult this partial reprint. On the other hand, not to have reprinted Mayr's provocative contribution on "Wallace's Line in the Light of Recent Zoögeographic Studies" would have been a genuine loss! Yet Mayr's original article is quite as accessible as Wallace's book, if not more so. It is, then, rather the greater ready reference value of Mayr's paper that makes the difference.

Among the original articles and their authors, the archeologist will want to read: Prehistoric Research (von Heine-Geldern); *Pithecanthropus* Studies (Weidenreich). For the agriculturalist there are: Veterinary Science and Practice (Frickers, Haasjes and Hoskins); Horizons in Agriculture (Honig); Rubber Program (Coombs); Rubber Culti-

vation (Tengwell); and the Hot Springs Conference (Riemens). For the geologist: Stratigraphy of Java Sea Sediments (Myers); Structural, Stratigraphic, and Economic Geology (Stauffer); Contribution of Meinesz (Field); and Volcanology (Braake). For the physical scientist: Astronomy Research (Kuiper). For the naturalist: Biota of Tjibodas (Went); Conservation of Wild Life (Westermann); and Fish and Fisheries Research (Herre). For the physician: Survey of Diseases and Clinical Medicine (Snapper).

Among the reprinted articles, useful for ready reference, may be mentioned those on: Climate and soils, climate and meteorology, soils against population density studies, livestock and veterinary science, history of quinine culture, development of Sumatran naval stores and pulpwood, hydrodynamics, history of Buitenzorg Botanic Gardens and of the Zoological Museum, paleobotanical research, Rumphius, vegetation of Sumatra, Flora and Botanic Garden of Tjibodas, Massart's Java Studies, extracts from Fairchild's writings, missionary medicine, beriberi, rabies research, medical education, Wallace's Celebes explorations, Forbes's observations in the Bantam and Preanger Regencies, phytochemical research, tobacco studies, Sumatran forest regions, mineral resources, and earthquakes!

Though the polyglot character of the contributions (most of them are in English) would make the preparation of an index difficult, yet its absence is conspicuous, especially since the contents are arranged alphabetically by author, not by subject. Thus information upon Teysmann appears on page 185—there spelled "Teijsmann"—pages 232, 302, 392, 409, and 414, but to find these references one must search through many likely articles. The reader concerned with the East Indies will browse widely and often through the whole book, for, lacking an index, in no other way can its resources be mastered. Sumptuous in its format, printed on heavy plate paper of good quality, free from printer's errors, and well bound, the book is of enduring beauty. The 134 text figures and full-page plates (with fully documented sources) are well selected. Krakatao from the air is exciting. Many reproductions make the names of Nieuhof, Blume, Rumphius, and Valentijn come alive. Along with the maps and photographs, these reproductions enliven a book that under less imaginative editorship might have appeared as musty as an old Amsterdam garret. From the Bird of Paradise cover medallion to the rear end-papers, this encyclopedia *malesiana* is a satisfying, authoritative manual of importance. N.B.: Not the smallest satisfaction comes from the book's modest price—about four-fifths of a cent per page!

In the spirit of "one world" the editors write: "We hope there may be, among the readers of this volume, some who will feel drawn toward the Malaysian Islands, some who will feel that they have a training and knowledge which can be used there for the good of mankind. We also trust, and confidently, that the spirit of humanism and honest internationalism, unhampered by prejudices of race, creed, or personality, will guide the future of science and its applications in the Netherlands Indies."

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GLAMOURIZING SNAKES

They Hop and Crawl. Percy A. Morris. 196 pp. Illus. 1945. $3.50. Jaques Cattell Press, Lancaster, Pa.

THERE has long been a need for a single volume which would adequately treat the reptiles and amphibians of the northeastern states in a way comprehensible to the layman or the beginning

student. This book might have been such a volume, but it is not. It claims to deal with the continental United States, which it does not do at all adequately (not a single Pacific Coast species is mentioned), and while there is "special emphasis" on the forms east of the Mississippi, I note the omission of two families and at least fifteen genera of this area. Among the forms not mentioned are: two salamander species which are common in New York and in Pennsylvania; the very extraordinary amphibian Siren; the most common sea turtle on the northeastern coast; and the only Amphisbaenid lizard in the country. At this rate of omission it seems strange that space is given to a considerable number of forms of the southwestern desert area. New England is the area most adequately covered, only one species being omitted.

The statements about the animals themselves leave much to be desired if they are meant to be used in identification. There are no keys for this purpose, and less than half the species are figured. Many of the statements about range, habitat, habits, food, etc., are erroneous, and such errors occur even in the accounts of northeastern forms.

In a work of this kind I can see no reason why the species of a genus or the genera of a family should not be grouped together, but this normal arrangement is frequently not adhered to. Classification is a part of the science, and it is just as easy and just as "human" to treat the species of a genus one after the other, as it is to intersperse them with species of other genera, and thus abandon (not "humanize") the scientific arrangement.

The author seems to have relied entirely on books already written for laymen. Indeed, to a considerable extent he seems to have been "humanizing" the writings of Ditmars, and of books of the "Handbook" type, and it would seem that he has not consulted some of the best or most recent of these. Aside from a quotation or two from Holbrook (over a century old) there is no evidence that he has consulted any scientific work in the field of herpetology; even the local guides to special groups such as Babcock's *Turtles of New England,* Bishop's *Salamanders of New York,* or Surface's *Serpents of Pennsylvania.*

Reading this book may well help to dispel prejudice against reptiles and amphibians and "humanize" these animals. As an attempt to humanize herpetology, or as a guide to a beginning student, it is quite unsatisfactory. It would not be advisable to *use* the book except in New England.

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SOIL CONSERVATION OR WORLD FAMINE

Food or Famine: The Challenge of Erosion. Ward Shepard. x. 225 pp. Index. 1945. $3.00. The Macmillan Company, New York.

Pay Dirt—Farming and Gardening with Composts. J. I. Rodale. x. 240 pp. Bibliography. 1945. $3.00. The Devir-Adair Company, New York.

FOOD or famine; conservation or waste; pay dirt or impoverished soil; to plow or not to plow; to be or not to be; peace or war. This is a day of drastic alternatives that stem from land and how it is used. Ward Shepard in *Food or Famine* sounds the alarm of danger in wastage of soil through erosion. Our food supply is imperiled by depletion and impoverishment of soil that supplies food and fiber, fuel and timber, and other raw materials for a host of things brought forth by modern technology. To undermine the productive capacity of the land is a calamity now and will become more so as demands of increasing populations grow. The world is faced with famine, for land has been misused nearly everywhere. Only in a few localities is the land used in such

a way as to assure sustained production from generation to generation.

Fortunately, there is yet time and still hope in recent movements for conservation of basic resources. The forest conservation movement got under way in the United States a generation ago, and now the movement for soil conservation through erosion control is under way. Only a beginning has been made, but it is a beginning built on a sound foundation, both in its technical and in its social features. Soil and water conservation measures are the outcome of tested farmer practice as well as of scientific experiments. They are technically sound.

Moreover, the mechanism by which the farmer, community, state, and nation collaborate in safeguarding the basic national heritage in the soil is founded on democratic principles; it safeguards and fosters a great national resource of individual initiative of the people; and it sets the initiative going within national objectves for the general welfare. This mechanism is the Soil Conservation District, which by state enabling legislation becomes a legal subdivision of the state. Its five elected district supervisors are empowered to call on any agency, state or federal, to assist the district in conservation of soil and waters, grass, and woodlands or forests, and in making full use of each resource.

Shepard believes this movement is one of the most significant in the country today by which the findings of science may be put to practical use for the benefit not only of farmers—the custodians of the land—but of the nation as a whole.

How far do the appalling effects of soil erosion, forest devastation, and river pollution call for the exercise of public controls? What will the people say? What will the courts say? Is full conservation possible by private enterprise? What should be done in cases of large-scale land abandonment and tax delinquency? How may land reconstruction or restoration be financed? What is the value of land apart from its selling price? How may public policies in conservation be implemented? These and other questions are discussed against the background of American experience with public works, land acquisition, and the Civilian Conservation Corps.

Land controls the rivers. Integral development of watersheds on the pattern of the Tennessee Valley Authority has many advantages in achieving full use, with conservation of interrelated resources. Objections are examined, but democratic participation can meet these objections and achieve the basically important aims in conservation and sustained use of land, waters, and forests.

And what about conservation and use of resources of the world?

As if in answer to some of the questions raised by Shepard in *Food or Famine,* J. I. Rodale in *Pay Dirt—Farming and Gardening with Composts* hails a revolution in farming. Depleted soils may be restored and improved in their production through composting. Special emphasis is laid on keeping up biological processes of preparing plant foods for crops. Dangers of resorting too readily to chemical farming as against biological farming are stressed. Chemical fertilizers, while furnishing chemical foods for plants, retard or destroy certain biological conditions that are essential to the most satisfactory plant and crop growth. The commercial farming now in vogue built on chemistry without due allowance for soil biological processes and controls is being challenged by practical farmers, soil biologists, agriculturists, nutritional authorities, physicians, and conservationists. Food that must be supplemented by vitamin pills is an indictment of "modern" agriculture.

The author asserts that plants that

get their nutrients through organisms, bacteria, and mycorrhiza rather than through chemical solutions of artificial fertilizers are less susceptible to attacks of diseases and insects. Moreover, such plants are purported to impart to livestock greater immunity to disease. It is alleged that human beings nourished by plants grown on biological nutrients and by animals feeding on such plants are also more immune to disease. This relationship has been claimed before by Russell M. Wilder, but the evidence for so weighty a deduction is not sufficient nor adequately marshalled in this book. While presumption for this conclusion is strong, there is need of further investigation to discover how far and under what circumstances immunity to disease can be traced back to biological and chemical relations of plants to soil.

Much is made of the role of earthworms in preserving fertility of soils. Earthworms feed on humus as they work, and treat soil and make it more productive of plant growth. Accumulations of insecticides in soils kill much of the bacterial life and earthworms, known to be important to maintenance of soil productivity.

Despite more hospitals, more physicians per capita, and the finest medical scientists, working in heavily endowed and elaborately equipped laboratories, draft boards rejected more than 40 percent of the nation's manpower for physical unfitness, most of which was due to malnutrition. And now malnutrition may be traced back to deficiencies in the soil. China, by returning human and animal wastes to the fields, has supported dense populations on the same land for many centuries. But in America the discharge of such wastes into rivers, and thence into the sea, impoverishes soils of mineral elements and perhaps of complicated oganic compounds significant for plant growth.

Is our health related to the soil? is a question raised but not completely answered. This question, however, needs a thoroughgoing answer. It is vital to national and to human welfare the world round—perhaps more than we know to peace of the world.

Pay Dirt is a challenging book; it would be more convincing if experimental results were the foundation of the author's conclusions rather than citations of a wide range of authorities from many parts of the world. This generalized treatment of vital subjects will arouse interest. But there is also badly needed for the general public a clear, concise, nontechnical treatment of the problems and experiments. This is a field of scientific investigation in which the essential discoveries and the results of experiments can and should be put in language that laymen will understand. Thus they will become common knowledge throughout the population.

Rodale in *Pay Dirt* leaves to others the questions of depletion and destruction of the physical body of the soil resource by soil erosion. Accelerated and induced erosion, unless controlled, removes and sorts the soil, separating its fractions by more or less great distances. Advanced gully erosion may cut up land beyond use for cultivation.

Shepard and Rodale, in these two books that largely supplement one another, enliven with prophetic implications the treatment of old and new facts. They should stimulate more discoveries of science and practice in these fields. There is no more important problem before the United States and the world of today and tomorrow than how to grow and increase crops on a limited land area for a growing world population with its augmented demands.

W. C. Lowdermilk
Soil Conservation Service
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Washington, D. C.

SCIENCE ON THE MARCH

THE CHEMICAL TREATMENT OF SEED

ALONG with hybrid corn, mechanized cotton growing, and rustproof wheat, we can include protectant seed treatment as one of the great agricultural developments of the past decade.

Chemical seed treatment is not exactly new. In 1670 a wheat ship was wrecked off the coast of Bristol, England. Economical farmers salvaged the salt-soaked grain and, since it was unfit for breadmaking, used it as seed. Shrewd observation soon revealed that the salted grain produced a crop that was singularly free from the smut disease, and for years thereafter the English wheatgrowers regularly practiced "pickling" their seed in brine. But modern research has led to great advances in both the theory and practice of seed treatment, the success of which is witnessed by wholesale adoption of seed treatment in American crop production.

There are various motives for treating seed. Seed are often contaminated with spores or bacteria of plant disease, a consequence of disease in the seed crop. A principal reason for seed treatment is to destroy these dangerous seedborne sources of disease in the subsequent crop. But even a sanitary seed is not safe when planted in ordinary soil, which contains many aggressive fungi and bacteria that can attack the sprouting seed or delicate seedling before it has become a robust, self-supporting young plant. Chemical seed treatment also has the purpose of surrounding each germinating seed with a protective chemical smoke screen through which the pathogens of the soil cannot penetrate. Seed treatments for these purposes are termed "protectant," are the subject of this discussion, and are to be distinguished from seed treatments with other ends in view.

For example, there is the treatment of leguminous seed with a suspension of the beneficial nodule-bacteria which enable these plants to fix atmospheric nitrogen and restore it to the soil. This poses a problem: if protectant seed treatments are to be used on leguminous seed, some means must be found for securing the protectant action without interfering with the bacterial inoculation.

Then there is the practice of treating some hard-coated types of seeds with corrosive chemicals to soften the seed coat, facilitate entry of water into the seed, and permit its prompt germination. This is a type of seed treatment in frequent use in the propagation of hard-seeded trees and shrubs.

An old farmers' recipe calls for treating corn and peanut seed with kerosene, turpentine, or creosote in order to repel crows and rodents and discourage them from feeding on newly planted seed. This may sometimes succeed in its purpose, but it may have the opposite effect: crows have been observed systematically working down the planted row pulling up every tiny seedling in apparent hope of finding one free from the repellent odor. In this case it would have been less injurious had the bird dined on the first seeds encountered and flown away contented.

Still another type of seed treatment that has made scientific headlines recently—and temporarily—is treatment with growth-promoting substances in the hope of increasing the vigor of growth and yields. The early, enthusiastic reports of hormone seed treatments have not been substantiated by later, more thorough, testing. Seed treatments with insecticides have also been advocated for imparting to the plant a resistance to insect attack. This type of treatment as yet has no scientific basis. Recommenda-

tions for its use include simultaneous nitrogen fertilization of the soil, which alone is probably responsible for the advantages claimed for the seed treatment.

Returning, then, to *protectant* seed treatments, we find their use rather limited until the past decade. A bichloride of mercury dip was in use for seed potatoes and a few types of vegetable seeds, formaldehyde seed treatment was recommended for preventing oat smut, and copper carbonate dusting of seed was the standard control measure for wheat smut. None of these treatments had much value in protecting seed from soilborne plant disease organisms: the mercuric chloride because it was washed off before the seed were planted; the formaldehyde because it evaporated soon after treatment; and the copper carbonate because of its poor quality as a protectant in the soil.

With the introduction of the organic mercury seed disinfectants in the early 1930's there began a new era in seed treatment. These were excellent fungicides against both seed- and soilborne pathogens, and since they were in the form of dusts, the process of treatment was much simplified over the older liquid dip methods, which required subsequent washing or drying of the seed.

For small grains there was a rapid discarding of the formaldehyde and copper-carbonate treatments and adoption of the organic mercury dust treatments. More spectacular was the wholesale adoption of mercury dust treatment for cotton, seed of which had not previously been treated. The figures on cottonseed treatment tell the story. In North Carolina the acreage planted with treated seed rose from 7,000 acres in 1935 to 600,000 in 1939, and in 1941, 87 percent of North Carolina growers used treated seed. Similar enthusiasm was reflected in adoptions across the other Cotton Belt states to Oklahoma, where, in 1942, 80 percent of the growers used treated seed as compared with only 5 percent 3 years before. Such phenomenal adoptions are not to be explained by publicity alone; they could never have occurred had not the cotton farmers themselves become convinced that cottonseed treatment is a form of low-cost insurance that pays rich dividends in the saving of seed, in better stands of seedlings, and in higher yields. In the past it had been customary to plant ten times as much cottonseed as was expected to produce mature plants and then resort to laborious hoeing to thin the irregularly scattered seedlings to an even spacing. Now many of the cotton growers use much less seed and "plant for a stand," in the reasonable expectation that the majority of treated seeds will produce healthy plants. This reduces, or in some cases eliminates, the cost of "chopping," reduces the seed cost, releases cottonseed for oil crushing, and produces stronger, higher-yielding plants.

General seed treatment for vegetables soon followed. The great pea-canning industry led the way, turning 100 percent to the use of seed treatment on the cannery farms. Hybrid corn received such a boost from the treatment that today practically all the seed corn produced in the Corn Belt is pretreated before sale. Workers in the many experiment stations initiated cooperative, uniform vegetable seed treatment tests and soon a flood of bulletins was released, with reports that nearly every kind of vegetable crop profits from protectant treatment. Sometimes the results were nothing short of phenomenal, with spinach stands, for example, increased up to 1,000 percent, and yields proportionately increased when treated seed germinated under conditions favorable for seedling attack by plant disease organisms. Further indication of the acceptance of the practice is seen in the fact that one of the largest mail-order houses now offers for sale only treated vegetable seed.

With these adoptions came marked improvements in the nature of the protectant dusts. If Farmer Jones carelessly leaves the lid of his seedbox open and his cow makes a meal of the treated seed, she no longer dies, as formerly would have been the case when the highly poisonous mercury dusts were used. The newer protectants, organic sulphur compounds, while often of equal or even superior effectiveness, are much less toxic than the mercury compounds. They are less likely to injure the seed if used in overdoses. And, best of all, both the protectant dust and the nitrogen-fixing bacteria can be applied to leguminous seed without interfering with the functioning of the latter.

The work goes on. Still newer, better, seed protectants are in the testing stage. The testing is being extended to include seed which have not previously been treated as a general practice. The effect of treatment on retention of seed viability is being determined. Methods are being developed to determine whether or not commercial seed has been treated. The friction due to dust on seeds, which may interfere with free flowing of the seed through the planter, is being reduced by changes in the composition of the seed protectants. Some states have adopted laws requiring that imported seed be treated.

The amount of seed protectant used is often very small, perhaps .5 ounce per bushel of seed. If this amount of chemical—sometimes too small to be detected by ordinary inspection of the seed—produces such notable results, it was natural to inquire whether increase in the chemical dosage would lead to further advantages. But here a difficulty arose: there is a limit to the amount of dust that will adhere to smooth-coated seeds.

Soon a way was found to by-pass this difficulty. Seed were coated with a sticky layer of quick-drying plastic, into which could be incorporated much larger quantities of seed protectant than could be carried by ordinary seed. This practice of "pelleting" seed has proven valuable in onion culture, and experiments are now under way on a broad scale to determine whether it is advantageous to other crops.

There is much indication that we are rapidly approaching the time when there will be a desirable seed protectant for every major crop, and when seed treatment will be regarded as much a routine in crop production as tillage of the soil.

K. Starr Chester
Oklahoma Agricultural and Mechanical College

COMPRESSION DISTILLATION

One of the interesting and significant developments fostered by the war is the cheap production of very pure water by compression distillation. Water is one of the commonest substances, but its powerful solvent action makes it very difficult to obtain in a high state of purity. The new process, originally developed to produce battery water for submarines, not only gives a product containing less than one part per million of impurities by a single distillation, with sea water as a feed, but at a cost which may be less than 10 cents per hundred gallons.

In 1824 Carnot set forth the principles of the conversion of heat into work, and in 1849 Joule proposed the reverse process as a "heat pump." Since, however, heat, as such, was most easily obtained by the burning of fuel, the only large-scale application of the heat pump for many years was in mechanical refrigeration. Attempts to apply the principle to househeating have been made but have not proved very attractive, because the heat pump employs relatively expensive equipment and is, therefore, best adapted to applications requiring continuous operation at a constant load. Since, also, the power used to deliver a given quan-

tity of heat is proportional to the temperature rise required, the heat pump will show its greatest economy where heat needs to be delivered at a temperature only slightly higher than that of a source from which it can be obtained.

After I had pondered these principles for twenty years, there occurred in 1935 one of those fortunate conjunctions of idea and need that are commonly referred to as "invention." As a result of an investigation by Arthur D. Little, Inc., of problems facing the U. S. Navy, it was found that the Navy needed a better means of obtaining drinking water from sea water, especially on Diesel-driven vessels, without boilers, and for landing operations on desolate islands. I recognized that here were ideal conditions for the application of a heat pump. The theoretical temperature difference above the normal boiling point of fresh water required for distillation of sea water is only about 1° F., and the heat quantities required are enormous. The first crude model built in the Little laboratories produced 4 gallons per hour with a $\frac{3}{4}$-horsepower motor, or an expenditure of only 80 B.T.U. of energy per pound of distillate, as against 300 for good quadruple-effect evaporator plants. Comdr. J. O. Huse was sent to inspect the plant and at once visualized its possibilities for submarines. After nearly two years of negotiations, an experimental unit was delivered to the U. S. Naval Engineering Experiment Station at Annapolis, followed by a shipboard unit a year later, and in March 1941 production models began to roll off the assembly lines of E. B. Badger & Sons Co.

Normal operation of a compression still is preceded by heating to bring the apparatus to operating temperature and fill the evaporation compartment with steam. In regular operation, sea-water feed enters through a triple-passage heat exchanger in which it extracts heat from the outgoing distillate and the brine carrying away waste salts. The feed enters the evaporator at about 207° F. and mixes with a relatively large volume of brine circulating naturally. From the evaporator, steam is led through an entrainment separator to the compressor, which raises its pressure to about 3 pounds per square inch gauge. At this pressure, the steam condenses at about 222° F. Since the brine boils at 213° F., there is a 9° temperature differential to permit transfer of the latent heat from the condensing steam to the boiling brine. No separate condenser or cooling water is needed. In some of the more recent models, more than 175 pounds of distillate have been produced for each pound of fuel used.

The first units went into the new submarines being built in 1941, but by December of that year a considerable number of units for older submarines were in the Pacific area and several were damaged on the wharves at Pearl Harbor.

Meanwhile, mobile, gasoline-engine-driven units were developed for the Marine Corps and for the Army. These units became familiar and welcome sights to those who took part in the invasions of the Pacific islands and of North Africa.

Although originally developed primarily as a means of saving precious fuel in distant places and on shipboard, this process, like most fundamentally sound processes, proved to have many other characteristics which may be of even greater interest than its economy of power. Thus the units developed by Arthur D. Little, Inc., and E. B. Badger & Sons Co. are simple to operate, requiring control of only two valves, one for starting and one for control of the rate of feed of raw water. Any Navy engine-room rating can learn the operation in ten minutes. Another rather unexpected result is the extreme purity of the product, which, when made direct from sea water, has less than one-tenth

of the impurities allowable under Navy standards for double-distilled battery water. It is also sterile, since all the vapor is heated by compression to over 240° F.

The industrial possibilities of extremely pure water at low cost are enormous. In the photographic field alone there is a great opportunity for better and cheaper products. Pharmaceutical and other chemical industries, high-pressure steam plants, railroads, sugar refineries, and even textile factories, paper mills, and laundries in hard-water regions, may find distilled water within their reach.

Compression distillation is in its infancy. It has gone to war and made good. The same principles and techniques will be converted to peacetime use and will make pure water available practically anywhere on earth and for any purpose where real advantages would result from its use.

R. V. KLEINSCHMIDT
ARTHUR D. LITTLE, INC.

INDIAN PICTOGRAPHS AND PETROGLYPHS

OF THE many branches of archeology none appeals to the layman so much as "picture writing." First noticed by the early explorers of our country, who told of the strange hieroglyphics they had seen, they were extremely interesting to our first citizens and were believed to represent some unknown language. For many years attempts were made to decipher these "writings" by comparison with Chinese, Egyptian, and other scripts. No fewer than six hundred articles and books were written concerning a site in Massachusetts known as Dighton Rock. All such attempts were futile because the level of writing had not been reached by the primitive artists, but even today in some sections of the country I have found people still trying to decipher the pictures in the hope of reading the message.

No doubt most readers have seen examples of paintings (pictographs) and carvings (petroglyphs) in the western areas in the National Parks, where they are carefully preserved. They may even be encountered on a Sunday afternoon's drive in the country. The sites commonly consist of geometric designs or pictures of animal and human forms, which were pecked, carved, or painted upon the hard surface of a canyon wall, a boulder, or a cave ceiling. The paintings may be in any of a number of colors, in many cases still as bright as they were a hundred years ago. These pictures were drawn to represent hunting scenes or to celebrate certain events; some are mere sketches with no more meaning than the scribbles on the wall of a phone booth today. The sites occur in almost every state where the proper rock surface presents itself and where Indians lived or traveled. More than 10,000 sites exist in America today, and there were probably twice as many a century ago. Some of the pictures are very crude and have no artistic value, whereas several sites in the Southwest have painted designs of animals that equal in artistry any to be found in the caves of Europe. The same care should be given these

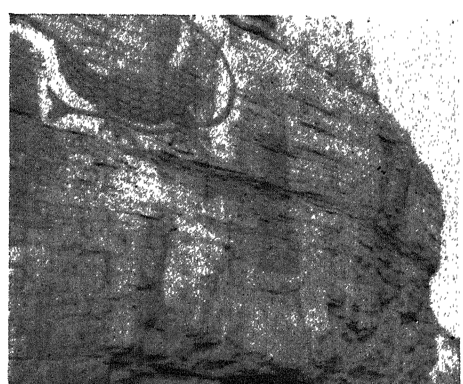


Photo by B. W. Stephens

PETROGLYPH AT ALTON, ILL.

SITUATED ON A MISSISSIPPI RIVER BLUFF, IT WAS NOTED BY LEWIS AND CLARK IN THEIR TRAVELS. UNFORTUNATELY, IT WAS DESTROYED.

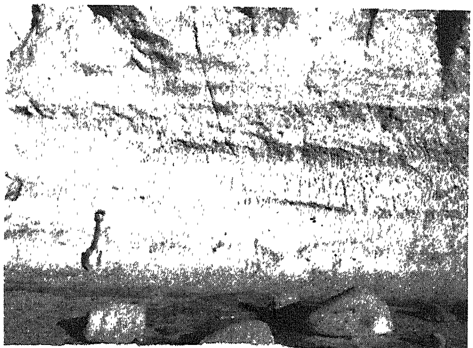


Photo by National Park Service

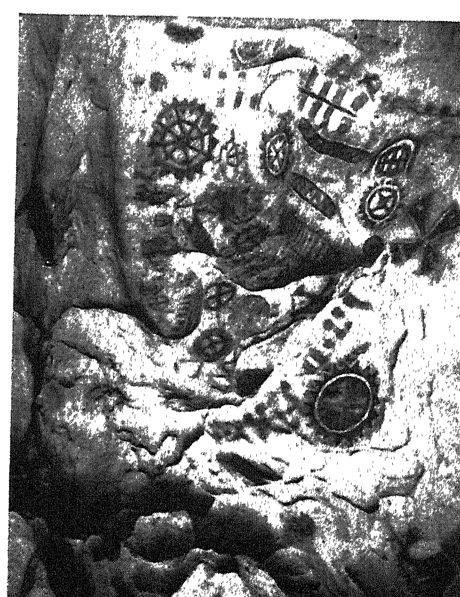
SOME PETROGLYPHS THAT SHOULD BE PRESERVED

drawings as has been given to those in Europe.

The value of these pictures or petroglyphs is very great from a scientific standpoint. They represent the first examples of early American art in this country; their designs often give clues to religious ceremonies and tribal life not found elsewhere; finally, their very presence is often the only remaining evidence that Indians inhabited certain areas.

Because of the complex nature of petroglyphs and of their relation to other cultural remains, the field has been greatly neglected. Many early American archèologists merely noted the presence of unusual designs and failed to record adequately the less imposing sites. Recently, however, many papers have been published on the subject. J. H. Steward has studied many sites in far-Western states. In California he found some two hundred sites where the figures were present in great numbers. From the seeming haze of figures, circles, dots, animals, etc., Steward was able to discover differences in both design and technique in different areas. The possibility of correlating sites by a study of design elements led to a new interest in the subject. In other states workers began to rummage through old files seeking photographs and reports, and in the field a search began for new, unreported sites. Also it was noted that there were certain designs that could be called typical of various areas and that continued study of correlation would show cultural migration and infusion. Some of the workers went further and compared the petroglyph designs with those on pottery from the same area.

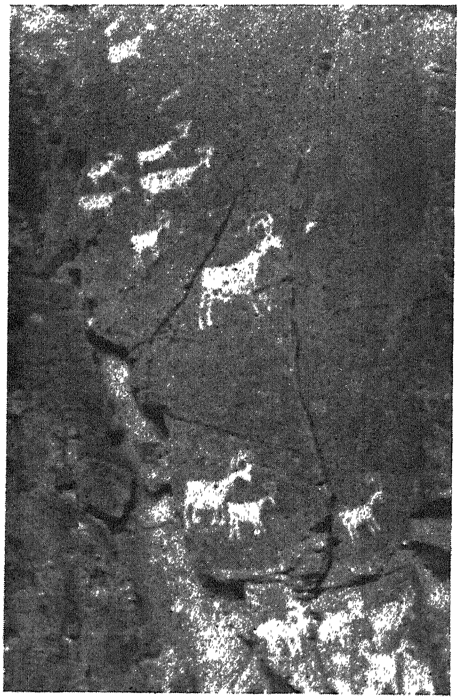
The possibilities of a thorough study of the subject are enormous. At present the most exact methods of dating sites depend upon use of pottery rela-



From J. H. Steward

PAINTED CAVE, SANTA BARBARA

NOTE DESECRATION OF DESIGNS BY INITIALS. THIS SITE IS NOW UNDER OFFICIAL PROTECTION.



ANIMAL PICTURES IN NEVADA

tions and tree rings. If it were possible to correlate petroglyphs with these, then for those localities lacking pottery or timber the designs would help to solve the culture problem.

The task facing the archeologist today is to locate the petroglyph sites, note the various designs, and by means of statistical methods to correlate, first, the different sites, and then the petroglyph designs with those to be found on pottery, hides, bones, etc. When this job is completed a great advance in this field will have been made.

While working on a survey of sites in the United States I have seen their rapid destruction by man and nature. In the eastern section of the country practically all the sites known fifty years ago are now gone or defaced. The loss of this material makes an accurate study of this region impossible. Most of the destruction has been due to the building of homes and roads in regions once inhabited by Indian tribes. Unfortunately, in many cases no record was made of the sites. Today those remaining are also in danger of the destruction caused by natural elements or the Sunday traveler. For example, in one case a schoolteacher took a class of children to a site, furnished them with paint, and allowed them to cover the petroglyphs with their own daubs, thus ruining a good site. In other instances vandals have carved their names over the designs or tried to blow the designs off the surface of the rock with rifles during target practice.

Scientists of all kinds—not necessarily archeologists alone—by cooperation and by means of their professional standing in their communities, can do much toward the preservation of this material so vital to the complete story of mankind's development in America. Local sites should be pointed out to the proper authorities, who will then see that the information comes to the attention of trained investigators. In this way, for generations of Americans yet unborn, their rightful heritage of a valuable historical record will be made more secure.

R. M. TATUM

ANNAPOLIS, MD.

VALEDICTION

ARTHUR MANGUN BANTA

Dr. Arthur M. Banta, Emeritus Professor of Biology in Brown University, died on January 2, 1946, at the age of sixty-eight. He had retired from active service in June 1945, planning to devote all his time to the conduct of his research.

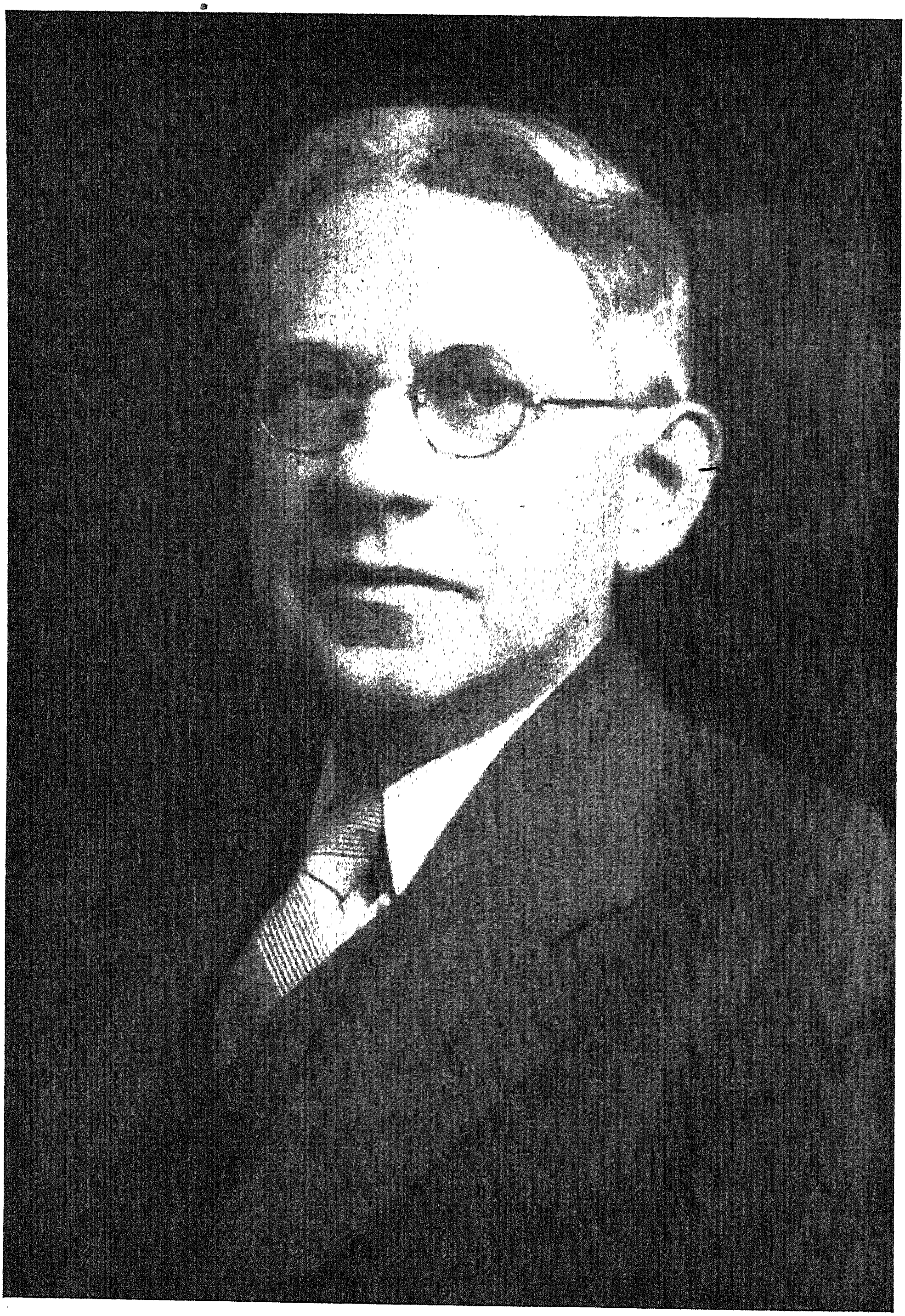
I first met Dr. Banta in 1916 when at the end of my sophomore year I went with H. E. Walter to the summer laboratory at Cold Spring Harbor. Dr. Banta was there at the Station for Experimental Evolution of the Carnegie Institution. Our class in Field Zoology was taken to the Station to see Dr. Banta's cave. As he told us about his experiments, his friendliness and enthusiasm made a lasting impression on me that has grown with the more intimate association of recent years. A sincere and cultured gentleman, a competent scientist, he became a valued member of our faculty at Brown.

The artificial cave beneath the main laboratory at Cold Spring Harbor was the outcome of a project he started as a graduate student at the University of Indiana. He had come under the influence of C. H. Eigenmann who set him the task of studying the fauna of Mayfield's cave. His holidays were spent in making an exhaustive study of the inhabitants of this cave and of their habits. This led him to the problem of the origin of these forms with their striking adaptations to a peculiar habitat. The question of the role of the environment in the transformation of organisms confronts the investigator most insistently in the study of these adaptations. It was this question that became the focus of his research interest for life. His study was later published as *Carnegie Institution of Washington Publication #67* (1907).

He took his Ph.D. degree at Harvard in 1907 and for two years was Professor of Biology at Marietta College. When Dr. C. B. Davenport became director of the station for Experimental Evolution at Cold Spring Harbor, the work of Dr. Banta fitted so well into his program that he invited him to become a member of the staff and built for him the artificial cave under the main building. Working in this cave, Dr. Banta attempted through generations of breeding to produce inheritable adaptations in forms usually living in the light. He used crustaceans especially. He finally became convinced that within the conditions of his experiment permanent transformation of the organisms would not occur.

He was led thus to study variation and inheritance in Crustacea and selected the small, rapidly-breeding Cladocera as material. Their parthenogenetic reproduction made it possible to establish clones and thus to investigate the variation within a genetically uniform stock. In a relatively short time many generations with large numbers of individuals could be examined. *Publication #305* (1921) of the Carnegie Institution contains the results of his attempts at selection within clones on the basis of a single physiological character—reaction to light. His results he interpreted in terms of mutations.

His work with the Cladocera led him to an investigation of the role of environmental factors in the control of the reproductive processes. By control of such factors he was able to induce the production of males and of sexual reproduction in parthenogenetic lines. Many years of work on this and other related subjects were summarized in his monograph



ARTHUR MANGUN BANTA (1877–1946)

Carnegie Institution Publication #513 (1939). These books summarize and analyze the many papers that were the product of his life of research.

He came to Brown University in 1929 as visiting professor to take charge of my courses in Experimental Biology during my leave of absence. On my return he was made a permanent member of the staff and from then to his retirement taught courses in Experimental Evolution and in the Biology of Sex to enthusiastic classes of advanced students. During these years he had many graduate students who worked for the most part on problems connected with his research on the Cladocera. His students and colleagues found him always interested in them, and never too busy to discuss their problems with them.

On his retirement, he planned to continue his research on the Dauermodification—or "carry-over effects"—effects that endure over several generations after the causative factor has ceased to operate. His preliminary observations on this had been discussed in his monograph. He had obtained a grant of funds to carry on the work and was optimistically settling down in new quarters in our Biology Annex to many years of concentrated work. It is indeed unfortunate that his plans were not realized. The loss to science in such unfinished projects is irreparable, for the momentum and insight that comes with long years of familiarity and experience with the material and problems may never come again.

His death was wholly unexpected by all of us. He seemed in full vigor of health. Throughout the war he had conducted a successful and outstanding victory garden along with the rest of the faculty gardeners on the periphery of the college baseball field. We were looking forward with pleasure to having his kindly influence in our academic family for years to come. With deep regret we record his passing.

J. Walter Wilson

Brown University

COMMENTS AND CRITICISMS

On Grant's Fish Story

I have been following the explanations put forward by writers as to what caused the formation of the Carolina Bays.

The latest article, appearing in THE SCIENTIFIC MONTHLY for December 1945, by Chapman Grant, "A Biological Explanation of the Carolina Bays," in my opinion, fails to explain the formation of these flat, sandy basins, which would attract spawning fish.

To me there would seem to be a simple cause of these silted areas and I am quoting from the above article to prove my point. According to Grant on page 444: "The shallows received some surface runoff and in addition it has *numerous artesian springs* welling up through holes in the impervious layer. These springs were of various sizes, but were not necessarily large."

If there are numerous springs in this area it is reasonable to suppose that in the last great ice age they were both large and active and, because of land slippage, they would well up and change position from time to time. Hence, the depressions filled with sand that are so numerous in this area. Tidal current constant in direction would account for the shapes; drainage of the area would also help. The irregular bays are to be expected where harder strata is met with which would resist the scouring action of the springs.

Anyone who will examine areas where even the smallest spring occurs in a flat, porous expanse will see bays formed by these springs similar in many respects to the Carolina Bays.—KENNETH C. ALEXANDER.

I had thought that I was the only contributor to the MONTHLY who told big fish tales, but Mr. Chapman Grant has left me at the post. I know a good deal about fish behavior, particularly the breeding and nesting habits, but his explanation of the Carolina Bays is a new thing to me. I suspect that Professor Johnson, who before his death was for many years at Columbia University, has been making rapid revolutions in his grave at this explanation. There must be hundreds, if not thousands, of "bays" in the eastern part of North and South Carolina and probably in the northeastern part of Georgia. To make all those "bays," the fishes must certainly have worked overtime—Saturdays, Sundays, and nights—to do this job.

I spent ten summers at the Fisheries Laboratory at Beaufort, N. C., and seined all over the sounds round about. I know the fishes of the region pretty well, but I never found any of them behaving as Mr. Grant suggests, even back from the sounds in the fresh-water inlets. I also spent four summers at the Carnegie Institution Laboratory, Dry Tortugas, Fla., and watched fishes there in the shallow bays and inlets in the coral islands, but I have never seen anything like this explanation of Grant's nor have I found anything in the literature, although I have been a fish bibliographer for forty years.—E. W. GUDGER.

As a newly elected member of the AAAS, I was badly jolted to read in your December 1945 issue "A Biological Explanation of the Carolina Bays" by Chapman Grant. Certainly an article which shows such a scant knowledge of geological processes and such a studied disregard of biological data should not be published in THE SCIENTIFIC MONTHLY on a subject which is essentially geological. It simply excites ridicule for the publication and lowers the standing of the AAAS as a serious scientific body.

It is quite true that the origin of the Carolina Bays has not been satisfactorily explained by the theories advanced to date by Johnson, Melton, and others. However, the geologists and other scientists who have heretofore attacked the problem have given earnest scientific consideration to all the conditions which obtained at the time of the development of the "bays," and have drawn their conclusions with due regard to these conditions. Thereby, an accumulation of fact and scientific deduction has been built up which may well assist in a final solution of the problem. To this accumulation, Grant's fantastic statement adds nothing but a confusing cloud of irrational conclusions which are given an aura of authenticity by appearing in a publication which contains the word "scientific" in its title.

It is my opinion that the technical publication of a society which purports to advance the cause of science, should take every precaution to publish only those papers which a qualified group of men have selected to carry out the purposes of the society.—JOHN L. FERGUSON.

Perhaps our readers should be reminded that we rarely publish new and untested hypotheses. Major Grant's fish story seemed to us to be such a tour de force of the imagination that we could not resist it. So our knuckles have been rapped again. Our future experiments in the SM will be made in other directions.—ED.

Humanism

In a very interesting letter (SM, January 1946) on "The Social Significance of Jewish-Christian Intermarriage" George Wolff advocates the merging of the Jews with the peoples with whom they live by means of biologic union, accompanied by a "spiritual fusion" to be brought about by an "actual reconciliation and reunion of the Old and New Faith."

While the intermarriage part of the program is perfectly clear and one which appeals as an eminently rational approach to the resolving of the age-old strains of Jewish-Christian relationships, that part of the program relating to a "reconciliation and reunion" of "the Old and New Faith" seems vague and impractical. It is difficult to see how such a reconciliation can take place unless the Jews adopt, or the Christians give up, Jesus, either of which would not be a reconciliation but rather a conversion.

From a practical, as well as from a scientific viewpoint, another method of attaining the desired "spiritual fusion" would seem far preferable. As Wolff remarks, both the Jewish and Christian religions have the same basic ethical ideals; and from any objective modern standpoint it must be conceded that the moral content of religions is the only lasting contribution that they have made to man's advancement. However important the myths, fables, and folklore which make up the supernaturalist trappings of religions and which encrust their ethical ideals may have seemed in the past, they have now ceased to have any further social value—unless the furnishing of a livelihood for their professional advocates may be so termed.

Thus the discarding by both Jews and Christians of all supernatural, traditional, and folkway elements of their faiths, and the uniting of their identical moral code, recognizing it as the natural result of man's social progress, and divorcing it from fanciful ideas of a "divine" origin, would not only "spiritually fuse" Jews and Christians, but would bring the religious ideas of both squarely in line with current scientific knowledge. They would, as scientific humanists, then be able to live together harmoniously. Their respective religious myths would be relegated to the archives of history along with those of other ancient peoples, instead of being constantly reiterated by interested groups for the purpose, or at least with the result, of creating discord and ill will.

This method of approach would not only solve the so-called "Jewish problem," but also the "Protestant problem" and the "Catholic problem," with great benefit to all concerned. And if the Western world would have enough good sense to adopt this solution, perhaps it would serve as an example for the Near Eastern as well as the Oriental peoples similarly to solve the "Mohammedan problem," the "Hindu problem," the "Buddhist problem," and all the other "problems" raised by religions, by which, from time immemorial, the peoples of the world have been divided, and have been kept divided, into antagonistic groups.—HAROLD R. RAFTON.

Jest for Fun

In the public prints a few weeks ago I noted a United Press dispatch, originating from somewhere in New Jersey, that gave me serious pause and that I think may be of interest to those of your readers who are also writers of one sort or another. It read as follows: "The late Albert Payson Terhune is continuing his literary work beyond the grave, his widow said today. Already he has dictated three books to her, she said."

This came, you understand, right on the heels of the atomic bomb, and being still somewhat stunned by that disaster, I was a bit slow, I fear, in fully realizing the implications of this second calamity. Finally, however, I was impelled to address a few lines to Mr. Terhune himself and to others of the supernatural brotherhood who also may be carrying on empyreal literary activities and in whom the well-known *cacoëthes scribendi* may be threatening to break out in new and dangerous ectoplasmic rashes. My hope is that the apprehension that the following couplets convey may, through your widely read pages, reach the addressees in time to do some good:

Souls of writers dead and gone,
What Elysium have ye known . . . ?
When your friends laid you away,
Weren't you laid away to stay,
Or, in manner ghostly clever,
Are you going to write forever?
Even though we like your stuff,
Isn't one lifetime enough?
Must there be, without restriction,
All this otherworldly fiction,
Or, indeed, what might be worse,
Ultra-cemetery verse?
Shakespeare, Milton, Jonson (Ben),
Are you going to start again?
Goethe, Dickens, Eugène Sue,
Won't there be an end of *you?*
And there's this predicament—
Where shall royalties be sent?
To avoid such complications
Please seek other occupations;
Dead men tell no tales, you know . . .
Anyhow that *once* was so.
Incorporeal authors, han'ts,
Give us living guys a chance!

PAUL H. OEHSER

THE BROWNSTONE TOWER

April has been officially designated as the month during which an attempt will be made to arouse the nation to fight cancer with all its available resources of money, knowledge, and intelligence—to fight cancer with cold fury and warm cash as no disease was ever fought before. Can there be any mature person who has not seen a relative or friend struck down in his prime by cancer? To the agony of the victim is added the terrible helpless distress of the one who loves him most. The time comes when morphine must be used to ease the way to the permanent relief of death. But there is no relief for the memory of those who sat by the bedside—no relief except to fight cancer in person or by giving financial support for education and research to the American Cancer Society.

In our leading article in this issue Dr. E. V. Cowdry has explained the complexity of the cancer problem. It seems unlikely that a single specific drug or treatment will be found for the group of diseases called cancer. But, on the other hand, it seems certain that intensive cooperative research on predisposing factors, early detection of the disease, and possible treatments will disclose ways and means of arresting cancer supplemental to, or replacing, methods now in use. Let no one suppose that surgery and X-ray treatments, even when promptly applied, guarantee protection against the spread or recrudescence of cancer. Too often they offer only hope, to be followed by bitter disappointment.

The success of cooperative research on weapons, particularly the development of the atomic bomb, has caused people to believe that any complex problem can be solved by scientists if a large enough number of them are put to work on it and given millions of dollars of borrowed money. Let me point out that complex biological problems, such as cancer, are likely to be more recalcitrant than difficult physical problems because the former involve a larger number of factors, some only vaguely recognized and some that are extremely difficult to measure. Although large sums of money are necessary for cancer research, it is still more important to bring into the work the most talented investigators available and to steer promising young men toward a career in such research. As the procedures of the Civil Service Commission were not designed to discover and recruit scientific talent and as some good investigators do not thrive in Government laboratories, it would be inadvisable to depend entirely on the Government for the prosecution of cancer research. Furthermore, the spending of Government money, except in time of war, is not conducive to a sense of urgency and responsibility for results. Therefore, it is highly desirable that a large part of future cancer research be done in private institutions by means of funds privately contributed. In this way also contributors will have the satisfaction of personal participation in a war against cruel and premature death.

Although the American Cancer Society was founded in 1913, it was not until last year that any substantial sum was raised for a nationwide attack on cancer. From $4,000,000 contributed in 1945 a large amount was appropriated for research for the first time. In 1946 the Society plans to raise $12,000,000 of which 60 percent will be retained within the contributing states for cancer education and improvement of medical services to cancer patients. A part of the remaining 40 percent will finance a national program of cancer research under the supervision of a committee of the National Research Council. Thus a scientific general staff will tie together the work done in different institutions. If the campaign is successful, no capable investigator or no promising lead in research should lack effective financial support.

F. L. Campbell

THE SCIENTIFIC MONTHLY

MAY 1946

AMERICAN ROAD MAPS AND GUIDES

By WALTER W. RISTOW

MAP DIVISION, NEW YORK PUBLIC LIBRARY

TO MANY Americans the end of the war, with the removal of restrictions on gasoline, tires, and automobiles, means freedom to travel. Soon the concrete and asphalt arteries of our land, virtually deserted since 1941, will be pulsating again with the movement of millions of cars. It is interesting to note that this return to the highways comes as the automobile industry is celebrating its golden jubilee.

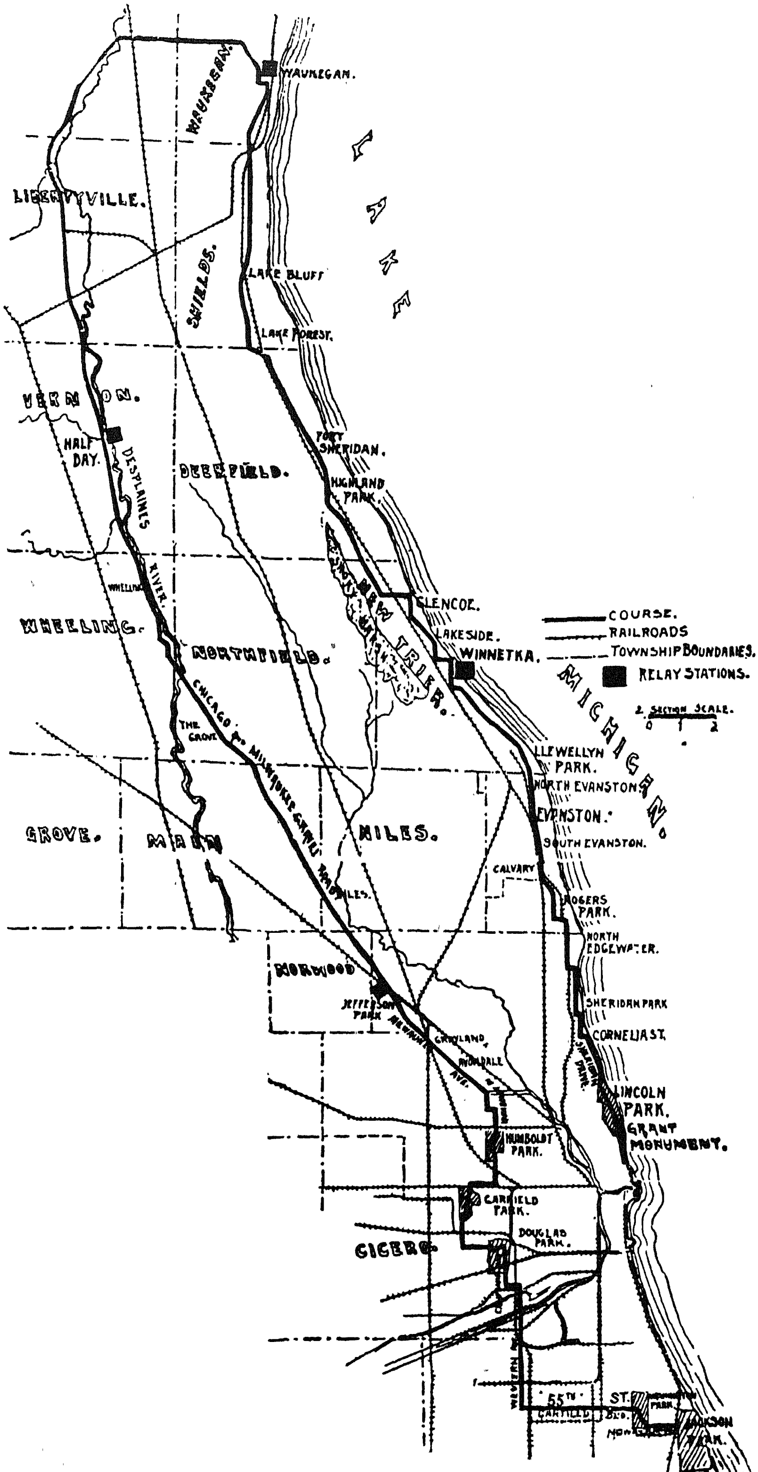
A half-century ago, on Thanksgiving Day 1895, the *Chicago Times-Herald* sponsored a $5,000 auto race, and this historic event, which attracted a number of contestants, decisively demonstrated the superiority of the gasoline car over other types of self-propelled vehicles. Winner of the race was J. Frank Duryea, who drove a car designed by himself and his brother Charles.

The 1895 race was originally planned for November 2, but postponement was necessary to the later date. However, a consolation race was held on November 2, with a Mueller-Benz and the Duryea car the only contestants. The latter ended up in a ditch in a vain attempt to dodge a farm wagon, but the Mueller-Benz completed the 92-mile course from Jackson Park, Chicago, to Waukegan, and back to Lincoln Park.

A map printed in the *Times-Herald*, tracing the course of the November 2 race, is believed to be the first road map prepared for the specific use of American motorists. It was reprinted in the initial number of the *Horseless Age* magazine of November 1895. The route of the Thanksgiving Day race was also outlined on a map in the *Times-Herald* and reprinted in the December 1895 *Horseless Age*. Thus, automobile road maps, too, celebrate their fiftieth anniversary.

The story of the automobile during the past 50 years is an interesting one, and the parallel development and evolution of the American automobile road map is no less fascinating. The excellent and attractive maps distributed free by the millions today are as superior to the two just described as are the modern streamlined passenger cars in comparison with the gas buggy driven by Frank Duryea half a century ago.

As we travel over the miles of excellent highways of the United States today, it seems incredible that less than 50 years ago hard-surfaced roads existed only in and around the larger cities. This was a result of the almost complete dependence upon railroads for long-distance transportation during the last half of the nineteenth century. During these years, highway construction was almost nonexistent. Under such conditions, of course, there was no need of road maps,



THE FIRST AMERICAN AUTOMOBILE ROAD MAP, 1895
FROM THE *Horseless Age*, NOVEMBER 1895. ORIGINALLY PRINTED IN THE *Chicago Times-Herald*.

and virtually all the transportation maps of this period feature railroads and disregard roads entirely.

Shortly before 1890, with the invention and improvement of the new "safety" bicycle, there appeared the first faint indications of a new era in transportation. The growing popularity of the sport of cycling, principally among fashionable and wealthy young men and women in the eastern cities, gave birth to the revolutionary idea of traveling for pleasure.

But to these pioneer cyclists, "traveling for pleasure" was an ideal seldom attained. The roads were in wretched condition, highways unmarked, road maps nonexistent or unintelligible, and the country folk antagonistic or, at best, indifferent to the misfortunes and tribulations of the amateur wheelmen. Following a hazardous Sunday outing, perhaps, one contemporary cyclist, Wilder Grahame (*Good Roads,* **2**(3): 199. 1892) was moved to compose the following lines:

The pathway of life may be narrow and steep,
But the roads through the country are steeper.
The pitfalls and snares that beset us are deep,
But the mud that surrounds us is deeper.

Receiving no help from outside sources, the bicyclists organized in 1880 as the League of American Wheelmen, and thus united they carried on a vigorous campaign for improving and marking the public roads. Travelers in the eastern United States also profited from the pioneer efforts of the L.A.W. in publishing road maps and guides.

Fortunately, by 1890 the United States Geological Survey had published topographic quadrangles for large portions of the northeastern states, and these maps were used by cyclists and also constituted the source of information for most commercial maps and guides. Walker's sectional maps of New England, New Jersey, and New York, widely used by cyclists during the nineties, were almost exact copies of the government maps.

In 1888 Charles G. Huntington brought out *The Cyclist's Road-Book of Connecticut* for the Connecticut division of the L.A.W. This publication, which included 9 individual county maps, bicycle laws, lists of hotels, road directions, and other useful information, may have been the first of the modern American tourist guidebooks. A similar work *The Cyclist's Road Book of New Jersey* was published by Henry A. Benedict in 1890.

Other guidebooks issued by and for the L.A.W. and covering New England, New York, Pennsylvania, and New Jersey soon appeared. As the cyclists increased in numbers, various private publishers also prepared maps and guides, which ranged in price from 25 cents to \$2.00. Rand McNally & Company, still one of the leading names in the road map field, published road maps and guides for cyclists as early as 1894.

After 1905 few, if any, bicycle maps and guides were published, for a new wheeled vehicle, the automobile, had won the interest and affection of the amateur mechanics and young sportsmen. The L.A.W. lost members and influence as the new motor-propelled vehicles increased in number and quality. But to the pioneers of that energetic organization motorists owe a lasting debt of gratitude, for it was largely because of the improved highways resulting from the L.A.W. "Good Roads" movement that the early experimental motor cars were able to move easily through the countryside.

Self-propelled vehicles were in existence as early as the beginning of the nineteenth century, but it was not until 1895 that the manufacture of automobiles really became established. For the next 10 years bicycling and motoring shared the interest and attention of wealthy and adventurous young Ameri-

cans. It was natural that the early motorists should avail themselves of the maps and guides prepared originally for cyclists. They, however, soon found these inadequate, for, as one automobile enthusiast of this period noted: "Road maps are not very helpful, as they are usually made for wheelmen. A road may be good for bicycles, as they need only a narrow strip, but an automobile must have wide wagon roads."

Following the example of the cyclists, automobile owners united in 1899 as The Automobile Club of America. Limited at first to motorists within the New York area, the Club soon expanded to include members in other parts of the country. With the American Automobile Association (founded in 1902), with which it later merged, the A.C.A. continued the campaign for better roads and maps started by the League of American Wheelmen.

In 1900 the Automobile Club of America issued its first guidebook, which was described in the *Automobile Magazine* as containing "a vast amount of information that should prove valuable to all users of motor vehicles. It may be said that among other things, it contains . . . road ordinances, legal opinions on automobiles, lists of road books and maps, etc." This early yearbook unfortunately did not include maps, and several more years passed before the A.C.A. entered the map publishing field.

A year later, in 1901, the *Official Automobile Blue Book* made its first appearance. Covering eastern United States, this guidebook included a wealth of useful information, as well as four carefully prepared maps. It set a standard and a pattern which was followed by a number of other guidebook publishers.

Although a few road maps had been issued separately by 1904, most of them before this date were printed in, or as supplements to, guidebooks. From this period, however, individual road maps were prepared by a number of different organizations. In this year Rand McNally published its *New Automobile Road Map of New York City and Vicinity.*

One of the earliest maps issued by the American Automobile Association is a small road map of Staten Island, N. Y., which is dated 1905. The same year the Automobile Club of America started publishing road maps, which were prepared by the George H. Walker Company, of Boston.

Guidebooks continued in popularity despite the increasing importance of maps, and in 1905 the Hartford Rubber Company published a very complete route book called *Automobile Good Roads and Tours,* which sold for $2.00. Covering the northeastern states, this guidebook included a number of page-size maps and detailed touring information.

The first *Official Automobile Blue Book* to carry the endorsement of the A.A.A. appeared in 1906. This popular guide was issued annually until 1926, when it was replaced by the current *AAA Tour Books.* It held top rank among automobile guidebooks during the years it was published.

The Hammond Map Company, one of the early producers of bicycle maps, entered the auto map field in 1906 with a *Road Map of Northern and Central New Jesey.* During the next 25 years the Hammond Company published a long list of motorist maps and guides before abandoning this phase of cartography about 1930.

Automobile and tire manufacturers early recognized the business value of preparing guidebooks for sale or free distribution to their customers. Among the former concerns was the White Motor Company of Cleveland, who in 1907–08 issued a series of *White Route Books* covering various sections of the

Across and Around About Staten Island.

Staten Island (Borough of Richmond, New York City) is best known to motorists as the first stage of the shortest line of road to New Brunswick, Trenton, Philadelphia, and points beyond. Though requiring two ferry transfers as against one across the North River direct to New Jersey, there is a material saving of distance, and the gain of a much less complicated route for, approximately, the first third of the way to Philadelphia. Many tours are planned to go one way and return another.

New York-St. George-Tottenville Line.

Chiefly important is the practically level country road between the opposite extremities of the island — St. George and Tottenville—nearly straight and in excellent condition throughout. This is one main line of the island: From South Ferry, Whitehall St., New York City, take (half hour) ferry across bay to St. George, leaving the ferry slip by left exit. Go straight out to where the way ahead is blocked by irregularity of first cross streets, where bend left and follow Shore Road through Tompkinsville and Stapleton to Clifton, all small places. Entering Clifton turn right on splendid macadam road — Vanderbilt Ave. — direct to New Dorp. Here take left turn (sign) into Amboy Road — a direct, unbroken line across the center of the island, the railroad taking the same general course, with now and then a crossing.

Continue on Amboy Road through Oakwood, Giffords, Eltingville, Annadale, Huguenot, Princess Bay, Pleasant Plains, and Richmond Valley to

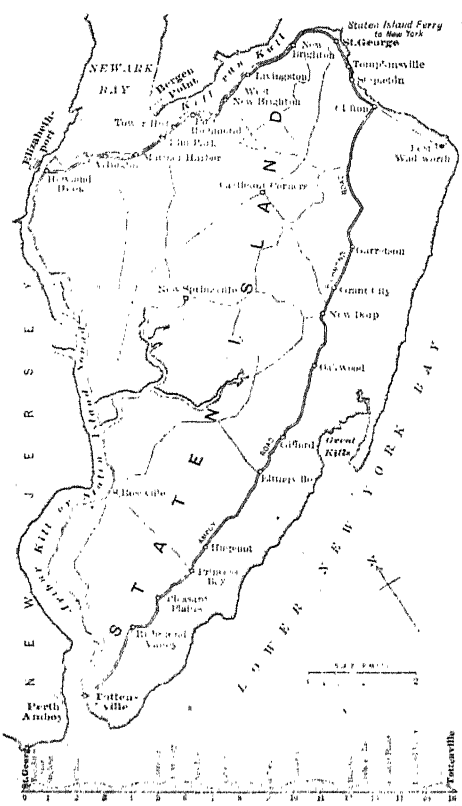
16 miles from St. George. Tottenville.
21 miles from Central Park, N. Y.

NOTE. At Tottenville Village leave Amboy Road, keeping Totten St. to Bentley St. to ferry. Amboy Road ends at private dock below ferry, necessitating return and possibly losing boat, if figuring on time of leaving.

The continuation of this run to New Brunswick, N. J., and beyond will be found in full detail in the New York-Philadelphia routes, and somewhat condensed in the tours to and from the Jersey Coast resorts.

Other Connections from New York.

In other ways Staten Island may become a part of tours from New York and vicinity into New Jersey. After the landing at St. George, one may turn right (instead of left as before) and follow upper Shore Road through New Brighton, Port Richmond,



Main road from St. George to Tottenville is the first part of a short line between New York and Philadelphia, the Jersey coast resorts, etc. Diagram shows grades across the island

HOW TO ARRIVE IN 1905

TOURING INFORMATION AND ROAD MAP FROM A 1905 AUTOMOBILE GUIDEBOOK. (*Automobile Good Roads and Tours*, COMPILED AND PUBLISHED BY HARTFORD RUBBER WORKS CO., HARTFORD, CONN.)

country. Their popularity is noted by a statement in the preface to one edition which boasts that "the circulation of these books is more than twice that of any other publication containing road directions."

The *White Route Book* included one double-page map, photographs, and detailed touring directions. Nothing was left to chance, as the following excerpt will testify:

> Mile 21.5. At the end of the road turn right (school house on the right, church on the left), 23. Turn off to the left with wires (old sign board on the right). 24.7. Turn right with wires, passing on left, after turn, large red barn marked *James White.*

There is no price marked on the *White Tour Books*, and it is possible that they were given to customers. If this is true, it was perhaps the earliest free distribution of published road information to motorists.

By 1909 automobile maps and guides had become quite common, and the *Automobile Club of America Tour Book* of that year lists 20 different maps published by the A.C.A., in addition to a number of guides and maps of other publishers. Many of these covered individual states and were compiled or endorsed by local automobile clubs.

The Goodrich Company followed the

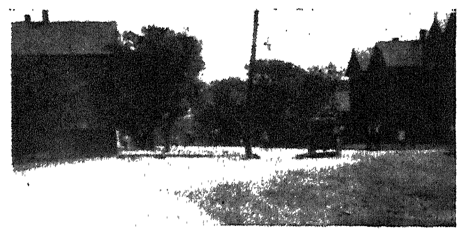
example of the Hartford Rubber Company and from 1912 to 1915 published a series of *Goodrich Route Books*. Including sectional maps of the territory covered, they followed the plan of the *White Route Books* in listing mileages along with detailed directional information. All sections of the United States were covered by the guides, which were distributed to tourists gratis by the Goodrich Company.

Throughout the period 1910 to 1930 there was stiff competition in the preparation of automobile guidebooks, and many novel ideas were tried out. For a time the preferred approach was to outline specific tours, as are found in the *Associated Tours* booklets published by the A.C.A. from 1916 to 1931. Many hotels, resorts, and communities likewise prepared route books aimed to attract tourists to their particular locality.

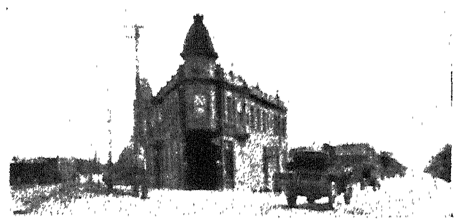
ROUTE NUMBER 55
CHICAGO TO SOUTH BEND
110 MILES

.0 Leaving the Auditorium Annex go south on Michigan Avenue
1 Pass on the right the Chicago branch of **THE WHITE COMPANY**, 240 Michigan Avenue
1 Go under railroad viaduct
2.8 At the red light, turn left into 33d Street
3.1 At the red light turn right into South Park Avenue (church on the right of turn)
3.4 Bear slightly to the right on Grand Boulevard, passing large watering trough on the right
4. Go under the railroad
5.4 Enter Washington Park passing Washington statue on the right
5.6 Pass road to left
6. Pass two roads to right which lead from the park and at triple fork beyond take left road
6.2 Pass road to left
6.4 Pass road to left and then pass power house
6.7 Pass road to left and at the road immediately beyond turn left into "The Midway"
7.7 Bear slightly to right under viaduct and one block beyond go straight ahead into Jackson Park
8.3 At the fork with circular grass plot in the angle, take the left road
8.5 At the next fork keep to the right
8.7 Pass road to right
8.8 At the next fork take the right road
9. Leaving the park keep straight ahead
9.5 Cross the railroad at **BRYN MAWR** station
9.8 Cross trolley
10. Cross trolley
10.2 Cross the railroad
10.6 Cross trolley

11.1 Make sharp hairpin turn, meeting the car tracks, and immediately turn away from them
12.1 Near railroad bear to the right keeping on macadam road
12.3 Bear to the right keeping on macadam road



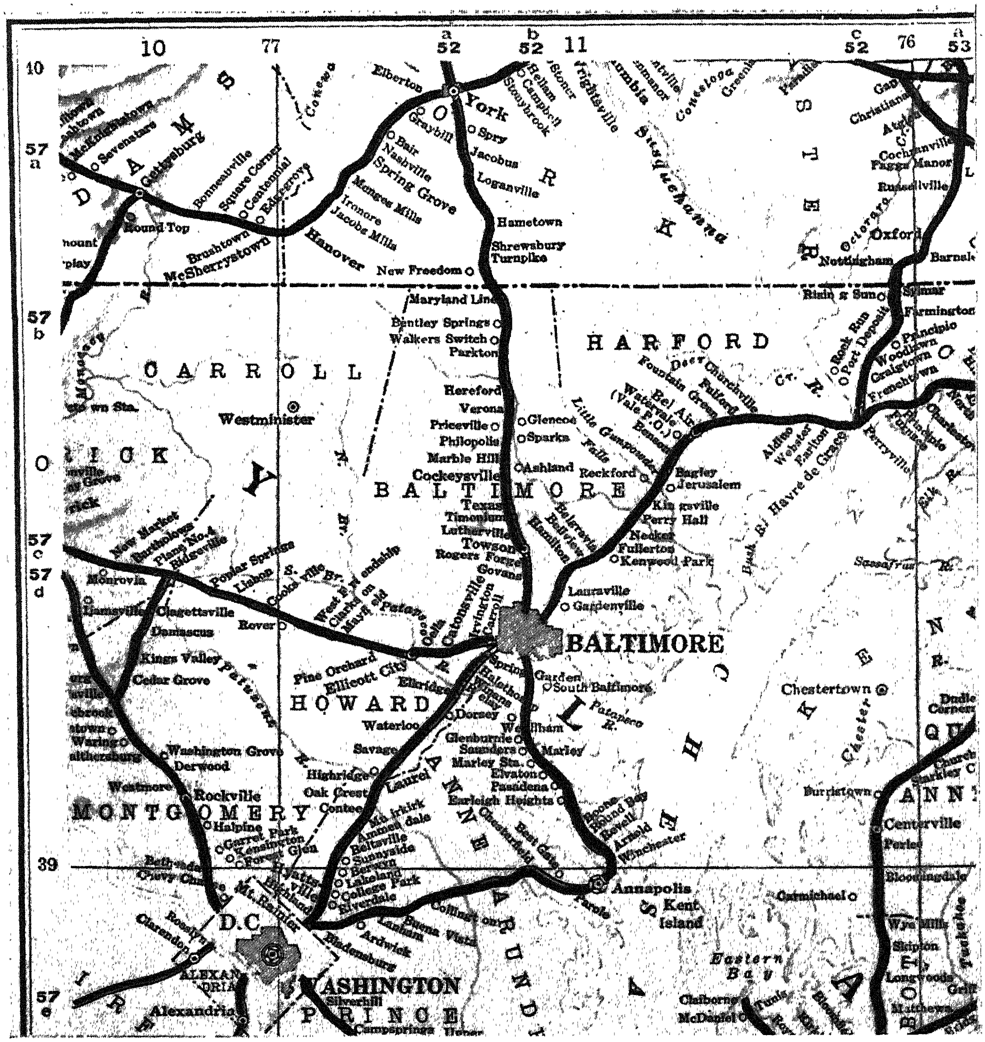
12.7 Jog slightly to the right onto asphalt street
13.4 At the South Chicago Hotel turn left into 92d Street. **SOUTH CHICAGO**
13.6 Cross the railroad
13.9 Cross the railroad (lumber yard on the right)
14. Cross drawbridge over the Calumet River
14.3 Cross railroad side track



14.4 At the fork with saloon in the angle, take the right road leaving the trolley
14.9 Cross three railroads; at the next street turn left passing the East Side Station and at the next corner turn sharp right meeting the trolley. At the next street turn 60° left and follow the trolley
15.7 Cross the railroad
15.8 Cross the railroad
15.9 Just beyond the plant of the Columbia Malting Company, cross the state line from Illinois into Indiana
16.9 Cross railroad side track
17. Cross small iron bridge

FOR A FASTER RATE IN 1908

THE MOTORIST WHO USED THIS PRECISE GUIDEBOOK USUALLY REACHED HIS DESTINATION. (FROM *White Route Book*, NUMBER 7. COPYRIGHT 1908 BY THE WHITE COMPANY, CLEVELAND, OHIO.)

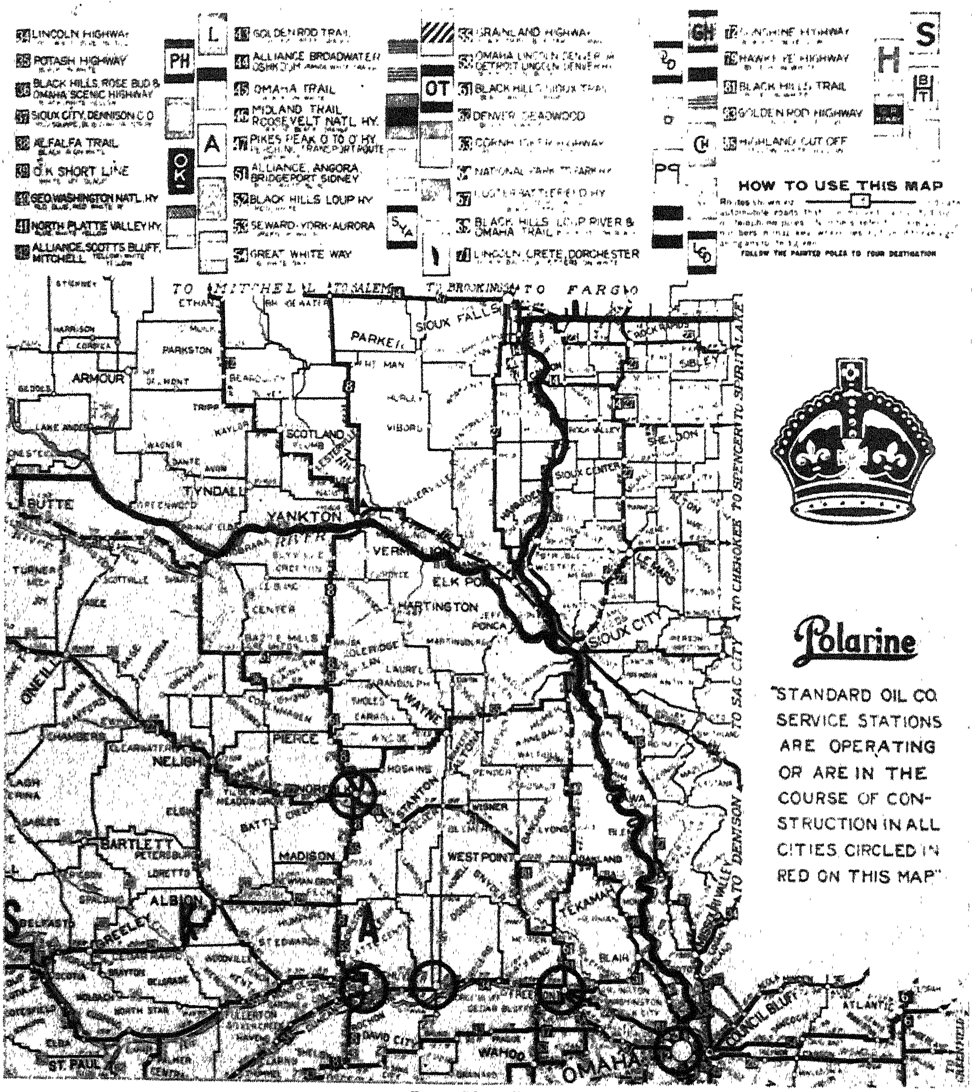


A PART OF AN EARLY COLORED ROAD MAP, 1914

DURING THE 1910'S THE NATIONAL HIGHWAYS ASSOCIATION CONDUCTED A VIGOROUS CAMPAIGN TO DEVELOP ROAD MAPS AND IMPROVE ROADS FOR MOTORISTS. (FROM THE *N.H.A. Tour Book*, 1914.)

In 1911 the National Highways Association was organized and shortly started its campaign to improve and mark the highways of the nation. During the seven or eight years of its greatest activity this organization issued a number of excellent road maps, featuring the several National Highways which crossed the country from north to south and from east to west. The N.H.A. also pioneered in publishing a uniform set of individual road maps for all the states. For the part it took in the improvement of roads and road maps in the United States, the National Highways Association ranks with the League of American Wheelmen.

The year 1913 is significant in the history of road maps. In that year William B. Akin, a Pittsburgh advertising man, sold the Gulf Oil Company on the idea of sending road maps to automobile



Base Map Copyright by Rand McNally & Company, Chicago

THE PERIOD OF THE BLAZED TRAIL, 1921

ALTHOUGH THE FIRST FREE OIL COMPANY ROAD MAP WAS ISSUED IN 1913, THE PRACTICE DID NOT BECOME WELL ESTABLISHED UNTIL THE 1920'S. THIS RAND MCNALLY ROAD MAP, WHICH IS SHOWN IN PART, WAS PRINTED IN TWO COLORS FOR THE STANDARD OIL COMPANY OF NEBRASKA IN 1921.

owners as an advertising scheme. As an initial step, 10,000 road maps of Allegheny County were mailed to motorists in the Pittsburgh area. The following year state maps were prepared, and 300,000 road maps of New York, New Jersey, New England, and Pennsylvania were distributed gratis through Gulf Service Stations. Thus was established a practice which motorists today take entirely for granted and which has placed road map publishing in the ranks of big business.

For several years Gulf monopolized the free map field, and in 1920 this one company gave away more than 16,000,-

000 maps. Within the next few years, however, the other large oil companies recognized the promotional value of free maps and added them to the list of services offered by their stations.

During the second decade of the twentieth century, the American Automobile Association strengthened its position as the leading motor organization in the United States. In 1917 this club issued a series of strip maps covering major highways, but the idea was shortly abandoned in favor of state maps. The A.A.A. state maps soon attained a reputation for their accuracy, and they are today distributed to the number of 2,-500,000 yearly.

As early as 1900 certain of the states had established highway bureaus and issued maps of the road systems. It was not until the 1920's, however, that state departments began free distribution of road maps to motorists. Recognition of the potential profit in attracting tourists within their borders has led virtually every state in the union to adopt this practice. These state highway folders are today frequently attractively illustrated with pictures of the scenic centers or major industries.

The policy of free map distribution had its repercussions among map makers. Prior to 1917 road maps and guides were a motley lot and were prepared by a great number of printers and publishers. Small concerns were able to issue satisfactory maps because touring was still largely limited to short distances, and the necessary road information could thus be easily gathered. Furthermore, the maps of this early period were simple black-and-white affairs, of the type which any small print shop could produce, and they were issued in quantities not beyond the capacities of such shops.

Expansion of the practice of giving maps away forced small publishers to abandon road map printing. Only the larger map concerns were equipped to fill orders for millions of road maps. Such companies also had the financial strength to engage field workers for compiling road information in all parts of the country. The perfection of photo-offset color printing and its adaptation to map making further eliminated smaller concerns because of the high cost of such equipment.

Rand McNally and Company had advanced to a position of leadership in the road map field by 1917. In that year the firm published the first of its automobile guidebooks, the forerunner of the present large *Road Atlas* which is issued annually. Also about 1917 Rand McNally started a campaign for numbering and marking the highways, and this resulted in their series of *Auto Trails Maps,* or *Guides to the Blazed Trails.* These were in the form of large maps covering all or portions of several states, folded to accompany a booklet which listed hotels, garages, and repair shops. By the early 1920's Rand McNally road maps were being printed in several colors, the state unit had been substituted for regional maps, and the maps had taken on a "modern" appearance.

Because of the recognized excellence of the *Blazed Trail Maps,* the Gulf Oil Company in 1918 engaged Rand McNally to prepare maps for free distribution at Gulf Service Stations. For a number of years Rand McNally had no serious competitor in producing maps for oil companies, and at present it remains one of the "big three" among road map publishers. In the prewar period approximately 50,000,000 maps were prepared annually by Rand McNally for several of the large oil companies.

In 1923 the General Drafting Company, of New York City, began making road maps for the Standard Oil Company of New Jersey and the following year took on a similar project for Socony-Vacuum. General Drafting's annual pre-Pearl Harbor output of road

maps (principally for the above two companies) was over 20,000,000.

The third of the "big three," The H. M. Goushá Company, of Chicago, was established in 1926 by several former employees of Rand McNally. It is the only one of the three engaged exclusively in road map production. Between 50–70 million road maps were published annually by Goushá prior to 1942.

The above three companies and a few smaller concerns put out some 150,000,000 road maps before the war for annual distribution through gasoline stations. In addition, the American Automobile Association and the various state highway bureaus disposed of another 10,000,000 free maps. The oil companies pay about 3 cents per copy for the maps, and for the larger companies this item in the annual budget may amount to more than $200,000.

Since Pearl Harbor road maps have been among the minor casualties of war. With our entry into the conflict, road map printers, along with most other map publishers, started working for the Army, and for the past four years their presses have been turning out military maps.

Before the 1946 touring season gets under way, however, free road maps should again be available, perhaps in greater quantities and of better quality than in the prewar period. The 12,000,000 or more servicemen and women, who had intensive training in map reading and who have used the excellent maps of foreign lands, will expect improved road maps. Likewise the millions of us who remained at home have acquired a better appreciation of maps through studying atlases, war maps, and newspaper maps.

Notwithstanding the new emphasis on travel by air, it is fairly certain that for some years to come most of us will do our cross-country touring in earthbound vehicles. And to guide us on our way we will continue to rely upon the familiar oil company road map. Though he may cuss them for an occasional inaccuracy or omission, the American tourist looks upon the road map as a friend in need. For, although road maps have undergone many changes in form during the past 50 years, fundamentally their function is the same. To the cross-country traveler of 1946, as to the gas buggy enthusiast of 1895, they are still indispensable guides and companions in his wanderings over the face of the earth.

CATALYSIS IN INDUSTRY, BIOLOGY, AND MEDICINE

By JEROME ALEXANDER

During World War I, C. F. Kettering and Thomas Midgley, Jr., of the Research Department of General Motors, in the course of their collaboration with the U. S. Bureau of Mines to produce an aviation gasoline as free from "knock" as possible, made extensive engine tests with a variety of volatile organic liquids. The comparatively rare hydrocarbon cyclohexane was found to be a superior airplane engine fuel, and the suggestion was made that its manufacture be attempted. Dr. Leo H. Baekeland, then a member of the Naval Consulting Board, regarded the project as impractical and advised against it. He even promised a wooden medal to Kettering and his collaborators if they could make a single pint of cyclohexane.

Nothing daunted, the investigators applied to this problem their knowledge and skill in the preparation and use of catalysts for the hydrogenation of benzene and in a comparatively short time sent Dr. Baekeland a liter bottle of cyclohexane ensconsed in a plush-lined mahogany casket. Baekeland kept this on his desk for many years as a prized possession. And since the product had been made by CATalysis, Dr. Baekeland was furnished with a design for the wooden medal he had promised (Fig. 1).

Today, catalysts and catalysis have achieved extensive publicity. The average newspaper reader, even if he does not know what a catalyst is, does know that 100-octane gasoline, which gives our aviators speed and ceiling, is produced with the aid of catalysts. The more technically informed reader knows that the catalytic production of sulfuric acid, ammonia, nitric acid, and wood alcohol is an old story. In fact, Germany was able to start World War I in 1914 only because she had just then become independent of Chile as a source of nitrates, owing to German perfection of the Haber process, whereby nitrogen and hydrogen can be catalytically combined to produce ammonia. This, in turn, can be catalytically oxidized to nitric acid, the basis of most explosives as well as

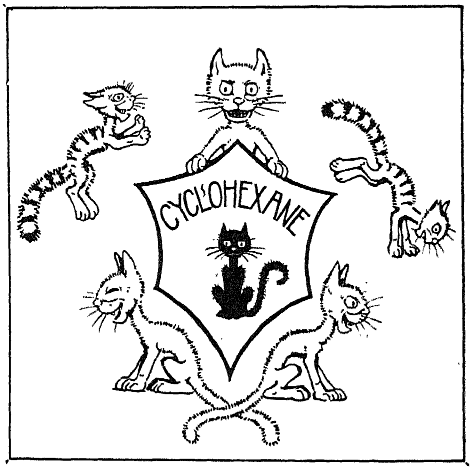


FIG. 1. CATALYSIS
DESIGN OF A WOODEN MEDAL FOR KETTERING. SUGGESTED TO BAEKELAND, WHO DID NOT BELIEVE CYCLOHEXANE COULD BE MANUFACTURED.

a vital ingredient in fertilizers. Those connected with the chemical industry or profession know that an ever-increasing number of useful and valuable organic compounds are now being catalytically produced, some in immense quantities. For example, the 1944 catalytic output of phthalic anhydride, used in making plastics, was over 62,000 tons. In 1936 about 250 tons of nickel were sold in the United States for

catalytic use, about two-thirds having been used for hardening vegetable oils by catalytic hydrogenation to produce a huge tonnage of the well-known edible fats, which, like lard, are nonfluid at room temperature.

To illustrate the magnitude and importance of catalytic processes in the petroleum industry, consider in outline the recently perfected "fluid-catalyst" cracking process, devised by long and expensive cooperative research undertaken by large petroleum refiners, and now operating in about 30 plants (Fig. 2).

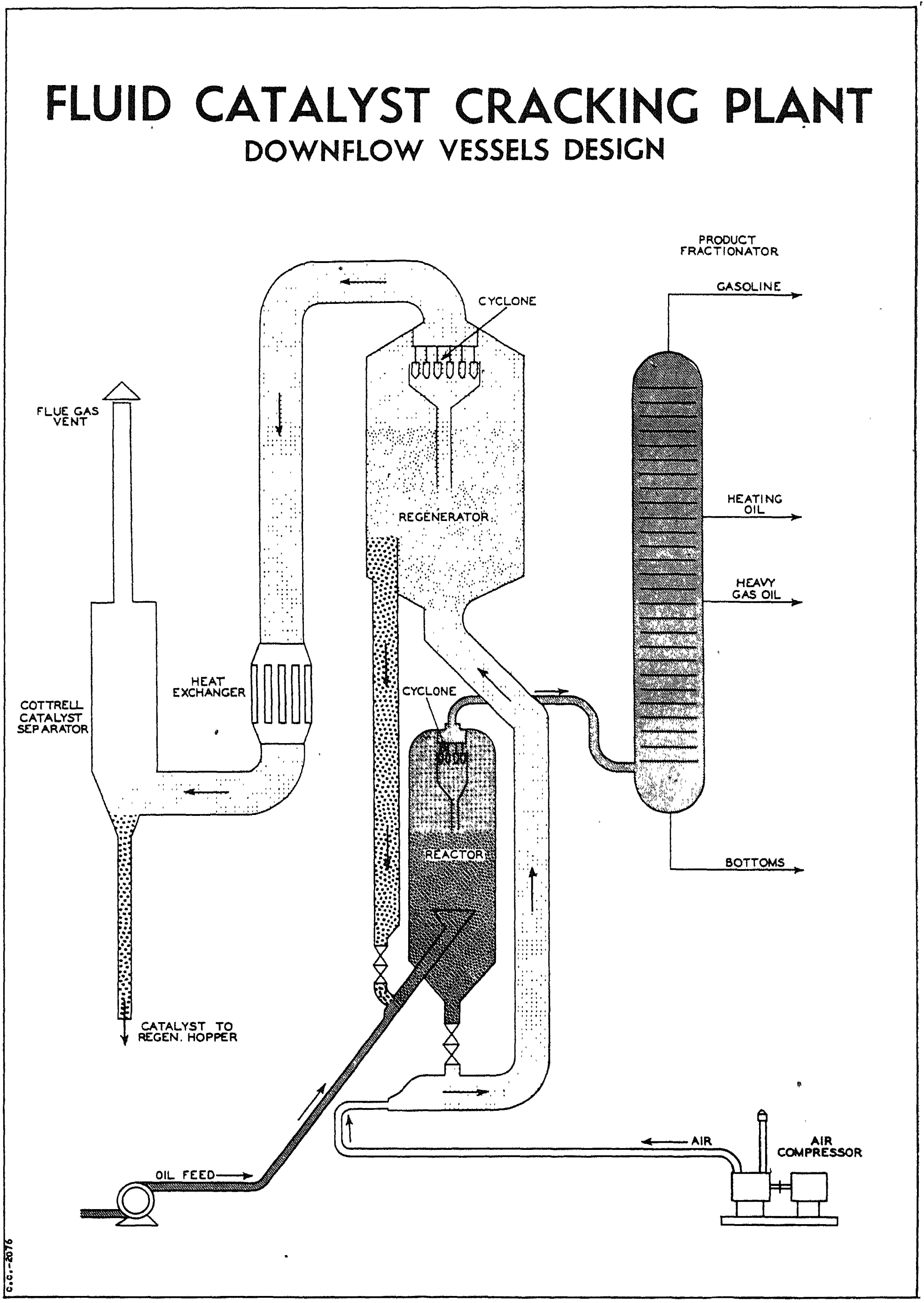
From a standpipe about 200 feet high, a claylike powdered catalyst cascades into a stream of vaporized oil at the rate of about 2 carloads a minute, and the torrid, oily duststorm swirls like a gas into a reactor vessel where, at the huge catalyst surface, there take place the complex chemical transformations termed "cracking." The cracked reaction products are separated from the now blackened, carbon-coated catalyst, which then falls into a stream of incoming air and is carried to a regenerator where the carbon is burned off. The revivified catalyst is returned to the standpipe for reuse at the rate of about 40 tons a minute. About 73,000 tons of catalyst are consumed annually in this process, which has been yielding daily (after certain additions) over 400,000 barrels of 100-octane gasoline. Incidentally, there are also produced certain raw materials for synthetic rubber.

Germany developed the chemistry of coal and coal tar—because she had coal. American chemists realize that besides coal we have in our enormous supplies of natural gas and petroleum important raw materials for a great and novel organic chemical industry, in which catalysts are finding ever-increasing use.

Origin of the Term "Catalysis." A little over a century ago the great Swedish physican and chemist Jöns Jakob Berzelius introduced to science the word "catalysis," and it is well to consider his original definition, uncontaminated by subsequent *obiter dicta* (*Jahresberichte,* 1836, **15**, 237):

> It is then proved that several simple and compound bodies, soluble and insoluble, have the property of exercising on other bodies an action very different from chemical affinity. By means of this action they produce, in these bodies, decompositions of these elements and different recombinations of these same elements to which they themselves remain indifferent.
>
> This new force, which was hitherto unknown, is common to organic and inorganic nature. I do not believe that it is a force quite independent of the electrochemical affinities of matter; I believe, on the contrary, that it is only a new manifestation of them; but since we cannot see their connection and mutual dependence, it will be more convenient to designate the force by a separate name. I will therefore call this force the catalytic force, and will call catalysis the decomposition of bodies by this force, in the same way that one calls by the name analysis the decomposition of bodies by chemical affinity.

Even though recent X-ray research has given to catalytic forces a local habitation as well as a name, Berzelius outlined, in essence, the present view. For example, in 1935 J. Monteath Robertson in Sir William Bragg's laboratory at The Royal Institution, by an elaborate mathematical development of the X-ray data, established the structure and electronic contours of phthalocyanine and nickel phthalocyanine (Figs. 3 and 4). Note how the introduction of a single metal atom (nickel) produces a marked change in the electronic contour at the center. Figure 5 gives a "surveyor's map" of nickel phthalocyanine, the molecule lying flat in the plane of the paper, the electronic maps being foreshortened because the molecules are tilted in the crystal at the angle shown. If we remove the 4 symmetrically placed benzene rings from the periphery of these molecules, we have left an interior ring of 4 pyrrole groups similar to what



Courtesy Standard Oil Co. of N. J.

FIG. 2. CATALYTIC CRACKING OF PETROLEUM

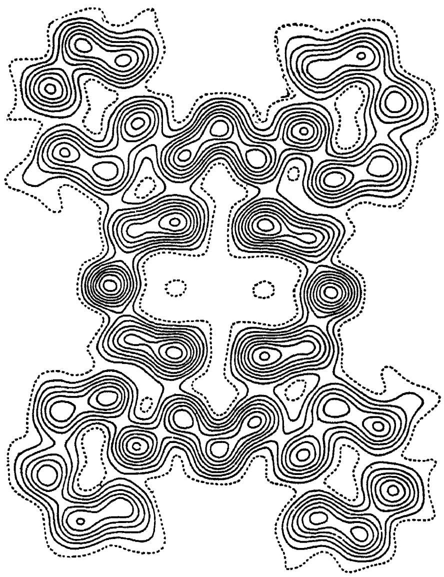


FIG. 3. PHTHALOCYANINE
STRUCTURE AND ELECTRONIC CONTOURS AS COMPUTED FROM X-RAY DATA BY J. M. ROBERTSON.

is found in the porphyrin of hemoglobin (the oxygen-carrier of red blood cells, where the central metal atom is iron) and chlorophyll (a basic catalyst in green plant cells, where the central metal atom is magnesium). Then in 1938 A. H. Cook found that iron phthalocyanine alone of all the metallic derivatives tried is able to act as a catalyst in the decomposition of hydrogen peroxide; that is, it could function as does the biological catalyst, or enzyme, catalase.

These printed molecular diagrams give merely an inkling of the extreme specificity of the submicroscopic structure responsible for the unique effects of catalysts. And of course they fail to show Brownian motion or the rapid fluctuations in the molecular and atomic electronic fields; that is, the extensive particulate and intraparticulate activity developed by catalysts in action. They do, however, give some idea of the electromagnetic "auras" surounding molecules and indicate the almost infinite variety of electromagnetic "keys" that may be formed to open or to close specific electronic "locks"; that is, to break down (analyze) or to build up (synthesize) specific substances.

In attempting to give a reasonable explanation of the catalytic processes which constitute the basis of so many biological as well as technological processes, we should recall the remark of Daniel Webster relative to political matters, that nothing ever turns up unless someone turns it up. Though catalysts cannot direct lasting chemical changes in defiance of chemical affinities, they very often determine what and when and where specific reactions will occur and their relative velocities.

How Catalysts Function. Catalysts function by virtue of their specific outwardly directed electronic fields of force, which bring about characteristic distortion, or warping, of the fields in susceptible particles (atoms, molecules, ions) that approach close enough and remain long enough to be sufficiently

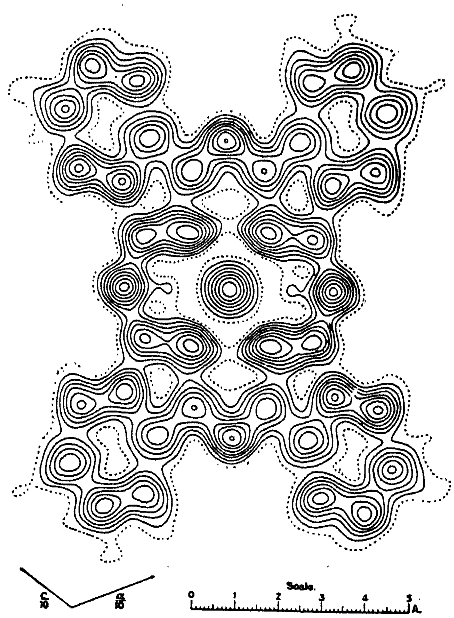
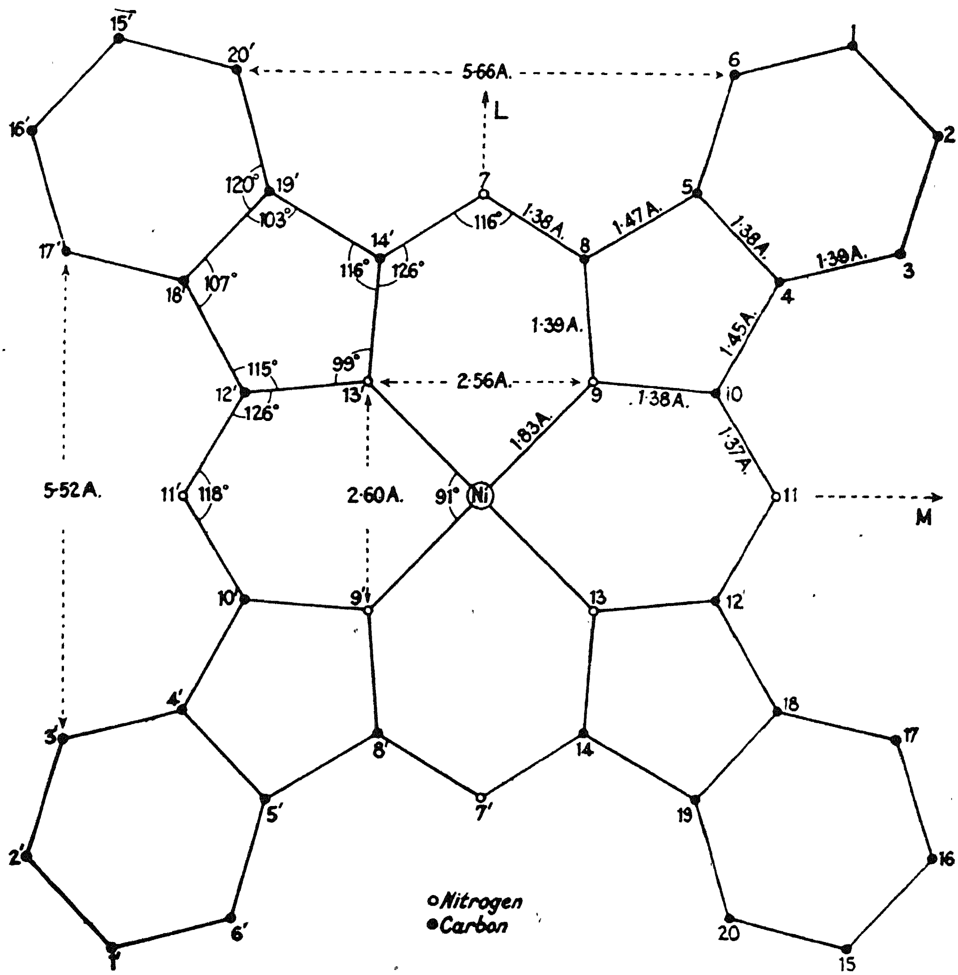


FIG. 4. NICKEL PHTHALOCYANINE
STRUCTURE AND ELECTRONIC CONTOURS AS COMPUTED FROM X-RAY DATA BY J. M. ROBERTSON.

affected to bring about a synthesis or a breakdown. As a result of this distortion, or "activation," the particle may be able to combine with other particles, or it may be split into smaller fractions. the opposing "hooks" so that they are linked together; drawn in the opposite direction, the "key" releases the bond between the opposing "hooks" and the closure is opened. Within rather close



From J. M. Robertson

FIG. 5. DIMENSIONS OF THE NICKEL PHTHALOCYANINE MOLECULE

Catalysts thus function like a judge, who may wed or divorce qualified couples who come within his jurisdiction and remain there long enough for the operation of due process of law. Another analogy is that the catalyst functions like the "key" of a zipper closure: drawn in one direction, the "key" massages limits the key and the hooks must be mutually adjusted to each other. An adhering bit of paint or solder or a mechanical deformation of key or hooks may prevent proper functioning. Similarly, catalysts may be "poisoned" for certain reactions by physical or chemical change or by adsorbed impurities.

It is well known by those who use catalysts industrially, that the presence of very small amounts—even traces—of particular substances may make a catalyst function exceedingly well or may completely alter the chemical output of the catalyst. Where such additions are made intentionally to obtain an increased yield of a desired product, they are called "promotors." Sir Gilbert T. Morgan found that when a mixture of carbon monoxide and hydrogen was passed over a catalyst consisting of equimolecular amounts of manganese dioxide and chromic oxide, there was a yield of 80.5 percent of methyl alcohol; but when 15 percent of rubidium was added to the catalyst, the yield of methyl alcohol, under the same operative conditions, dropped to 41.5 percent, while large amounts of different, more complex compounds appeared in the output. By varying the nature and amount of what is added to the catalyst, a great variety of complicated organic compounds may be produced, even under identical operative conditions, from these two gases.

In a way this is a corollary of the well-known fact that the nature of chemical change may vary sharply with the nature of the catalyst. For example, with formic acid the breakdown varies as follows:

Catalyst	Resulting Products
(1) Pd, Pt, Cr, Ni,	$H.COOH \rightarrow H_2 + CO_2$
Cd, ZnO	$H.COOH \rightarrow CO + H_2O$
(2) TiO_2, W_2O_5	$H.COOH \rightarrow H.CHO$
(3) SiO_2, ZrO, UO_2	(formaldehyde) +
	$CO_2 + H_2O$

Thoria can catalyze any of these three types of decomposition, depending on the temperature.

Catalysts in Biology. Because a promotor alters the chemical output of a catalyst, it may in a more general sense be termed a "modifier," a term free from any connotations as to the desirability of the result. Since even traces of material may serve to modify, or, as a limiting case, to form a catalyst, the importance of catalyst modification in biological events is apparent; for it is the biocatalysts that determine the nature, proportions, and the rates and places of production of the manifold chemical substances underlying all biological structures and behaviors, both normal and abnormal. Much of the recorded work in biology and medicine describes the consequences of catalyst changes, without considering the comparatively simple principle whereby the extensive and extremely diverse results may be readily understood.

When biochemistry was developing, a favorite gibe of orthodox organic chemists, whose experimental ambit was often the flask, the still, and the autoclave, was: "Whenever a new biological reaction is to be explained, a new enzyme is invented." What was then said in jest is in the main correct, for we have become increasingly aware of the numerous cases in which biocatalysts are brought into being and activity. What had long been known as Warburg's "yellow enzyme" was quite recently reported by O. Warburg and W. Christian (*Biochem. Z.*, 1938, **298**, 150–168; *ibid.*, 1939, **301**, 231–2) to be an artifact resulting from loss of adenylic acid during its preparation. These authors describe five different yellow enzymes of the type of alloxazine dinucleotide, some having similar proteins but different prosthetic groups, others having the same prosthetic group combined with different protein carriers. They estimate that one molecule of dinucleotide can transfer 1,440 molecules of oxygen per minute. D. Keilin and T. Mann (*Proc. Roy. Soc.*, Series B, 1937, **122**, 119–133) have shown that "the same haematin nucleus combined with three different proteins forms three distinct compounds: methaemoglobin, catalase, and peroxidase, which

have many properties in common but show, however, striking differences in . . . their activities.''

Just recently S. Granick and H. Gilder (*Science,* 1945, **101**, 540), reporting on the structure, function, and inhibitory action of porphyrins, find that for growth *Hemophilus influenzae* requires hematin, the iron compound of protoporphyrin, which is characterized by its side chains. Though the bacillus cannot synthesize protoporphyrin, it can insert iron into this compound and thus produce the catalyst needed to make oxygen available to the organism and control its metabolism. The bacillus cannot insert iron into porphyrins without vinyl groups, but the latter ''could compete with the iron porphyrins for the combination with the specific proteins which go to make up the heme enzymes. Such a porphyrin would thus be a natural inhibitor, and in a sense a regulator governing the degree of anaerobic versus aerobic metabolism.''

The term ''prosthetic group'' requires some explanation. The adjective, derived from Greek roots, means to supply missing parts. Thus prosthetic dentistry deals with the insertion of missing teeth. The prosthetic group (it may be an atom, an ion, a molecule, or a larger particle) puts ''teeth'' into the catalyst enzyme and helps give it its specific ''bite.'' The same prosthetic group may be bandied about among various ''carriers,'' reminding one of the Norse fairy tale of the solitary tooth that had to serve each of the three weird sisters. On the other hand, the same carrier may be able to accept any of several prosthetic groups, so that from a few basic units many catalysts of diverse effectiveness may be formed.

What are regarded as very ''simple'' chemical changes in ''simple'' organisms, may actually be the outcome of complicated reaction series, mediated by many cooperating enzymes, coenzymes, donors, acceptors, and other essential directors of the chemical sequences. For example, the fermentation of glucose by yeast does not yield ethyl alcohol and carbon dioxide directly but proceeds through a series of intermediate reactions each of which is catalyzed by a specific enzyme or enzyme system.

Within the past half-century geneticists have demonstrated that genes are self-reproducing entities, or units, which are the main (some believe the only) carriers of heredity. It is generally agreed that genes act by directing chemical changes as catalysts or by furnishing, through catalysis, ''carriers'' or prosthetic groups from which other biocatalysts may be formed. Genes are autocatalytic catalysts; that is, they catalyze the formation of precise duplicates of themselves and are therefore to be considered as living units. Although genes are much larger than atoms and the simpler chemical molecules, they are submicroscopic and well within the range of colloidal dimensions, as are also the ''macromolecules'' of proteins, starch, and cellulose. Some of the self-duplicating bacteriophages and viruses may also be regarded as living units; for while some of these may be very tiny organisms, others behave like large molecules and may even represent the simplest conceivable form of life, termed by Alexander and Bridges a ''moleculobiont''; that is, a catalyst molecule capable of autocatalysis.

Living units possess another important power, the ability to undergo chemical or structural changes that alter their catalytic output, without, however, preventing self-duplication in their newly acquired form. If the changed unit is incapable of self-duplication, it is not living. Geneticists have given ample proof that while genes generally breed true, an outstanding characteristic of living units is that they can undergo heritable changes.

Comparatively recently (1927) H. J. Muller found that heritable changes can be produced in the fruit fly Drosophila by exposing its eggs or sperms to selected dosages of X-rays; and similar effects were soon created in many other animals and plants. The gene and chromosomal changes that can be produced are unpredictable, and most of them result in sterility or death. However, as early as 1928 Goodspeed and Olson had developed with X-rays about 120 new varieties of tobacco, all breeding true. While the chemical production of gene changes is undemonstrated, chromosomal mutants have been produced; e.g., by colchicine (an alkaloid from saffron). This leads to diploidy, i.e., cells with doubled chromosome number, and consequent changes in the organism. Many of our well-known plants are polyploids; e.g., certain roses have 56 chromosomes, eight times the normal number in the basic type of rose, but have lost their ability to produce viable seed and are propagated by cuttings. In the Neolithic period in Europe the Einkorn wheats had only 7 chromosomes. Over 7,000 years ago wheats of the Emmer group, with larger grains and 28 chromosomes, were grown in Egypt. The Vulgare group of wheats, to which our modern wheats belong, appeared in the Graeco-Roman period, with 48 chromosomes. No matter how the basic biocatalysts are modified or increased, visible and practical consequences generally follow.

How shall we explain the astonishing but very common fact that from a small and relatively simple fertilized ovum there develop in regular sequence highly specialized and differentiated cells, tissues, and organs? As Ross A. Harrison pointed out, this wonder is thus expressed in the 139th Psalm, (16): "Thine eyes did see my substance, yet being unperfect; and in thy book all my members were written, which in continuance were fashioned, when as yet there was none of them."

Embryologists have shown that certain parts of the developing embryo act as "organizers" by exerting "a morphogenetic stimulus upon another part or parts, bringing about their determination and the following histological and morphological differentiation." The "evocator" is described by Joseph Needham as "the chemical substance, acting as the whole or part of a morphogenetic stimulus emitted by an organizer."

The simple principle of catalyst formation or modification readily accounts for the regularity of the development of the embryo, provided we assume that in the original egg or in its milieu and food there are found the essential specific atoms and molecules needed to form or to modify its biocatalysts. Amounts of specific substances unbelievably small in weight will furnish large molecular numbers. Thus one part of biotin by weight in 400 billion influences the growth of yeast, and certain molds will not grow in the absence of traces of gallium. It is, then, understandable how the cytoplasm of an egg may contain atoms and molecules ample in number and variety to initiate the formation and modification of biocatalysts; and physical or chemical conditions in the developing embryo may lead to the fixation (adsorption) or elution (desorption) of these specific substances at appropriate places and times. Great changes in chemical output, reflected in morphology and physiology, may follow tiny catalyst changes initiated by traces of specific substances.

In the cooperative book *Colloid Chemistry* (Vol. 5, p. 593, 1944) I wrote:

> Catalyst modification, which may in turn lead to the formation of new carriers or new prosthetic groups, offers a simple but potent mechanism whereby nongenic as well as genic changes may be heritably transmitted. This does not mean that any and all nongenic changes are necessarily heritable; but the un-

equivocal demonstration of a single case of nongenic catalyst change would compel us to regard the principle as demonstrated, or, to use a legal analogy, as permissive, though not mandatory, in function.

Recently S. Spielman, C. C. Lindegren, and S. Lindegren (*Proc. Nat. Acad. Sci.*, **31**, 95, 1945) have reported that in the presence of melibiose certain yeasts develop an enzyme capable of splitting this sugar and that the production of this enzyme can be maintained even in the absence of the gene responsible for its initial synthesis, provided that melibiose continues to be present. Apart from the well-established genic inheritance, we have here evidence of cytoplasmic inheritance and therefore demonstration of a mechanism whereby *some* acquired characteristics *might* be transmitted. This mitigated physicochemical aspect of Lamarckism is far removed from the view that led to James Russell Lowell's amusing lines in *The Bigelow Papers* (*First Series*, No. 4, lines 31 *et seq.*):

Some flossifers think thet a fakkilty's granted
The minnit it's proved to be thoroughly wanted . . .
Ez, fer instance, thet rubber-trees fust begun bearin'
Wen p'litikle conshunces came into wearin'—
Thet the fears of a monkey, whose holt chanced to fail,
Drawed the vertibry out to a prehensile tail.

If a heritable catalyst change, genic or nongenic, leads to the formation of substances or structures that give the possessor some advantage in the struggle for food or mates, we must envisage the probability that such a new form will dominate its competitors and, as the fittest, will tend to survive. However, it is equally possible that heritable catalyst changes may lead to very disadvantageous results and thus handicap the descendants.

Excess, deficiency, or imbalance of essential trace substances (hormones, vitamins, minerals, etc.) may lead to such abnormalities as pellagra, giantism (acromegaly), cretinism (myxoedema), etc. The various clinical symptoms (fever, inflammation, swelling, eruptions, etc.) which constitute syndromes of diseases may generally be traced to interference with the normal biocatalysts or with the permeability of tissues or septa controlling the diffusion of raw materials and final products to and from the biocatalysts. Invading organisms such as bacteria or fungi may form endo- and exotoxins which produce effects remote from their places of origin. Though the fungus *Tinea*, responsible for "athlete's foot," commonly grows between the toes, an eczema of the hands is a frequent symptom.

In the early days of medicine diseases were classified by their visible results, and the Greek or Latin professional terms applied to them often added nothing to their popular names. Progress in the understanding, treatment, and cure of many diseases followed investigations into increasingly lower levels of structure: first, by gross dissection; then with the microscope, and, finally, by chemical methods to the invisible but basic chemical level, where the importance of catalysis has become evident, as we may see by considering that group of abnormal conditions called "cancer."

The term "tumor," of clinical origin, originally included swellings of all kinds. It is now generally restricted to neoplasms (new growths) arising from a progressive duplication of cells. If the neoplasm remains localized, without invading surrounding tissues, it is said to be "benign"; if removed by operation, it does not recur. But if the neoplasm invades healthy tissue, it is said to be "malignant"; even if removed by operation, it tends to recur and often sets up secondary growths, or metastases, initiated by cells from the initial growth, which, from the crablike appearance of the invasive tendrils, is called cancer.

About 1914 the cytologist Theodor Boveri published a paper (English translation, 1929, Williams & Wilkins Co., Baltimore, Md., entitled *The Origin of Malignant Tumors*) in which he attributed cancers to a "certain abnormal chromatin-complex, no matter how it arises. Every process which brings about this chromatin condition," he asserted, "would lead to a malignant tumor. . . . Typically, every tumor arises from a single cell," whose definite and wrongly combined chromatin-complex leads to a tendency to rapid cell proliferation, which, in addition to all other abnormal qualities of the tumor, is passed on to all descendants of the primordial cell arising by regular mitotic cell division.

The work of H. J. Muller indicated that treatment of Drosophila eggs and sperms with X-rays can increase by about 1,500 times the expected rate of mutation, perhaps in part due to natural radiations. This gave support to the view that cancer will follow any mutation of a somatic cell which would have as its consequences progressive duplication and invasion of surrounding tissue. Attention was thus focused on the molecular or near-molecular genes and other biocatalysts as the units some of whose alterations, mutations, or modifications could lead to the clinical symptoms of cancer. Furthermore, there is ample evidence that apart from cancer gene and catalyst changes may also produce a great variety of other results in organisms, some normal, some abnormal. So there seems no escape from the view that the genes and other biocatalysts, despite their tiny masses, inexorably dominate the chemical changes in organisms which sometimes lead to results we recognize as disease, and in certain clinical cases to cancer.

A solution of the cancer problem will be at hand when, without causing serious damage to the organism, we can inactivate the cancer-causing biocatalysts or modify them or their products so that the syndrome we call cancer will not result. The proximate "cause" of cancer lies at the biochemical level; but successful efforts to arrest the disease may come from quite different levels; e.g., X-ray radiation, neutron radiation, or surgery if done early enough. Recently (*Science,* 1943, **97,** 541) Charles B. Huggins, of the University of Chicago, reported:

> It is possible by reducing the amount of the activity of circulating androgens to control, more or less but often extensively, far advanced prostatic cancer in large numbers of patients. In this special case, androgen control seriously disturbs the enzyme mosaic of the cancer cells, at least with respect to the important energy producing protein-catalysts, the phosphatases. As a contribution to the problem of cancer treatment, it is well to emphasize that any interference with an important enzyme system of a cell, normal or malignant, will cause that cell to decrease in size and function.

Though cancers are known to follow a diversity of conditions; e.g., mechanical and thermal injury, radiation, virus inoculation, introduction of chemicals like 3,4-benzpyrene and methyl cholanthrene, it is the clincally visible consequences of heritably changed catalysts that lead to the diagnosis "cancer."

INSECT CONTROL FOR THE MARINES

By Lt. (jg) JOHN M. HUTZEL
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It was late afternoon, and the Marines were digging in to consolidate their positions before nightfall. To the south Mt. Suribachi poked its squat, ugly peak toward an overcast sky. From its base, and halfway across the isthmus of volcanic ash leading to the northern creviced end of Iwo Jima, heavy artillery sent projectiles hissing overhead to explode seconds later about Motoyama Airfield No. 2. Just off the eastern shore, and north of the convoy of troopships and churning Higgins boats racing supplies to the crowded beachheads, a heavy cruiser surged slowly forward pouring explosives into ravines at point-blank range. Battleships, farther out, maintained an intermittent fire, directed by observation planes above. Upon the captured airstrip, to the left, stood several small Marine monoplanes. Enemy shells dropped in scattered bursts across its runways. The landscape ahead was spotted with puffs of smoke interspersed with the glow of flame throwers and echoing with the rattle of machine guns. Suddenly the artillery fire slackened, and from the east two planes zoomed in low over the beach, a trail of black "smoke" pouring out behind. Thousands of faces turned upward in curiosity. As the planes passed overhead a wave of unprintable language swept the onlookers; not the monosyllabic epithets of adolescents, but the blasphemous, soul-searing well-rounded phrases of Marines in battle. In subsequent flights there was a general scramble for cover, for the coal-black DDT spray speckled the unwary, like measles. Thus did Iwo Jima become the first objective sprayed by carrier-based aircraft for the control of insects.

Airplane spraying was but one development in the Navy's comprehensive program for the control of malaria and other insect-borne diseases. To meet the demand for trained malaria control personnel early in the war, Navy doctors, hospital corpsmen, and commissioned specialists in entomology, bacteriology, parasitology, and related fields were enrolled in special schools. Here they were given thorough review and training in methods of preventing, diagnosing, and treating malaria and other diseases of epidemic portent. Upon completion of instruction the men were organized in malaria control units and epidemiology units. The primary functions of the former were to control insect carriers of disease and make malaria parasite surveys, whereas the latter provided laboratory facilities to track down disease sources and make recommendations for the prevention of disease. Each unit usually consisted of one or two officers and four or five enlisted technicians, and they were deployed among training, garrison, and combat forces, according to need. Marine Divisions were assigned both types of units which operated under one command as a "malaria and epidemic control unit." I shall call it simply a "control unit."

As the war progressed the functions of control units were supplemented by medical research units and rodent control, special sanitation, and construction battalion components. The latter were equipped with draglines, bulldozers, and other heavy machinery for mosquito control and general sanitation projects. Combat forces, such as Marine Divisions, required an extensive program of sanitation to initiate protective measures concurrent with the sudden movement of

large numbers of troops into disease-ridden enemy territory. For this purpose battalion sanitation squads were organized and trained by the attached control unit. Throughout the war the control unit bore the immediate responsibility for the control of mosquitoes and other insects transmitting disease, and its members performed the gamut of duties from poisoning rats to the disposal of enemy dead.

Protecting the health of military personnel in the United States was just as important, though less dramatic, than in forward areas. As the front was approached, however, the problems confronting control units became increasingly difficult, culminating in the protection of troops during combat. Prior to an assault operation of the type undertaken by Marine Divisions the men had to be "educated" to take suppressive drugs, to avoid areas where disease was most likely to be contracted, to eliminate conditions leading to disease transmission, and to use in action the protective chemicals and equipment furnished them. Front line troops were not expected to be overly conscious of mosquito control while subjected to enemy fire, but rear area personnel were required to observe all disease control precautions. Otherwise, an epidemic might have seriously reduced troop reserves and harassed hospital staffs already overburdened with combat casualties. Through lectures, demonstrations, and the use of training films the life histories of insects were outlined, and their part in disease transmission and measures necessary for their control were explained. It was pointed out that atabrine, in most instances, served only to suppress malaria, and when its use was discontinued personnel that had been bitten by infected mosquitoes were likely to develop clinical manifestations. Consequently, the control of insects and protection from their bites was of foremost importance.

In addition to these duties the control unit supervised the impregnation of clothing with repellents and the residual spraying of tents and prefabricated units with DDT. It recommended the type and distribution of insecticides and other sanitation supplies, and loaded special material, including laboratory equipment, for its own use in the field. While the Marine Division was in the staging area, the control unit also carried out all usual functions assigned to it; e.g., sanitation inspections, mosquito and malaria parasite surveys, and control of mosquitoes, flies, lice, roaches, etc.

During invasion and early occupational stages every effort was directed toward immediate reduction of existing insect populations and termination of further propagation. This was most rapidly accomplished by the use of insecticides; the more permanent control programs, such as draining and filling for mosquito control, were gradually developed at a later date. The effectiveness of insecticides, however, depended not only upon toxicity but also upon the method of application and type of dispersing apparatus. To effect immediate control extensive applications had to be made within a few hours. Early control units were equipped only with knapsack, decontamination, and hand sprayers for chemical control procedures. The fact that they accomplished so much with so little was indeed a tribute to their skill and resourcefulness.

The pyrethrum-freon aerosol "bomb" was one of the first outstanding contributions to the technique of insect control used to advantage by Navy personnel during the war. This method of applying an insecticide increased its effectiveness and provided a convenient means of controlling mosquitoes in and around buildings. The aerosol bomb, however, had limitations arising from transitory effectiveness and limited extent of area that could be adequately treated. It was

not until the advent of DDT that modern means of application were fully realized.

The insecticide DDT is a stable chemical that retains its lethal effect against insects long after it has been applied to surfaces on which they alight. Further, it is extremely toxic to adult mosquitoes and flies in minute quantities. These properties made possible its successful adaptation to a variety of means of application of particular value to military forces. The techniques of airplane spraying of chemicals and laying smoke screens were modified to accommodate DDT. Ground operated smoke generators were converted to produce fogs composed of fine droplets of DDT solution that drifted downwind like morning mists across the countryside. The residual toxicity enabled small labor parties working only with knapsack sprayers and paint brushes to set huge booby traps for insects by treating walls of tents and barracks with solutions of DDT. Experience proved no one of these techniques of applying DDT a panacea for all insect control problems, but when used in combination and as supplemental measures the batting average of the control units was increased enormously.

Special emphasis was placed on the development of airplane application of DDT, for this method promised to provide large-scale insect control simultaneously with assault activities. The main advantages of plane spraying stemmed from the speed of application and extent of area that could be treated. Large labor parties could scarcely hope to cover in weeks the variety of insect breeding and resting places effectively sprayed by plane in a few minutes. Moreover, many places susceptible to aerial treatment were inaccessible to ground crews.

Land-based Navy planes were first used to spray DDT during combat at Peleliu in September 1944. It was but one step farther to use carrier-based aircraft in an attempt to close the gap between landing of assault waves and initiation of effective insect control measures.

Prior to the invasion of Iwo Jima a joint session of Army, Navy, and Marine personnel formulated a plan for repeated air spraying of the island. Two torpedo bombers based on one of the accompanying escort carriers were to stand by for spraying when directed by request of malaria control personnel operating with the ground forces. As soon as land-based facilities had become available and the carriers had withdrawn, especially equipped Army planes from Saipan were to continue the spraying program so that no interruption of insect control would occur between the assault and garrison stages. The control unit of one of the Marine Divisions assigned to the operation was charged with the responsibility of over-all sanitation of the island and direction of plane spraying until relieved by the Island Surgeon of the Army garrison forces. This team had already developed two sets of spray apparatus for use in light aircraft of the attached Marine observation squadron and were instructed to take them along in case plans for spraying with other planes failed to materialize.

Spray apparatus and an insecticide concentrate for use in the torpedo bombers were loaded aboard an escort carrier at Pearl Harbor; the concentrate to be diluted with fuel oil carried by the ship. A Navy flight surgeon with previous experience in plane spraying at Peleliu was assigned to brief pilots during the operation and direct installation of the apparatus. Two weeks before D-day a member of the control unit flew to Saipan to coordinate last minute details affecting the transition between Navy and Army spraying.

The assault on Iwo Jima began on D-day, February 19, 1945. For the first few days intense enemy opposition restricted shore activities. Aboard ship,

members of the control unit assisted the overburdened medical staff in caring for the wounded. It was not until D plus 5 that they were disembarked and sent ashore. An immediate survey of the beachhead area indicated that mosquitoes were no problem, but that houseflies were abundant and threatened to become dangerously prevalent. Plane spraying from carriers was impractical prior to D plus 9 because intense artillery fire prohibited low-level flights. An attempt to use a Marine observation plane having direct liaison with the artillery before the torpedo bombers arrived was aborted by enemy shell fire. The plane was knocked out during the installation of the spray apparatus.

On D plus 9 the two torpedo bombers made the first application of DDT. Before they arrived arrangements were made to reduce friendly artillery fire for the spraying interval. During this and subsequent missions treatment was restricted, insofar as possible, to the occupied area, but irregularly drawn battle lines resulted in some spraying of enemy-held territory. Later interrogation of prisoners revealed that the Japanese first thought the cloud of spray was gas. Presumably their observation that most of the spraying was done over American lines allayed this fear.

Surveys conducted the day following the first application showed that the existing adult housefly population had been reduced almost 100 percent. However, breeding still persisted in places inaccessible to the spray, such as in pail latrines and covered refuse, and repeated treatments in conjunction with ground activities were necessary to keep the fly population at a low level. A daily survey indicated the areas most in need of aerial treatment. These were plotted on target maps, and directions were radioed to the responsible air group to spray a given target area. When land-based facilities became available for aircraft and the carriers had withdrawn, Army Douglas Transports from Saipan continued the spraying. They were directed by the control unit until the island was officially declared secured and the garrison commander assumed charge. The combined activities of Navy-Army spraying were similarly employed on Okinawa. Here the tactical situation permitted spraying on D plus 1, and 22 square miles of beachhead had been treated by carrier aircraft before land-based planes took over the spraying.

In spite of its usefulness, aerial application had the limitations of other space-spraying methods; that is, lack of uniform diffusion of the toxicant and short duration of effectiveness. Military personnel continued to depend heavily upon basic ground procedures to carry out the insect control program. The introduction of DDT midway in the war changed the perspective of matériel needs. New methods of application were visualized requiring new types of equipment. However, standardization of equipment for volume production was a time-consuming process. After an experimental model had been successfully demonstrated to Naval authorities, tooling and other manufacturing problems delayed delivery. When finally furnished in quantity to supply depots, the new equipment was held there for months owing to shipping difficulties. Thus at the end of the war control units still relied to a great extent upon hand-powered appliances for the distribution of insecticides. Fortunately, the free interchange of practical ideas and experimental data among authorized civilian agencies, military research activities, and field control units did much to aid the last in designing and building their own insecticide dispersing apparatus to meet emergency needs. The results achieved were little short of phenomenal; a triumph of which all hands, civilian and military, could be justly proud.

THE ARMY'S WAR AGAINST MALARIA

By Major THOMAS A. HART
SANITARY CORPS, ARMY OF THE UNITED STATES

Malaria has always been a scourge of the world, but not until the recent global war have its devastations been brought forcefully to the attention of the people of this country. The year 1942 was a critical one for the Army with respect to tropical diseases. In the Southwest Pacific area malaria was an acute problem. It is conceivable that mosquitoes alone could have acomplished what the Jap banzai charges failed to achieve. Casualties from malaria in the Pacific four years ago were eight times those from Japanese action. It became urgently necessary to set up an organization of trained personnel to study and combat the situation in the field.

By December 1942 a malaria control organization was established in the Sanitary Corps of the Medical Department. This organization consisted of malariologists, malaria survey units, and malaria control units. The malariologists, one for each theater of operations in which malaria was a problem, were malaria control specialists and liaison officers between higher headquarters and the malaria survey units and malaria control units in the field. Malaria control units consisted of small mobile teams commanded by a Sanitary Corps officer trained in sanitary engineering and malaria control. Malaria survey units were likewise small teams operating from mobile field laboratories. The malaria survey units were supervised by two Sanitary Corps officers; one an entomologist with special training in collecting mosquitoes and identifying them, the other a parasitologist trained in laboratory and field procedures for estimating the status of malaria in a population through blood examination, spleen rates, and the interpretation of epidemiological data. The survey units were charged with the responsibility of searching out and finding malaria-carrying mosquitoes and the type of malaria present in the native population. These findings were made available to the malaria control units and enabled them to set up and carry out efficiently the control measures indicated for an endemic area. By February 1943 this organization was functioning in the field overseas.

The personnel of the field units carried on the battle against the deadly mosquitoes under difficult and trying conditions. They had to adjust themselves to the dank humidity and heat of tropical jungles, swamps, and kunai grass plots. In certain locations wild boars and crocodiles were menacing to them and interfered with their work. Wading hip-deep across streams and walking knee-deep in muck was the common lot of these men. Sometimes they had the eerie experience of losing their way in the jungle or in a tropical mangrove swamp. The matted roots of trees, the trackless jumble of plant life, the half-light filtering through the dense foliage, and the palpable stillness were frightening. The humidity and the insects added to the discomfort and apprehension of the wanderer.

The cooperation of the New Guinea fuzzy-wuzzy and other native laborers in this dangerous work was one of the pleasant experiences the men remember. These people were eager to learn and were willing workers; thousands of them aided in the important work of malaria control. Their usually jolly and carefree manner, their singing on the way to work, and their love of any mechanical equipment, particularly trucks and jeeps,

endeared them to the GI and helped him to banish monotony.

"Man-made" malaria follows the footsteps of an Army in the tropics. Slit trenches, dugouts, abandoned gun emplacements, shell craters, wheel ruts and borrow pits—all water-holding depressions—increase the number of places in which mosquitoes can breed. The Army in the field literally tore up the earth. To combat the tiny mosquito the malaria control units used giant bulldozers and draglines. For grading and filling low, water-holding depressions the bulldozer proved to be a perfect tool, and the draglines were used for ditching to drain off accumulations of water. Whole swamps were eliminated with this engineering equipment. The greatest technical aid developed for the use of antimalaria units was DDT, the powerful chemical insecticide. The discovery of the insect-killing properties of DDT was one of the outstanding wartime contributions to the entire field of preventive medicine. Because it was effective in relatively small quantities, the problem of shipment and transportation of insecticides was simplified. The malaria control units and malaria survey units, aided by this mechanical and chemical equipment, including atabrine, reduced the incidence of malaria to a very small percentage of what it was in 1942. Their heroic and persistent labors helped keep our Army in excellent condition.

These acomplishments resulted from much tedious, hot, sticky work. Research in the jungle, done under the most primitive and difficult circumstances, added to our knowledge of how to combat malaria as well as many other tropical diseases. Radio talks on the "Jungle Network" helped to sell malaria control to the GI. Roadside billboards with luscious pin-up damsels were made, painted, and erected by the antimalaria units to attract attention to the malaria warning carried by the sign. These signs and posters and lectures and radio appeals were all used to educate every man to the dangers of malaria and the best methods he could use to protect himself. Actual control work in the field was a "back-breaking" job. Knapsack containers strapped on one's back were used to spray oil containing DDT on every body of water down to the circumference of a watch crystal. Slow-moving streams choked with vegetation were cleaned out, sometimes by very primitive means. Men, materials, and vehicles had to get to the job through mud and torrential rains. Training new gangs of native labor each week, sometimes at daily intervals, was a drain on patience. To these tasks were added the everyday problem of housing, feeding, and protection of personnel from disease and from Japanese action. Although these malaria units were not combat troops, in the shooting sense, they were waging war nevertheless—a war against disease. They carried malaria control to the front lines and they were there on D-Day with the beachhead landing parties. Their lot was at times unenviable, but usually they were so interested in the job they were doing that no moment was dull.

The fight against malaria has been successful. Through technical skill, educational methods in which all publicity media were used, and intense application to the problems of malaria control and their solution almost unbelievable feats were accomplished against terrific odds. To the malaria control organization, its malariologists, and the personnel of the malaria survey units and malaria control units goes the lion's share of the credit for reducing the malaria rates of the American Army to the lowest level ever recorded for an army in the field in endemic malarious areas.

THE STRANGE CASE OF BLAISE PASCAL

By RUFUS SUTER

THE FRENCH civilization of the seventeenth century was a bright and diversified tapestry. Not only was there talent, but the men and women were remarkable for the complexity of their characters. There was, for example, Antoine Gombault, chevalier de Méré, a gambler, and the arbiter of good taste in Paris. He was outrageously conceited about his mathematical prowess, wrote books on etiquette and the art of sprightly conversation, and strove to be a paragon of elegant manners for all future generations. There was Pierre de Fermat, an honest and respected lawyer of Toulouse and the patron saint of amateur scientists. Despite his legal duties he found time to make contributions to mathematics that have put his name on a par with Newton's. To de Fermat we owe the foundations of the theory of numbers, and to him, as well as to Descartes, we are indebted for analytic geometry. Then there was Antoine Arnauld the younger, a theologian, who was a leader of the religious movement known as Jansenism after its founder, Cornelius Jansen, a bitter critic of the Jesuits at the Louvain. Jansenism, now extinct, was the Roman Catholic version of New England Puritanism, and came the closest to the religion of the evangelical Protestants of any movement within the Catholic Church.

These three men—a gambler, a mathematician, and a theologian—have been mentioned so as to emphasize the versatility of their friend, Blaise Pascal (1623–1662), a precocious youth from Clermont, who came to Paris after his father's death to have his fling at the world. He established himself with the ducal family of Roannais, which traced its pedigree back to Gaulo-Roman days. He fell in love with the Duke's sister Charlotte, the sole heir of her brother's estates, who afterwards married a nobleman of her own rank. After this unfortunate event, the chevalier de Méré hoped to distract Pascal's mind by making a genteel sport of him. No procedure could have been more shrewd, in dealing with a mathematical prodigy, than to suggest the opportunities for mathematical research at the gaming table. The result of the chevalier's good-natured suggestion, however, was only a series of learned letters between Pascal and the respectable de Fermat. But it was a correspondence that has become a classic in the history of mathematics. It contains the foundations of the theory of probability and of combinatorial analysis.

For Pascal the transition from the gay de Méré to the sombre Arnauld was a return to the memory of the authority of his father, who had been deeply impressed by the teachings of the Jansenists. His sister Jacqueline had even become a nun at the Jansenist convent at Port Royal in the outskirts of Paris. The young Pascal, his sense of the uncertainty of human existence quickened by a nearly fatal horse-and-buggy accident, suddenly experienced religious conversion, with all the mystical imagery described by medieval saints. The rest of his short life was spent, one might say, as a "lay" monk, thinking and writing along Jansenist lines. His reverence for Arnauld led him to compose the *Provincial Letters,* a critique of the Jesuits, who had charged the older man with heresy. Although the *Letters* failed in their immediate purpose of ex-

onerating his spiritual mentor, they became the model for the satirical essay in French. Translated into almost every other European language, they were best-sellers for two centuries, and were one of the several causes that led to the suppression of the Jesuit order by Pope Clement XIV in 1773. Pascal's *Pensées,* a set of philosophico-religious meditations, are the only legacy of Jansenism that continue to inspire the religious imagination.

The paradox of Pascal is particularly striking in an age of paradoxical thinkers. As a physicist and mathematician he is the peer of Galileo, Torricelli, Boyle, and Descartes. As a Christian mystic he belongs, in spite of the denominational difference, to the company of our own Jonathan Edwards, the Puritan divine from Connecticut who mixed hairsplitting logic with visions of the spiritual world. Finally, as a skeptic he is almost as disillusioned as David Hume. One would suppose that the mutual incompatibilities among three such contrary points of view would have made it impossible for a single integrated mind to hold them all.

One of the features that makes Pascal an alluring subject for study is the personal incident that was often the occasion for his most impersonal investigations. Thus, when in 1646 his physicist friend, Pierre Petit, came through Rouen, and told Pascal and his father of the experiments in creating a vacuum in glass tubes by an Italian pupil of Galileo, named Evangelista Torricelli, Petit and the two Pascals decided to try the experiment for themselves. At that time the possibility of a vacuum was denied not only by the respectable conservatives, the Aristotelians, who were entrenched in most of the pulpits and professorial chairs of Europe, but also by the right wing of the radical scientific party, the followers of Descartes. It was held that nature "abhorred" a vacuum, and that nature would as soon vanish as to permit the least bit of a vacuum. The experiment Petit and the Pascals repeated after Torricelli was as devastating as that by which Galileo, in the tower of Pisa, refuted the Aristotelian notion that heavy bodies fall faster than light ones. The Torricellian experiment, impressive in its simplicity, consisted in upsetting a glass tube packed with quicksilver into a bowl of the same substance, without letting any air seep into the tube in the meantime. The mercury falls away from the top of the tube, but never reaches a common level with the quicksilver in the bowl into which it is poured. Instead, the encased column stands jutting above the free surface in the basin—an unexpected sight, since there seems to be an unaccountable lack of equilibrium. However this may be, the point that interests us at present is that the space in the sealed top of the tube, after the mercury has dropped, is left empty. Here possibly is a vacuum, though nature has not annihilated herself in its generation. But to return to the odd halt in the drop of the mercury: we see here a second point. Nature apparently has a limited or restrained "horror" of the void, for the column does not fall as low as it could, as if there were a tension holding back the expansion of the vacuum. Indeed, the force by which nature exercises her "horror" is constant and measurable.

Pascal performed this experiment several times before audiences, using tubes of various sizes and shapes, some of them 46 feet long. Descriptions and results were published in his *Expériences Nouvelles Touchant le Vuide.* It is interesting to note that his modest and cautious temper already revealed itself here in his first published essay on physics. Rather than assert confidently that he has proved the possibility of a vacuum,

he is satisfied to state that in his opinion the apparent void is real, and that he will continue to believe so until somebody shows the contrary.

The significant part of Pascal's experiment, however, is not the object for which he performed it. Whether a true vacuum can exist or not is a metaphysical question depending upon the definition of emptiness, and it has little if any relevance to experimental physics. But the second point—the point noted in passing—that the column of mercury remains suspended above the free level of the mercury in the basin, has bearing upon the science of the statics of fluids. Pascal was too modern to imagine that this suspension was literally the effect of a limited "horror" of nature for the void. But then what force is it that prevents the quicksilver from pouring out altogether into the open bowl?

Before the appearance of the *Expériences Nouvelles* Torricelli had made the bold suggestion that this force was the weight of the earth's atmosphere pressing down upon the free surface of the mercury in the basin, and pushing some of it up into the tube. In other words, this experiment exhibited a simple case of the equilibrium of fluids. A column of air (one fluid) counterbalances a column of mercury (another fluid). Pascal knew of this explanation; but Galileo's school, in those days, represented the extreme left wing of the radical scientific party, with hardly a reputable representative in Europe, and the cautious Pascal was wary of allying himself with such revolutionists until all the evidence was in.

So he asked himself this question: Suppose that the earth's atmosphere really does push some of the mercury up into the tube; then, if this bowl, along with the tube, is insulated against atmospheric pressure, should not the mercury in the tube settle to the level of the mercury in the bowl? To test this implication of Torricelli's idea, Pascal and his brother-in-law, Perier (the husband of his sister Gilberte), performed the experiment of the "void within a void"—the whole Torricellian experiment carried out inside of the empty upper end of a larger Torricellian tube. They saw the column actually fall. But Pascal was unconvinced.

He asked himself another question (or, rather, if we accept Descartes's testimony, Descartes posed this question): Suppose that the air weighing down upon the open surface in the basin really does press the quicksilver up into the tube; then, when the apparatus is carried to a higher altitude, should not the mercury be pushed up a shorter distance, since the mass of air above the tube is less? To test this consequence of Torricelli's suggestion, Pascal instructed his brother-in-law to try the experiment on Puy-de-Dôme, a mountain near Clermont, Pascal's birthplace, where Perier lived. The investigation was conducted with all the solemnity of a religious ritual. At 8:00 A.M., September 19, 1648, in the garden of a local monastery, before a small group of honest townspeople, the Torricellian experiment was performed three times with two tubes 4 feet long. The column of quicksilver stood at the height of 26 inches, 3.5 lines. (A "line" equals 1/12 of the French inch of the time, which latter equals 1.065 of today's English inch.) Then, while one set of the apparatus was left in place under the care of a monk chosen for his integrity, the party carried the other set to the top of the Puy-de-Dôme, about 4,806 feet above Clermont. There, in five repetitions the mercury stood at 23 inches, 2 lines, despite rain or shine, open air or shelter. To clinch the argument, there was a retest between the summit and the foot of the mountain and again near the foot. In this last place the mercury stood at 25 inches. Back at the monastery the height was

found to have remained where it was before the party left. Finally, the investigation was repeated in the garden, with parts of the apparatus switched. There was no gainsaying the corroboration of Torricelli's suggestion.

Pascal published a description of this experiment in his *Récit de la Grande Experience de l'Équilibre des Liqueurs,* the last of his essays on physics to appear during his lifetime.

As for the knowledge these studies brought to light: they suggested that the old assumption about nature "abhorring" a vacuum was unnecessary; they added new evidence to the idea that air has weight; they gave us an instrument (the barometer) of use in determining altitudes, and of limited value in forecasting weather; and they hinted that air pressure when applied to partial vacuums can be put to work—can, for instance, lift a column of mercury. Pascal never dreamed how widely this last suggestion would affect our understanding and our mastery of nature. We know today, for example, that an airplane flies because of the availability of air pressure for work in a partial vacuum. We speak of "differential atmospheric pressures" rather than of "partial" or "relative vacuums," but this is a new name for an old face. The propeller, driving out the air in front of the cockpit, creates a relative vacuum into which the weight of the atmosphere pushes the airplane, thus forcing it along its course. Again, the rush of air round the wing driving the air away from the top surface, when the wing is curved in a certain way, causes a relative vacuum to be formed above the wing so that the weight of the atmosphere pushes the plane up into the area of lower resistance, causing the machine to rise.

Pascal's contributions to the statics of fluids were as remarkable on the theoretical as on the experimental side. Two posthumous treatises, *Traité de l'Équilibre des Liqueurs* and *Traité de la Pesanteur de la Masse de l'Air,* present an elaborate system of the statics of fluids. The former treatise belongs in the classical tradition of hydrostatics coming down from Archimedes in that it makes use of a tightly woven deductive method of exposition analogous to the geometry of Euclid. The latter presents the phenomena of atmospheric pressure as a special case under the statics of fluids in general. Pascal's originality as a physicist lies in this masterpiece of systematization rather than in any specific experiment or invention, in all of which he was anticipated by others.

One should also note that Pascal's experiments were perfectly planned arguments to clinch the points he tried to prove. They are as flawless as the ideal examples of scientific method to be found in textbooks of logic today.

As an experimentalist Pascal was one of the founders of modern physics. As a mathematician he invented, with Gérard Désargues, projective geometry. With Fermat he created the theory of probability and combinatorial analysis, probably the most fertile of his achievements, although neither he nor Fermat could possibly have foreseen what wide application the principles of this science were to have in later centuries. The attempt to bring chance under law has developed into a powerful new weapon indispensable to life insurance statistics, population studies, intelligence tests, subatomic physics, extragallactic astronomy, etc. In the field of applied mathematics, Pascal invented and constructed the first modern adding machine.

We may therefore be astonished that a man who was more successful than most of us in the pursuit of truth should have been dubious about the power to

know. If we consider, however, we will recall that even the ablest scientists have not been outstanding for self-confidence. Newton's comparison of himself to a child playing on the shore of the sea of truth and picking up here and there a bright pebble is not typical, but it represents a frame of mind perhaps more commonly felt than expressed. The skeptical mood, after all, is a phase of the virtue of humility.

Pascal's skepticism has its origin in a sense of the physical smallness of the human being. Some of the early scientific thinkers, like Giordano Bruno, were awakened to a joyous ecstasy by their discovery of the infinitude of the starry sky. But Pascal was depressed. Possibly he could see in the physical puniness of the race some reflection of the moral unworthiness of man. At any rate, he set a richly suggestive example of gloom for the many astronomical writers after him who have painted the familiar picture of the human insect on his tiny ball dropping forever through the chill vastness of eternal night.

It is a curious anomaly that the universe that depressed Pascal—if we may judge by some random passages in the *Pensées*—was Ptolemaic rather than Copernican. He seems satisfied, that is, to speak of the earth and not the sun as the center of the planetary and stellar system. One would have expected him to speak for the heliocentric theory since he showed independence of tradition in his physics. But one should be wary of supposing that this conservatism was dictated by fear of ecclesiastical censure, as was the same reticence on the part of Descartes. Pascal, always cautious about jumping at conclusions, probably felt it premature to declare in favor of the Copernican astronomy. His attitude is clear in the *Provincial Letters* where he chides the Jesuits for obtaining a papal decree against Galileo, the great champion of Copernicus. His point is not that either Galileo or the Pope was incorrect; rather, he wished to indicate that whether the earth moves is not to be settled by papal bulls. In the *Pensées,* moreover, Pascal's aim was to emphasize the physical as well as the moral smallness of human beings, and this is as obvious in the setting of an infinite Ptolemaic universe as in a Copernican background.

Pascal was also made despondent by a complementary picture less familiar to us, of man's stupid hugeness in contrast with the subatomic world. He indulged in a speculation about the atoms being universes, each with its firmament, planets, earth, animals, atoms, etc., *ad infinitum.*

So the complete picture of our place in nature is one of a creature lost between two extremes: that of the immeasurably great and that of the immeasurably small, neither of which we can comprehend. A certain degree of skepticism is therefore unavoidable; but this is a skepticism of extremes. Only an inordinately conceited creature would dream of sounding to the nethermost depths from which he is built up, or of soaring to the outermost edge of the wilderness where he vegetates for a few moments. And for all practical purposes, this limitation on our knowledge is not gruelling.

But if we press further, our pride will collapse altogether. This skepticism of ultimates must dog us in all our science. Take geometry: As the subatomic world disappears into a bottomless well, so the foundations of geometry run down in the direction of ever more nearly ultimate axioms without attaining to one genuinely ultimate. Again, as the world of stars spreads outward without end, so the geometrical theorems to be proved are inexhaustible. The geometer's task is unmanageable: first, because he cannot fully demonstrate a single theorem; second, because he cannot solve all of

the problems the geometrical realm poses. So with the statics of fluids; so with physics in general.

We may flatter our vanity a little if we consider that here again we are concerned with a skepticism of extremes. Let us draw a line at a point in the receding array of premises, and say: Here we shall stop. These we will treat as if they were true axioms. Let us draw another line: This is as far as we shall go in our problem-seeking. Thus by a *tour de force* we have fenced in a neat interval where we can have partial, conditioned, relative science. Pascal, like Hume, attributed much of what we call knowledge to habit.

The genius of Pascal's skepticism is that it is enlightened common sense. He is merely a cautious experimentalist and mathematician, his conclusions tempered by Christian fear of intellectual pride, who knew the obstacles and blandishments in the path of complete proof. For science, both absolutely certain knowledge and absolutely certain ignorance are unattainable. One would go far to find a restrained, balanced estimate of the human capacity to know equal to Pascal's.

THE clue to Pascal's skepticism is, of course, his religion. An important part of the Jansenist teaching, as well as of the Puritan, was the idea that man is inherently evil. To the average Jansenist or Puritan Father, moral weakness was bad enough to exhaust the gruesome possibilities of this state of ruination. But for Pascal the old dogma of total depravity had a broader meaning. Man is feeble in wit as well as will. It is as impossible for a scientist to know anything unqualifiedly true as for men in general to will anything good. This condemnation applies only to what the theologians called the "natural" man, that is, man exiled from the source of truth and good. But since "regenerate" men—that is, men who through conversion have been reunited with the source of truth and good—have their interest turned in a different direction from the natural sciences, the condemnation amounts to a general suspicion of the human power to know.

In the end, Pascal's religion swallowed him up. After his conversion he abandoned his scientific inquiries, save for one or two half-frightened instances, and excelled Jonathan Edwards, or any of the Puritans, in his brooding over the sense of sin, and in prayer and Bible-reading. Some of his critics attribute this change to a mental and physical breakdown. For many years, indeed, a day never passed that he did not suffer acute physical pain. After his death, which came during a convulsion, his whole digestive tract was found to have collapsed and his brain to contain a lesion. But one should not discredit the last phases of Pascal's thought solely because he was diseased. His religious preoccupation may have been induced by mental and physical suffering, but this of itself does not constitute sufficient evidence that any of his insights were worthless. We should weigh the possibility that humanity really is corrupt, that the cognitive faculties are slightly out of focus, and that part of Pascal's genius was that he recognized this. As a matter of fact, we should, perhaps, even be willing to consider the idea occasionally that insights of a different order from those achieved in the laboratory and in mathematical reasoning may help to resolve our quandaries.

It is not unintelligibly erratic today for a biochemist, for instance, to turn Marxist, and to substitute the method of thinking associated with the materialistic dialectic for the customary processes of experimental science. In seventeenth century France, a Catholic physicist and mathematician who turned to the Bible was no more of an anomaly.

INTEGRATION IN SCIENCE TEACHING

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WHO WILL deny that "... everything in our experience is only a part of something else that in turn is only a part of still something else"? This concept of interdependency between the things, states, and events of environment is as old as ancient Indian cosmology and has been variously stated down through the years by many thinkers, not the least of whom are Pareto, Smuts, and Cannon. And who would deny that this business of living may be facilitated by increasing knowledge of how these somethings are part of each other?

Clearly, the function of science teaching, as of all teaching, is to guide all students to knowledge of the known relations and to prepare some students to add to that knowledge. The states of mind with which such students approach the unsolved problems and the ways in which they go about getting new knowledge are some index to the efficacy of the preparation.

It would seem that those who deal with the products of science teaching as research assistants are in a good position to judge of its efficacy and are perhaps better qualified than the teachers themselves to make suggestions for its strengthening. The proof of the pudding is in the eating, not in what the cook says about it.

Nine years of teaching and thirty-odd years of experience with the products of others' teaching from colleges and universities in this country, Asia, and Europe have naturally given me ideas. But let us first consider the evidence.

Proficiency in technique is compounded of two elements: awareness of basic principles that are universally applicable and facility in manipulation. Awareness comes from instruction; facility from experience. It cannot be demanded that science graduates be technically facile. But it can be expected that they be sensible of what constitutes proper conduct of experiment. Graduates in chemistry, biology, and kindred subjects have had to be taught the following:

(1) Orderliness in the keeping of records and segregation of data and impedimenta; (2) neatness and cleanliness of worktable and apparatus; (3) avoidance of contamination of experiment with material not pertinent thereto; (4) respect for apparatus, and its use and care; (5) respect for organisms, their feeding and gentle handling, and the desirability of performing experiments under conditions approximating the natural habitat as nearly as possible; (6) a sense of variables, or how and why genetic, chemical, physical, and structural factors of possible interference in experiment and not pertinent thereto should be stabilized; (7) a sense of controls, or how and why test material should match control in all respects save the experimental variable being tested; (8) accuracy in observation and measurement; (9) simple arithmetical procedures; (10) avoidance of conscious and unconscious bias; (11) and that just as one swallow does not make a summer, so not one but many experiments must be run before decision is attempted, and that when organisms are used, many are to be tried.

One could expect that students and teachers alike would accept the following: (1) That identification of control and test glassware is facilitated by marking one set with blue pencil, the

other with red; (2) that worktables cluttered with extraneous supplies are conducive to mistakes; (3) that tobacco-tarred fingers, cigarette ash and smoke are possible contaminants of solutions; (4) that the inside of dishes being used or to be used in experiment is no place for fingers, no matter how seemingly clean these may be; (5) that soap in any form is no proper cleaning agent; (6) that phosphate, HNO_3, chromate-sulfuric acid mixture, and other chemical cleaning agents must be thoroughly removed by repeated (70) rinsings in hot or cold tap water, followed by others (30) with distilled water; (7) that watch glasses, not filter paper, should be used on the balance pan for weighing; (8) that filter paper rather than sulfite-loaded paper towels should be used as the moisture carrier in moist chambers; (9) that to mix solutions by blowing through a pipette is pretty sure to result in fouling of cultures to which such solutions are added; (10) that test and control cultures should be matched for pH, illumination, and temperature; (11) and that many specimens in a single culture dish and infrequent changing of culture solutions are not conducive to reliable results. But it sometimes seems that if there is a wrong way to do things, this will be the way chosen. The amount of time, effort, explanation, and patience required to eradicate bad habits and replace them by good ones can readily be imagined.

What all this shows is that graduates and teachers alike are quite untutored in the fine art of meticulous experimentation. And they are untutored because their foundation does not include apperception and/or working knowledge of one or more of the four interdependent basic attributes of all phenomena, viz., direction, substance, state of substance, and form.

This is also evident in the states of mind with which science graduates approach the problems of research. Some have the idea that science is nothing more than the accumulation of data like that contained in a handbook of chemistry or Pratt's Manual. These envision a problem as occasion for collecting and recording only. Such may become as "sounding brass and a tinkling cymbal." Useful but not very inspiring.

Some have the idea there is no science other than that comprised in the subject of their preference. These envision a problem as occasion for learning more and more about less and less. Digging a pit, they fall into it. For them all horizons are lost.

And some, awed by professorial pontifications, see nothing more in science than the dogmas promulgated by their instructors. All else is heresy. To such, a problem is occasion for proving the dogma right, all other ideas wrong. They know not that facts make the theories, not theories the facts. And authoritarianism is their god.

The degree of possession varies from individual to individual. It ranges from fanatical adherence to innocuous desuetude. But all are possessed by one trait or another. This indicates that science teaching effects a trend to canalization in the thinking of science graduates about research. When development follows this path there is first a groove, then a rut, and finally a molelike tunnel within which the worker bores his isolated way, uninterested in other tunnelers, uninteresting thereto. A digger, not an architect.

Correlative evidence that science teaching directs its disciples toward narrowing rather than expanding points of view is found in the fact that these traits are least noticeable in those who have gone no further than the A.B. or B.S. degree; are only confusedly held by those who have the M.S.; but are in full flower in the Ph.D.s.

These observations suggest that sci-

ence students are indoctrinated with what Charles Fort calls exclusionism, and that those who prepare students for research are separationists in practice, if not in theory. This trend to isolationism obfuscates the functions of science teaching. It results in one-sided interpretations. It leads to impasses, the multiplication of words without meaning, and the development of the defense mechanism of the stuffed shirt.

The foregoing is evidence enough that science teaching should pay more attention to the interdependencies. The almost hermetically sealed walls that separate subject from subject should be broken through. And there should be a shift from subject compartmentalization to intersubject integration.

I am quite aware of the truth that the mass of data accumulated on any science subject is so great that exposing students to it takes practically all the time allowed for its teaching. But I wonder if *all* the data of any subject is ever presented, and if it is, if it can be assimilated or even retained by any student. And I wonder if the time has not come to give up trying to crowd mind and memory with data that can be found in texts, and instead to start from principle, use data to illustrate, and direct students to use texts for data and mind and memory for understanding.

If this were done intersubject integration could easily be accomplished since the data of every science subject are united in the common possession of the basic interdependent attributes of direction, substance, state of substance, and form (Hammett, 1941).

A chemical reaction, an astronomical traverse, a mathematical formula, an organismic evolution, a philosophical concept, each and every one has direction, substance, state of substance, and form. The principle is universally applicable.

It is proper to use biology for exposition since more members of the American Association for the Advancement of Science are interested in biological subjects than in any other branch of learning. In biology, then, direction is comprised in the subject of genetics; substance in that of chemistry; state of substance in physics; and form in that of anatomy.

The pattern of organisms is ineluctably shaped by direction. That pattern is expressed regardless of impinging environmental influences. Pattern is expressed through substance. Direction works through substance to produce pattern. Substance cannot express pattern without direction. And direction cannot be expressed without substance. Direction and substance are therefore interdependent aspects of biological processes. And holistic exposition of biological mechanisms can neither be based on genetic direction alone nor on chemical composition and reactions. Both must be invoked.

Substance exhibits certain states such as color, viscosity, surface tension, electrical differentials, and so on. State of substance is an ubiquitous and basic aspect of organisms.. The states which substance exhibits are the product, not the producers, of substance. But state of substance influences the extent and rate of chemical change and exchange. Therefore, substance and state of substance are interdependent aspects of biological processes and their end products. Direct interdependency between state of substance and direction is not always obvious. But since direction and substance, and substance and state of substance are interdependent, state of substance and direction are interdependent through mediation of substance. No well-rounded exposition of biological phenomena is possible if only the data of genetic direction, chemical composition and reaction, or physical state are used. The contribution of all must be used for understanding.

Form is basic and definitive as aspect of organism. The forms of organisms are set by direction, molded by substance, and limited by state of substance. Conversely, changes in form may affect state of substance, rate of chemical reaction, and extent of genetic expression. Form is thus interdependent with direction, substance, and state of substance. And holistic exposition of biological processes cannot be had from form alone; but only from this, plus the data of genetic direction, plus the data of chemical composition and reaction, plus the data of physical state.

Every biologist knows that organisms are integrated. The foregoing is a brief demonstration of how integration is effected through the interdependence of the four basic attributes of direction, substance, state of substance, and form. *It is apparent that there are no boundaries in science; there are only interdependencies.* This precept is of course generally applicable. Its extension to all teaching could help avert the stupidities of war, racial bigotry, and class strife.

These things being so, it is evident that science teaching can and should direct its attention somewhat more to intersubject integration, and somewhat less to subject compartmentalization. By so doing its utility and effectiveness would be enhanced. The logical basis for such a step is unassailable; its advantages are not obscure.

It is a job for the young teachers. Their minds are likely to be more labile and less irrevocably coagulated than those of the older. It will be an adventure from which much of use may come.

And for biology let there be established in every department a course in biological chemistry equivalent in coverage to that now given to medical students, who, by the way, are really students of applied biology. Let it be required for all students of biological subjects, be these botanical or zoological.

This is advocated because experience shows biology graduates without exception to be incapable of even the most primitive analysis of biological processes in terms of chemical activity. Their ideas thereon are grotesque. They either close their minds to the fact that the organisms they work with are composed of chemical compounds, and the processes they study are expressions of reactions between these compounds; or they take ignorant refuge in the meaningless use of such wastebasket words as metabolism, respiration, enzyme, and the like for their explanations.

It is through chemical composition and reaction that direction, state of substance, and form are expressed. So how can it be expected that any biology student is properly prepared to study organisms holistically if he is unacquainted with the principles and properties of the substances of which they are composed? And who will deny that organisms should be studied holistically?

Clearly something more than lip service could be given to the principle that there are no boundaries in science, only interdependencies. And clearly science teaching should be reoriented on a basis of intersubject integration, if it is to live up to its responsibility of leading all students to cultural and pragmatic knowledge of the integrity of environment, and some to adding to that knowledge.

In 1897 Justice Oliver Wendell Holmes said in an address at Harvard:

> Your education begins when you . . . have begun yourselves to work upon the raw material for results which you do not see, cannot predict, and which may be long in coming. . . . No man has earned the right to intellectual ambition until he has learned to lay his course by a star which he has seen—to dig by the divining rod for springs which he may never reach. Thus only can you gain the secret isolated joy of the thinker, who knows that, a hundred years after he is dead, men who never heard of him will be moving to the measure of his thought.

AN ADVENTURE IN SYNTHESIS

By OLIVER JUSTIN LEE

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Even to the man who labors to increase knowledge because he finds deep satisfaction in this activity and who professes knowledge because he loves to lead students into an appreciation of it, the world of knowledge seems indescribably wide and complex. The present is a far cry from the days of simplicity when a polymath could encompass in his own mind most of the knowledge of his day. This ancient scholar could at least maintain a speaking acquaintanceship with some of the unities, which must have pierced their red ways through most of his intellectual experiences.

Came the age of science, the departmentalization of knowledge, and the resultant "confusion of tongues," as men's researches became deeper and deeper and more and more narrowly confined to their own special areas. What chance, O Unities?

Add to this the tremendous increase in the complexness of life in a modern civilization, with its hysterical sense of speed, its far-flung relationships that span the globe and even reach to the most distant observable galaxy. What hope for you, O Unities?

To the experienced scholar and teacher, no less than to the eager-eyed freshman who comes seeking confirmation into the intellectual world, a glance at the lists of courses of study offered by any large university yields humility and a sense of bewilderment. The latter is hardly mitigated by four or more years of intelligent study.

The student is conscientiously led into the intricacies of knowledge and instructed to criticize, to question everything—the law of gravitation and even his own existence. Overanalysis and lack of perspective often lead to paralysis of the mind and the will. This in turn leads to dissociation of the intellectual world from the outer so-called world of reality into which the young person is thrown willy-nilly to sink or survive in the maelstrom of superficial change under the unhappy aegis of chance.

To be sure, statistics do show that the so-called educated person does make more of a success of his life by traditional standards than he who has not gone through the educational mill. He has gained a certain amount of skill or agility in adjusting himself profitably to his environment. What he studied seems to have mattered less, unless he enters one of the regular professions, in which pressure, applied internally, compels him to learn, to know, and to understand facts. For the rest, the great majority of college people, no basic principle or precipitate of understanding seems to have survived the years.

In the physical world certain laws of motion have been formulated which express the relationships of mass, energy, or force and motion. Newton's Three Laws of Motion and the Law of Universal Gravitation form the basis of Mechanics, and, given the underlying facts, they can be used to express satisfactorily most of the motions in the universe, no matter how complicated and interrelated these may be.

May there not be some similar statements made which will simplify the apparent chaos in our experience with Nature and Society?

Reflection indicates that this can be done. First, however, it is necessary to free our minds from nearsighted attention to *bewildering change* and to drive

our intellects deeper into the matter to search out the *laws of change.*

Change itself defies description except as single cases or categories of cases. The laws of change are few in number and relatively simple. In fact, it is not hard to show that, if we consider the action, reaction, and interaction of basic forces and tendencies in Nature and Society, about the reality of which all earnest minds must agree without debate, three short statements express the results.

1. A surging, pendulum-like swing to and fro, which may result in a static or a dynamic equilibrium.
2. A trend, operating over a long or short period of time, which may or may not reverse itself.
3. Various combinations of these two.

Four years ago sixteen members of the Northwestern faculty (seven, including myself, were heads of departments) got together to talk about such ideas as fundamental forces and processes; balances, imbalances, trends, and equilibria, and the possibility of associating them for our students in our various fields as, let us say, an adventure in synthesis.

Before long, twenty of us, nearly all seasoned teachers, research men, and authors, found ourselves each writing a chapter for a book on the significance of trends and equilibria in the study of Nature and Society. The directions were simple:

1. Do not try to write a survey of your fields.
2. Analyze your material so as to discern basic forces, tendencies, and processes which struggle more or less successfully for supremacy.
3. Select two or three pairs or groups of these contending elements and write about them as simply as you can, after divesting them of most of the adhesions which tend to strangle a subject when we teach it professionally.

Practically every fundamental field of knowledge and expression is included. They range from mathematics to law, from physics to literature, from astronomy, religion, or economics to music and philosophy. It has been interesting to note that in not a few instances the author has discovered a welcome new light upon the analysis of the material in his own field in the process of thinking about and writing his chapter.

The course was approved by the faculty of the College of Liberal Arts in the spring of 1943, and the book has been copyrighted by Northwestern University.

The shadows of the war and our own war efforts closed down upon the enterprise, to be lifted only last summer.

Last spring we selected a small group of superior juniors and seniors for a class and prepared to give the course this past fall and winter.

In the first quarter eleven of us each gave two lectures and conducted one period of discussion in which the class of sixteen usually grilled the lecturer royally. In the second quarter the other ten (geography was added) concluded the course for the year in ten weeks. Next year the course will be open to general but limited registration.

The matters of the student's participation in the work of the course and of evaluating his understanding of its significance have been worked out satisfactorily. It should be stressed that this course is given only to upperclassmen and women who have learned to think independently and constructively and to graduate students, who are assumed to be thinkers.

My colleagues and I are giving it with the earnest hope that the student will find in it something approximating a compass and rudder for his voyage through the apparent chaos of life. We trust it will aid him to develop an intelligent, constructive, and satisfying personal philosophy for living, characterized by confidence in the essential order in Nature and Society, poise of mind, and peace for the spirit.

CRITERIA OF PATENTABILITY

By J. HAROLD BYERS

No one holds a patent on the instinct of contrivance. Aptitude, however, is developed in men in different degree. The difference is due in part to incentive and opportunity, and in part to innate ability. The impulse to invent perhaps may be stimulated by hope of reward, but frequently the creative faculties flourish on lighter fare—on nothing, apparently, more substantial than some inner urge. With some men, invention is a consuming passion.

Scientists by nature and environment are especially inclined and equipped to make valuable inventions. Most scientists have at some time considered taking out one or more patents. Some, for one reason or another, dismiss the intention; others determine to proceed. Those who are members of the research staffs of large organizations that take out patents as a matter of course turn their inventions over to patent counsel.

The benefits of taking out patents include: publication of the inventions; credit for original work; control over important developments; financial remuneration; and protection against the possibility of like patents being taken out by other, adverse, interests. One or more of these benefits may be the dominant motive.

United States letters patent may on proper application be issued to any person if the invention possesses "patentability." A device has patentability if it is *new, useful,* and "inventive" and if it falls within one of the classes of inventions defined in the patent laws. The prescribed classes of patentable inventions include: art, machine, manufacture, composition of matter, and "any distinct and new variety of plant, other than a tuber-propagated plant," which has been asexually reproduced by the applicant. The term "art" by interpretation of court has substantially the same meaning as the word "method" or "process." Examples of "arts" that have been patented are methods of centrifugal casting and methods of manufacturing chemicals. In general, any method which consists of a series of steps in treatment of some material is cognizable by the patent law as proper subject matter of patent. Machines and manufactures cover a fairly obvious field, devices having moving parts being usually designated machines, and those that have none, manufactures, although the line between the two is not hard and fast. Compositions of matter such as plastics, chemical compounds, soaps, and cosmetics can be patented. The classes of inventions patentable are broad but not so broad as to preclude the possibility of the courts' holding that a thing is not patentable merely on the ground that it does not fall within the statutory definitions. For example, the Supreme Court in one case held that oranges coated with borax (to inhibit blue mold) could not be the subject of valid claims inasmuch as coated oranges were not "manufactures."

The requirement that the alleged invention be "useful" does not usually give trouble. Utility, as applied by patent law, does not mean any rigorously high standard of usefulness, but rather that the invention be capable of doing what it is supposed to do. One illustration that has been used to convey what is meant by useful in the patent sense is the match that the patent examiner rejected on the ground that it was inoperative. The story is that the inventor subsequently brought in a box of matches to

demonstrate before the examiner its operability. The inventor endeavored ninety-nine times unsuccessfully to strike a light. Finally the one hundredth match lit. "O.K.," said the examiner. "It's patentable." This story should not be taken too literally.

The requirement that gives the prospective patentee most trouble is "inventiveness," or, as it is more usually termed, "invention." Volumes have been written on the subject. But as yet no objective test or rule of law has emerged. The word invention cannot be defined; nevertheless, the Patent Office and the courts will demand to be shown wherein your proposal "amounts to invention."

Throughout patent history there runs a principle, or philosophy, that is sound in theory but difficult in application, namely, that to grant patents indiscriminately is not in the public interest. The requirement of invention, or inventiveness, is the outcome of efforts to evaluate proposed inventions from the standpoint of their value as contributions to the sum of human knowledge.

Hence, before an inventor can obtain and hold a valid patent he is required to prove that his idea measures up to certain standards of invention. But since there is no objective rule on the matter, the outcome depends upon the opinion of the tribunal judging the case.

The provision in the Constitution of the United States that confers upon Congress power to formulate and enact the laws relating to patents, states in part:

> Congress shall have power . . . to promote the progress of science and useful arts, by securing for limited times to . . . inventors the exclusive right to their respective . . . discoveries.

Accordingly Congress has enacted the patent laws providing that:

> Any person who has invented or discovered any new and useful art, machine, manufacture, or composition of matter, or any new and useful improvements thereof, or who has invented or discovered and asexually reproduced any distinct and new variety of plant, other than a tuber-propagated plant . . . may . . . obtain a patent therefor.

To this provision and law have been ascribed the foundation for the conclusion that the mere fact that a device is novel and useful is insufficient to warrant the granting of a patent; that, in addition, it must possess "invention." One judge put the matter thus:

> In the absence of this element of invention, the patent, if issued, must be held invalid, no matter how novel or useful it may be.
>
> (Smokador v Tubular, 27 F 2d 948)

In order to meet this requirement the inventor is permitted to show, if he can, that his conception or discovery is out of the ordinary. In the Patent Office he may persuade the examiner by advancing arguments contending that the idea is one that would not in the ordinary course of events have occurred to a "man skilled in the art." Or the applicant in the Patent Office may show that his results are unexpected, or that he made his discovery by accident rather than by design.

The courts, in some cases, have gone so far as to require that in order to qualify as being inventive the conception must amount to a "flash of genius." This hurdle, however, is not for the most part as drastic as the use of the word genius would imply. And the Patent Office usually is satisfied if there is some evidence that the stature of the applicant as an inventor is one degree taller than the human yardstick, this fellow "skilled in the art."

If the inventor before the Patent Office can show that he stumbled upon the discovery that is the basis of his invention in a manner analogous, for instance, to the reputed discovery by Roentgen of X-rays, or that the knowledge that he is endeavoring to impart does not follow habitual or logical lines of thought, but just popped out unexpectedly, as it were, he has a good chance of convincing the

patent examiner that his device is patentable.

An argument much used by patent attorneys in behalf of their clients before the Patent Office and the courts is that workers in the art have long been trying to accomplish the particular result of this invention but have failed, and that now applicant has stepped in and succeeded in solving the problem. This angle is sometimes referred to as the doctrine of the "longfelt want."

It is obvious that the prerequisite of "invention" in this view is identifiable as a criterion whereby the tribunals endeavor to evaluate the device from the standpoint of its value to society as a contribution to the sum of knowledge. Since, however, the direct evidence of value at the time the case comes before the Patent Office or the courts usually is a future event, the courts are perforce compelled to resort to indirect evidence, which is less satisfactory than could be desired.

It is clear that in securing patent rights to inventors it is the purpose of the Constitution, as plainly stated in the above-quoted portion, to "promote the progress of science and useful arts." In order to so promote progress, the authors of the patent laws proposed to establish the means whereby a relationship in the nature of a contract could be drawn up between, on one hand, each producer of an advance in "science and useful arts" and, on the other hand, the people, whose representative is the Government. The grant moving to the inventor is his exclusive right to exploit his innovation for a term of seventeen years in whatsoever manner he chooses, but obviously with emphasis on the possibilities for pecuniary profit. The consideration moving to the public is the disclosure of the contribution that the advance represents.

For example: Edison, taking cognizance of the patent laws, devoted his time and ingenuity to the production of new devices, notably, the incandescent lamp. Having produced this invention, he applied for the advantages offered by patent law. The Government thereupon granted to Edison a patent that secured to him the means to control the output for seventeen years. By this grant Edison did not receive any direct reward but only a means whereby he could obtain reward if he undertook to put the incandescent lamp into actual production and succeeded. Presumably, by virtue of exclusive control, Edison derived financial advantages for seventeen years. At the end of this period, the patent expired, the price dropped, and Edison ceased to receive any special profit, but now the public came into full use of cheap incandescent lamps, which, but for the patent laws, they either would never have received or would have received at a considerably later date. This illustration, of course, is oversimplified, but nevertheless describes fundamentally the operation of patent law which justifies its existence.

To be patentable it is not necessary that an invention constitute a long stride forward, nor need it be complicated. The patent law recognizes the virtues of simplicity, and the idea of invention seldom or never involves the idea of magnitude. It is a qualitative, not a quantitative, standard. A small improvement or advance is just as much entitled to patent protection as a large one. It has been held in some decisions that to obtain "absolute simplicity" is the highest trait of genius. Slight improvements are considered patentable, particularly in crowded arts, that is, where many prior inventions have been made. However, although the improvement be small, it still must amount to "invention."

The matter has been explained this way:

Many things appear easy after they have been explained, and doubtless many a man has won-

dered why he failed to think of some apparently simple device or improvement that yielded a fortune to the one who did and revolutionized an industry. The simple fact is that the average person sees things as they are, and he who has originality of vision enabling him to visualize defects and the means of overcoming them should receive adequate reward. (In re Huff, 259 O. G. 386.)

Scientific discoveries as such are not patentable. In one decision the following statement of law appears:

> It is well settled, however, that the discovery of a principle or law of nature, however valuable and beneficial the discovery may be to the human race, can never be made the subject of a patent.

This doctrine does not rest on the theory of invention, but on a separate and distinct ground, namely, that patenting of scientific discoveries is not provided for in the law.

Obviously discoveries, such as Newton's formulation of the law of gravity, do not lend themselves to patent protection. Abstract concepts are not subjects of any property rights recognized by law. In the first place, the difficulties of defining the scope of an idea are formidable; second, it is believed by many that even if monopolies in ideas were possible, they would be contrary to public interest.

But the doctrine that scientific discoveries as such are not patentable has been carried rather further than it is possible to explain with entire satisfaction. While one might unhesitatingly agree that the constant of terrestrial gravitation or a new biological discovery lack patentable connotations, it is perhaps not so clear why the discovery of a new method of fumigating plants or of the use of ether to produce anesthesia are types of matters that have been denied protection by the courts. In each of these cases the Patent Office granted patents, but these were subsequently held invalid by the courts.

The basic decision holding scientific discoveries as such unpatentable is that which rendered invalid Dr. Morton's patent on the use of ethyl ether to render the subjects of surgical operations insensible to pain. The conclusion in this case was curiously vindicated by the subsequent revelation that Dr. Morton was not the first inventor, having been anticipated by an obscure German who published his findings on the subject of anesthesia in some out-of-the-way journal four years before. It may be that the judge who invalidated Dr. Morton's patent had actual knowledge of this prior publication, which, of course, would have invalidated the patent on ground of lack of novelty, but no mention of this circumstance appears in the decision. And this decision on its stated premise came to be the precedent for a long line of cases holding the works of creative discovery unpatentable.

It may be worth while, in view of the consideration that recently has been given to amend the law to include scientific discoveries in the category of things patentable, to review briefly Judge Shipman's reasoning in this case. First, Judge Shipman quoted the statute with the observation that to be patentable an alleged invention must come within the statute. He states that his inquiry was whether or not the Morton discovery was the type of thing that was embraced within the scope of the act. He concludes that it was not. He explains as follows:

> Very little light can be shed on our path by attempting to draw a practical distinction between the legal purport of the words "discovery" and "invention." In its naked, ordinary sense, a discovery is not patentable. It is only where the explorer has gone beyond the mere domain of discovery, and has laid hold of the new principle, force, or law and connected it with some particular medium or mechanical contrivance, by which or through which, it acts on the material world, that he can secure the exclusive control of it under the Patent Act.

Finally, in order to secure a patent, the inventor must comply with the law in all its formal aspects, in which undertaking the services of an expert in patent law are indispensable. The patent application must be drawn in accordance

with the requirements of the Patent Office and in contemplation of the possibility of the introduction of the document in court.

The application consists of prescribed parts: petition, oath, specification, claims, and drawing. Models and specimens are rarely required. The drawing must be in accord with exacting requirements, with which a patent draftsman is familiar but which are full of pitfalls for the inexperienced. Many chemical cases are not required to have drawings.

The application must be prosecuted before the Patent Office in accordance with fixed rules of procedure. Copies of these rules can be obtained on request from the Patent Office.

In the Patent Office the application is given primary evaluation by an examiner familiar with the particular field to which the invention relates. The course of prosecution rarely runs smooth. Normally, the examiner will raise a great many objections to the case, which it is the business of the applicant and his attorney to straighten out.

The examiner always endeavors to show that the invention is not new. Frequently he is successful. It is a matter of amazement to some inventors to discover that their ideas flourished in minds of prior inventors long dead and forgotten. It is impossible, they feel, that their inventions could have escaped adoption. Yet, while anticipations occur, there also is opportunity for novelty. While it is difficult for an inventor to bring into the world something phenomenally or basically new, it is also fairly difficult to conceive independently something exactly like someone thought of before. The contest usually resolves itself into a question of whether or not the novelty involves "invention."

It is upon comparatively small differences that patentability usually is based. Through the family resemblances of the children of necessity there occur individual variations. The examiner stresses the resemblances; the attorney emphasizes the distinctions.

An indeterminate number of alleged inventions are "knocked out" by attorneys in the course of preliminary searches of the prior art. Sometimes this is very devastating to an inventor, who has staked a great deal on his conviction that he had something new under the sun. One attorney told me of showing a client an exact anticipation of his invention. The balked inventor, an engineer who had invested heavily in the development of the device, was so deeply affected by this discovery that he burst into tears. Of those inventions that survive the first ordeal in the patent attorney's office, and are made the subject of applications filed in the Patent Office, roughly 50 percent are casualties.

When and if at last, after months of battle with the Patent Office, the inventor emerges victorious, what rights does the patent confer?

The patent empowers the patentee, or those to whom he assigns his rights, to sue for infringement, to restrain others from practicing the invention, to issue licenses. Some inventors choose to go into business and manufacture their inventions, but the majority are more interested in either granting royalty-bearing licenses or disposing of their patent outright to some firm.

It is not fair to inventors to hold patents up as royal roads to fortune. A distressing minority pay the inventor as much as one cent. There really ought to be attached to each and every patent that issues from the Patent Office the following warning:

> Inventor, beware! This patent is only a license to sue. It does not insure any monetary reward. It does not vouch for the commercial value of the invention. It may ultimately prove to be not valid. What the courts will do to it is beyond prediction. While you were before the Patent Office we tried to regard you as a benefactor of society. But from now on you're on your own. Allah be with you!

GENERAL SEMANTICS AND THE SCIENCE OF MAN

By CHARLES I. GLICKSBERG

MORE than ten years have gone by since Count Alfred Korzybski, a Polish mathematician and engineer, published his magnum opus *Science and Sanity: An Introduction to Non-Aristotelian Systems and General Semantics* (Lancaster: Science Press Printing Company, 1933). And although a second edition appeared in 1941, there are still many intelligent people—including not a few scientists—who do not know what general semantics is.

There are, of course, various schools of *semantics:* general semantics as formulated by Korzybski; semantics, or Orthology, as developed by I. A. Richards and C. K. Ogden; sematics, or logical syntax, as set forth by Carnap. Korzybski, in "Psychiatry, Psychotherapy and Prevention" (M. Kendig, Ed., *Papers from the Second American Congress on General Semantics.* Chicago: Institute of General Semantics. p. 95. 1943), explains his use of the term thus:

> The word *semantics* is derived from the Greek *semantikos,* "significant", from *semainein* "to signify", "to mean". This term was introduced by Michel Bréal in 1897. Originally, and even today, "semantics" is used for the most part in the sense of the "meaning" of words as defined by words, and the significance of words as affecting human reactions has been neglected. It is true that the two terms "meaning" and "significance" somehow overlap, with a resulting confusion and difficulty of analysis. We use the term "general semantics" in preference to the old "semantics" to indicate a fundamental difference between the two. The older difficulties originated because specialists in the "meaning" of words disregarded an unavoidable factor; namely, that any linguistic or mathematical theory must begin with undefined terms which cannot be defined any further by words. In principle these *undefined terms* are labels for direct experiences and observations which involve subcortical processes on the *silent* (*un-speakable*) *level.* Obviously no amount of verbal definition can convey to the individual first order pain, which he has to *evaluate* on the silent, organismal level inside of his skin.

General semantics, therefore, attempts to employ scientific knowledge and the scientific method as an aid to sane living. But how can this be accomplished when each science is technical and specialized, dealing as thoroughly as possible with some isolated aspect of a more inclusive whole? As formulated by Korzybski, general semantics proposes to put an end to this atomization of scientific knowledge and scientific thinking. Science must be both humanized and synthesized. If we are to lead lives that are rational, well-ordered, instinct with meaning and purpose, we must have an adequate picture of "reality" and we cannot draw that picture without the aid of a science which will coordinate and unify the various other sciences. Whatever synthesis we arrive at will be provisional. New knowledge is created at every moment in the dynamic space-time continuum. The scientific synthesis is therefore perpetually unstable, subject to incessant revision. That is why it becomes necessary to "date" our knowledge and the beliefs we base on this knowledge.

Yet scientific knowledge must be applied if it is not to remain abstract and sterile. The supreme task of twentieth-century man is to utilize the knowledge he already possesses in such abundance and apply it to the end of personal and social happiness. Judged from this point of view, general semantics has much in common with scientific humanism. It levies heavy tribute upon every

science, from astronomy and higher mathematics to dynamic logic, psychiatry, anthropology, medicine, and colloidal chemistry, in order to build up a science of man. What it attempts to do is to train people in the consciousness of abstracting and in the proper method of evaluation. They must stop identifying words with things; they must also realize that they live in a four-dimensional world where every point in space has a time-coordinate. The universe of matter is constantly undergoing change; it is dynamic, whereas many of our linguistic counters and conventions are static. We must endeavor to make our language similar in structure to the facts it is designed to represent. The facts must come first, then the words and the definitions, the theories, interpretations, and philosophies. That is the *extensional* method of science. General semantics as a methodological discipline can free us from the prison that linguistic habits and unconscious assumptions build for each one of us.

Count Korzybski was quick to realize that education was fundamental if the semantic reactions of the oncoming generations were to be properly trained. Unfortunately, a majority of teachers, Korzybski charges, are at present largely ignorant of "modern science, scientific method, structural linguistic and semantic issues of 1933. . . ." Korzybski's aim is to educate the new generation to make the difficult transition from Aristotelian and Newtonian to non-Aristotelian and non-Newtonian systems. The goal is not intellectual understanding alone, but a transformation that will affect the organism-as-a-whole. The important thing is to "extensionalize," to look first at the facts, to learn something about the structure of the world, which is the only source and content of knowledge. General semantics is thus a generalized scientific methodology. Scientific thinking is not to be reserved for the lecture hall or the laboratory; it is to become a habit, a series of neurosemantic adjustments to the facts of life, a method of evaluation which will promote survival values at the highest human level.

General semantics in education will not come into its own until teachers make the effort to grasp what general semantics attempts to do, how it does it, and what, under the proper conditions, it can accomplish. It took a long time for science to find a place for itself in the curriculum, and science teaching even now, though it is highly successful in various specialized fields, is failing to achieve what should be its primary objective: the training of the young in scientific thinking so that it becomes as habitual as cleanliness or social politeness. Increasingly the demand is being heard that the basic science course should be required of all students, and that it should be integrated into the secondary school curriculum. That consummation, however, will not take place until the scientific attitude permeates every possible phase of the curriculum. The student as well as the teacher must cease to think of it as a departmentalized unit of instruction: so much technical subject matter to be assimilated in a given course. Here the methodology of general semantics can come to the rescue by humanizing the findings of science and applying them, in a language specially suited to the purpose, to the complex area of social and personal living.

The mistake of those who not so long ago propagandized the schools in behalf of propaganda analysis was that they made doubt an end in itself. Abstractions were to be distrusted because they might be, and often were, employed for evil purposes. General semantics avoids this danger by pointing out that language, though it can be abused, can also be used for effective adjustment to reality. Words are not fixed entities; they are dynamic, chameleon-like creatures

influenced by their contextual environment. They are shaped and colored by the organismic events that precede their emergence. They do more than state facts or single out objects for our attention: they also suggest; they evoke moods; they compose the substance of poetry and prayer.

Words admirably illustrate the theory of relativity. The same word may be multiordinal in nature; it may refer to a single aspect of a manifold of meanings. Light is meaningless apart from darkness; night from day; no from yes; strength from weakness; health from disease. Which aspect of meaning is in question is determined by the context. Language manifests the relativity that is observable in human knowledge. For Nature too is dialectical; things are not solid entities but streams of energy that merge with other streams and appear qualitatively different. Language must reflect this protean and dynamic character of the world.

Language, like the world itself, is constantly undergoing change. If words change in meaning, it is because men change. The dictionary can be of only small help in the search for meaning, except in the sciences, where the meaning of words is stabilized. The dictionary contains but the dead bones of language. Words are not isolated units that can be taken apart, defined, and then pieced together again. The general semanticist tries to make the young realize that words are not things. They are but symbols: maps representing some territory. There are good maps (which enable us to reach our goal) and bad maps (which represent territory never seen on land or sea). Hence we must learn that words mean what we make them mean in a given context. The same word may change its meaning from one sentence to another and even within the selfsame sentence. What is more, if words are symbols, then there are words for which there are no referents in reality: unicorns, griffins, and square circles. Conversely, there are things in reality for which there are no words.

Thus the process that determines the relation between an object or event and a word is extremely complex. The analysis of linguistic usage is no academic matter; it leads inevitably into a discussion of how we know that we know. What is the bond that obtains between verbal symbols and nonlinguistic objects and occurrences? How do we arrive at *meaning?* Furthermore, if words are not things, it follows that they communicate only a highly selected and condensed portion of experience, the part that is focused. No matter what we say about an object or event, something has been left out. That is why language, however detailed and precise in its description, does not exhaust the nature of reality. The important thing is to establish some sort of valid connection between word and object, between object and word. That is to say, language must be "extensionalized." Finally, since words are general, the correspondence between a statement and the experience it represents cannot be quite exact. Bertrand Russell says in *An Inquiry into the Meaning of Truth* (New York, 1940): "There is no point in the growing precision of our language beyond which we cannot go; our language can always be rendered less inexact, but can never become quite exact."

General semantics is concerned with language because language is a vitally important, man-made instrument for helping man to adjust himself to his environment. Adjustment to reality, through language, involves more than the command of a large vocabulary; if that were the case, lexicographers like Samuel Johnson would approximate our ideal of human excellence. No, language as a means of proper adjustment to real-

ity is inescapably associated with the use of dynamic logic. Most books on formal logic tell us how to think, concentrating on method to the exclusion, for the most part, of significant content. The implicit assumption of course is that if we know *how* to reason correctly, we will infallibly hit upon the right conclusions. General semantics demonstrates that this is a *non sequitur*.

And yet there has never been a more crying need for disciplined thinking. There is no reason why logic—dynamic logic—should remain abstract and theoretical. If it is to be dynamic, it must educate "the mind," and the best way of doing that is by attacking entrenched dogmas and popular fallacies and prejudices and exposing them to the light of day. Exercises of this kind are an excellent method of validating the scientific method in action.

We must come to understand that there is an advantage in having many hypotheses competing with one another for our acceptance. In this way, more and more facts are disclosed that must be accounted for. Even the scientifically trained eye observes only a part of all that there is to be seen. Judgment based on *all* the facts is an ideal that science cannot hope to achieve. Its data can never be altogether complete. It is therefore better to entertain a plurality of theories, each of which will demand that we order our facts within the required framework. This puts an end to the two-valued system of orientation: right or wrong, either-or. An infinite-valued orientation makes it clear that no hypotheses are absolutely certain; they possess a degree of probable truth ranging from zero to one hundred percent. Invariably it is a question of more or less; that is the best we can hope for.

General semantics, like science, pictures a problematical universe in which even stubborn, irreducible facts prove to be strangely elusive. They are changeable and subject to correction. A scientific logic consequently trains the young not to have any excessive faith in "facts." It is a superstition, Professor F. C. S. Schiller tells us, "that 'facts' are plain, straightforward, and easy to discover; they are often subtle and recondite and relative to circumstance, changing their aspect to suit their scientific environment like any chameleon." If facts are thus subtle and recondite and changeable, it is not surprising to find that words, too, are caught up and borne along on the tidal sweep of change. The recognition of the instrumental value of words and the role they play in precipitating meaning will help, as Schiller points out, to safeguard us against "the terrible verbalism to which logic has been enslaved!"

General semantics can arm us with potent weapons in the fight against this "terrible verbalism." It has worked out a number of devices (they are no more than that) which can teach students how to counteract the elementalism inherent in traditional language. These are the devices of indexing, dating, the use of "etc.," the hyphen, and quotation marks. If each thing in dynamic nature is unique, complete sameness between any two things does not exist. If language is to be similar in structure to the world it endeavors to represent, it must index its statements and thus achieve an infinite-valued orientation. Smith_1 is not like Smith_2; Negro_1 is not like Negro_2. It is also necessary to cultivate the systematic use of the device of dating statements. The Germany of May 1946 is unlike the Germany of May 1940. To reinforce the attitude of non-allness, it is wise to use "etc." to make it clear that not everything has been said. The hyphen is a linkage device, as in "space-time," to overcome the elementalism that haunts our use of a language still dominated by Aristotelian categories of thought. Quotation marks are merely

warning signals to the reader to beware of the way in which a word is being used. The general semanticist would enclose "mind" and "consciousness" in quotation marks because there are no such separable entities in the human organism.

Not one of these devices, if properly explained, is beyond the grasp of the high school adolescent. To teach them as principles, however, and to establish them as ingrained habits are two different things. One relies principally on verbal mastery, the other depends on assimilating the method and the material so thoroughly that in the future we do not jump hastily to conclusions or act impulsively. A kind of automatic neurological delay is effected.

Another advantage in the teaching of general semantics is that the student uses himself as a guinea pig: his experiences constitute his laboratory material; he makes his own scientific discoveries and applies them to problems with which he is personally familiar. In this way the "course of study" remains perpetually fresh and challenging and new. There are no rigid boundary lines, no compulsory textbooks. Take the device of non-allness. As an abstract principle it can be "taught," copied down in a notebook, and memorized in a few minutes. Nothing is gained thereby. The student persists in his old ways, like the sinner who backslides after listening to a soul-stirring Sunday sermon. He has only acquired another bit of interesting but useless information. The device should be withheld until the very last. Only after the student has worked out the application should the idea be driven home. The first step in applying the device of non-allness is to consider what a "fact" is—a most complicated procedure. Philosophers have spilled Niagaras of ink over this highly complex and controversial issue. When is a fact a fact? How do we know a fact when we encounter it?

"This pencil" (holding it up for the class to see)—"is it a fact?" Certainly. Well, how does one prove that the pencil "is"? The senses are our telltale reporters. We depend on them to help us understand the world of things aright. But do not the senses often deceive us? Sometimes we see things that are not there and hear things said that are not spoken. Witnesses are notoriously unreliable in their testimony. Neither the eyes nor the ears nor the other senses are perfect registering instruments. What we do is to verify our impressions by comparing them with those of other presumably qualified observers. Thus we obtain a sort of working agreement.

Even at that, "the fact" is still conjectural, an opinion, and not an ironclad, indubitable reality. Scientists have frequently emphasized this point, namely, that our postulation of a reality outside of us is a hypothesis and nothing more. We accept it as valid for the simple reason that it does not as a rule play us false. When we do not go wrong, we assume that our hypotheses concerning reality must be correct. Sometimes, however, our senses are not acute enough to perceive a "fact." How do we detect microbes, viruses, vitamins, electrons? We cannot see them, touch them, smell them, hear them. What we do is to devise scientific instruments which are, as it were, extensions of our five senses. They make recordings of what is happening on the microscopic and submicroscopic level. They furnish important evidence of what would otherwise remain unknown. Then we interpret this evidence and obtain a rough approximation of what a fact is—nothing but a conventional agreement.

The world of reality has now been disintegrated, broken down from its pulsating actuality and organic wholeness. A pencil mirrors the state of the universe in flux. The terrible doubt of appearances has overcome the student and he is

no longer the same. He begins to question, to analyze, to look behind the veil of solid, substantial reality. To the uninitiated who depend on common sense this may seem very much like quibbling. What difference does all this make?

It makes a great deal of difference. Our understanding of the world and of ourselves, our interpretation of reality, hinges on the determination of this issue: When is a fact a fact? How do we know that we know? Even if convinced that the methodology of general semantics does make a difference, the doubting Thomas is apt to ask what he considers a disconcerting question: "Can it be taught?" That is the question, usually with damaging implications, which often meets the proposal that general semantics be made an integral part of modern education. The only effective way of answering this question is with a *fait accompli*. When Alice asked, "What *is* a Caucus-race?" the Dodo, the reader will recall, replied : "Why, the best way to explain it is to do it." Which is, by the way, an excellent example of general semantics in action, even if the Dodo had never heard of it.

To come back to the device of non-allness. Our language is shot through with the assumption of "allness." There is a tendency, too, to strengthen our case, whenever we generalize, by making it universally applicable. Writers of advertisements will say, "All women are wearing this style of hat now." There used to be an old popular song which began: "Everybody's doing it." Proverbial expressions, the homely wisdom of common sense, testify to this linguistic habit. If, however, we extend the device of non-allness to other areas of experience, its significance and range of application become much clearer. One sin does not make a sinner. One crime does not make a confirmed criminal. One disappointment in love should not make us condemn all women as two-faced. A number of disillusioning encounters with the worst characteristics of human nature—selfishness, treachery, baseness, corruption—should not lead us to conclude rashly that all human nature is essentially vile.

The aim of general semantics, once more, is not to make students suspicious of the uses of language. They are not trained to hunt for a fallacy lurking in every statement. What it does is to provide them with exercises which will make them realize the nature of language and thus be forewarned and safeguarded against the abuses to which words can be subjected. They are not left with the dismal feeling that all language is a trap, that its main purpose is to betray, that all discourse is a species of cunning propaganda. No, language is meant to serve as a map.

General semantics seeks to make this linguistic map correspond as closely as possible in structure to the territory it is supposed to represent. After a time, students trained in general semantics derive considerable amusement from spotting "loaded" words like "globaloney," "Jew Deal" for "New Deal," "crackpot," "freedom of enterprise," "utopian," "radical," and "red." They enjoy studying the art of verbal camouflage as it functions during wartime. On September 26, 1943, the *New York Times* printed a release from the Office of War Information, which listed thirty different ways employed by the Germans to disguise the unpalatable facts of retreat. These propagandistic alibis from the German military lexicon make fascinating reading: retreats become "planned withdrawals," "defensive successes," "successful disengagements," "unencircling maneuvers," and so on.

The most beneficial lesson that general semantics has to teach deals with the consciousness of abstracting. What Korzybski emphasizes again and again in *Science and Sanity* is that we cannot

open our mouths without abstracting, and that it is to our interest, if we wish to lead sane lives, to become conscious of this process. Geometry, for example, deals with the abstractions of actual things in the world. The only difference between these abstractions and those fathered by the celebrated man in the street is that the former are exact and the latter are approximate. With exact abstractions we can gradually secure mastery over our environment. With loose, makeshift abstractions we not only make serious blunders but also bring about costly nervous maladjustments. What distinguishes man from the animals is precisely this power to abstract.

If abstractions are fundamental to the business of sane living, it is essential that we learn how to use them intelligently. Talking about language is futile. It only serves to confuse. It is better procedure to take some text with which students are already familiar and which they think they understand. For example, what does the saying, "All men are created equal," mean? What are the facts? Are men equal physically, financially? In skill, strength, talent, beauty, intelligence, length of life? Decidedly not. Then what does the phrase mean, if it means anything? Are the runners before a race equal? Are all students in the classroom equal? Gradually students learn that there are no absolutes, no hard-and-fast oppositions. A thing is not either good or bad, equal or unequal, superior or inferior, right or wrong. Such two-valued orientations are false to fact.

Where general semantics is to be taught is a problem that can best be decided by experimental practice. It has been applied with notable success in such fields as English, public speaking, mathematics, and even medicine. Where it is most needed and where it will probably achieve the greatest measure of success is in the teaching of science on the secondary school level. The habit of scientific thinking and a corresponding mastery of language as a man-made instrument—if general semantics can bestow these twin gifts on the young there is no reason why it should not be incorporated in the curriculum, no matter how "revolutionary" such an innovation may seem to the upholders of the educational *status quo.*

CONSIDERATIONS IN REGARD TO TAX CAPITALIZATION

By CARL F. WEHRWEIN*

THE burden of some taxes does not remain upon the persons or concerns who turn the money over to the government but is shifted by them to others. The burden of a new tax levied upon a merchant, for example, is shifted to his customers if he, because of the tax, raises the prices of the goods he sells. A merchant may not in every case of an increase in his taxes be able, however, to raise the prices of the goods he sells because his customers may, owing to the availability of acceptable substitutes sold by other merchants, refuse to pay the higher prices. The taxes levied upon property which endures either perpetually or for a long time (land or land and substantially built buildings) are sometimes shifted by the process of capitalization. This can occur only when such property is sold, and the taxes which it is expected will after the sale be levied upon the property are shifted from the buyer to the seller. In brief, this is done through the sale price, the price being lower than it would be if no taxes were going to be levied. The fact that the taxes reduce the capital or sale value of the property is the reason the term "capitalization" is applied to this method of shifting. In connection with every tax, the question of its incidence—the person or group upon whom the burden will finally rest—naturally is of great importance.

Much has been written about the capitalization of taxes, particularly in connection with the taxes levied on land, but there still is considerable confusion and difference of opinion in regard to this process. I shall try to show clearly (1) that in spite of the capitalization which takes place, the burden of all the taxes levied on land (in this discussion the term "land" should be understood to mean real estate,—that is, land and buildings) is borne by the landowning group, and (2) that some of the capitalized taxes do not stay capitalized.

It is generally agreed that in the cases of certain sales of land the taxes which are later levied on the property are not capitalized. In general, if land is sold when the market is a sellers' market, the purchasers cannot capitalize the taxes which will thereafter be levied on the property. In a sellers' market, the sellers are in an advantageous position as compared with the buyers—the demand for land is high, and the amount for sale is relatively low. A good illustration of a sellers' market is the period of the farm land boom which followed the first World War. Of course the burden of all land taxes which cannot be capitalized when the property is sold is borne by the purchasers, assuming that they cannot shift the taxes in some other way. In the cases of certain other sales of land, however, the taxes subsequently levied on the property are capitalized. In general, if land is sold in periods of buyers' markets (the opposite of sellers' markets), the purchasers of the property are able to capitalize and thus shift to the sellers the future taxes which will be levied.

It has been loosely argued that to the extent of the land taxes which are capitalized, the burden of this type of tax is

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lighter than has been claimed, and that complaining about the burden insofar as it involves these particular levies is not justified. This of course is true as far as the landowners whose taxes have been capitalized are concerned. However, it must be remembered that the process of capitalization of taxes on land consists merely of the shifting by the purchasers of such property of the taxes which will subsequently be levied on it to the sellers, who are other members of the landowning group. The sellers who after the sales have been made still own some land continue to be members of this group. However, even the sellers who after the sales no longer own any land should, as far as the taxes shifted to them by capitalization are concerned, be considered still to be members of the group, because it can be assumed that the losses they thus sustained came out of the wealth or assets they accumulated in the process of the ownership and operation of the land sold. Hence, the burden of all the taxes levied on land is borne by the landowning group (assuming that the taxes are not shifted to others by some method other than capitalization), and the arguments that have been advanced to the effect that these levies are excessive cannot, as far as these property owners as a group are concerned, be qualified by any claim of part of the burden having disappeared as a result of its capitalization. Of course, as between the members of the group, the burden may be inequitably distributed.

Professor Harold M. Groves has claimed (*Financing Government,* New York: Henry Holt. p. 123. 1939) that if land is purchased under circumstances favorable to tax capitalization, the purchaser "buys free of and capitalizes only the differential tax burden on the particular as compared with alternative investments" (the excess, if any, in the taxes on the land above those levied on alternative investments). Groves repeats this argument at another place (p. 150), as follows:

> It should be remembered . . . that capitalization and shifting even under the most favorable circumstances do not entirely absolve the taxpayer from burdens. Capitalization relieves only to the extent that taxes on one investment are higher than those upon others.

It would appear that this would be true only if the taxes on the alternative investments could not be capitalized. The future levies on land purchased can be capitalized to the extent of not only this differential but also the capitalized taxes on the other possible investments.

However, my main point is that certain capitalized land taxes do not stay capitalized but by a psychological process revert to the purchaser of the property. Let us suppose that a farm is sold and the conditions are such that the taxes which will thereafter be levied on it are fully capitalized. Let ten years go by. Will the purchaser still claim that he is not bearing the burden of the taxes currently being levied upon the farm? I do not think so. He will by that time be complaining about taxes as much as anybody else. He will have shifted his psychological base in regard to those taxes. There are two reasons for this change in his attitude. In the first place, the money which he will be turning over in payment of the taxes will be money that he has earned by his own efforts in operating the farm. In the second place, the fact that those taxes were capitalized when he purchased the farm will have faded into the distance, and, if he has not already forgotten all about that fact, it will have become so hazy as to appear unrealistic to him and can no longer serve as a sound basis for his views regarding the taxes being levied on the farm. Hence, the burden will seem to be resting on him.

Similarly, the seller will after the expiration of the ten years no longer be feeling that he is bearing the burden of the

taxes currently being levied upon the farm. During all the time since the sale he will not have been taking money out of his pocket to pay the taxes on the property, and the loss he sustained in the capitalization process will have pretty well faded out of his memory. It will seem remote and impersonal to him, and he will have stopped considering it in formulating his views regarding the taxes he is currently paying.

Certainly most buyers and sellers of land in the cases in which the new ownership situations created continue to exist for a considerable period of time undergo these changes in attitude toward the capitalized taxes. This is, therefore, for all practical purposes a general situation. It may be argued that, though land taxes may not stay capitalized in a psychological sense, they must actually remain capitalized in an economic sense. But if they do not stay capitalized in a psychological sense—that is, if, on the one hand, the owners of land after the expiration of a period of years generally become convinced that they are bearing the burden of the taxes, and, on the other hand, the last sellers of the land, after the same period of time generally feel that they are no longer bearing the burden, and if all other people, including politicians and lawmakers who may pass laws upon the basis of this view in general agree with them—do the taxes stay capitalized in the economic sense? I do not think it would be realistic to say that they do. During this period of time the owners and the last sellers will have adjusted all their related economic circumstances to, or built them up on the basis of, this view.

This change in attitude on the part of the buyers and sellers of land toward capitalized land taxes is natural and normal. The same type of thing happens in connection with unsecured debt, especially if the amount involved is relatively small. This is the basis of the statutes of limitation regarding debt. These statutes provide that if no part of an unsecured debt or of the interest thereon is paid, or the borrower does not in some other way reacknowledge the debt within a certain period of time, the debt is "outlawed," and the legal claim of the lender against the borrower terminates. These statutes are based upon the fact that if no part of an unsecured debt or of the interest thereon has been paid within a certain time, the lender has abandoned his claim; he has in his own mind written the debt off; he has fully adjusted himself to the nonrepayment of the money. Here, then, is another instance of the acceptance with the passage of time of certain circumstances as being permanent or realistic which had originally been expected or understood to be temporary or unreal.

Hence, it appears that the capitalization of land taxes is only a temporary phenomenon. Taxes levied on any land, if they are capitalized at all, remain capitalized only during a certain period of time, and at the expiration of that period the burden reverts to the purchaser. Groves also touches on this in a discussion of the capitalization of taxes on farm land, as follows (p. 150):

> . . . much farm land remains for many years in the possession of a single family. Of the 3,800,000 farm operators who owned their farms in 1935, about 44.5 percent had been operating their farms for a period of 15 years or more. . . . Insofar as farmers retain their farms, . . . the capitalization process does not operate.

What is true of land taxes in this regard is in general, for the same reasons, also true of taxes levied upon other types of tangible property.

Economic theory should not be made to be more logical or consistent than human beings or their economic activities themselves are.

SEEING SUMMER SOUNDS

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FROM midsummer to early autumn there is a wealth of outdoor sounds that we have scarcely dreamed of until we have made a conscious effort to listen for them. The human ear is a remarkable acoustic instrument. However, the average person cannot tell the pitch nor the loudness level very accurately without some form of detecting meter to make the sound *visible,* as it were. Some of the "sounds" may even fall outside the frequency range of 16 to 16,000 cycles per second that can be heard by the average person with good hearing. A few people can hear 22,000 cycles per second if the intensity is large enough.

On August 14, 1945 (V–J Day), I was using a General Radio Sound Level Meter to measure the intensity level and the loudness level of sounds produced by insects in my flower garden. When the State College V–J celebration was well under way, its intensity level was measured and found to be about 70 decibels at $\frac{3}{4}$ mile from the business district. The level for ordinary indoor conversation is about 60 decibels (Fig. 1). To carry on a conversation through this V–J noise, the speech level had to be raised to 75 or 80 decibels. At half the above distance, ($\frac{3}{8}$ mile) the V–J noise was probably about 76 decibels.

The intensity level of a sound is measured in decibels, and the intensity itself is measured in watts per cm². The level is said to be zero decibels for an intensity of 10^{-16} watts/cm². A 1,000-cycle note of this intensity is at the threshold of hearing (just audible) for a person with acute hearing. Ordinary conversation (60 db) represents 10^{-10} watts/cm.², which is one million times the threshold intensity. Ten decibels are added to the intensity level each time the intensity at the ear or the power of the source is multiplied by ten. The loudness, or noise, level is measured in phons by adjusting the loudness of a controlled 1,000-cycle sound to an equal loudness as judged by an observer. Then the loudness level in phons of the first sound is said to be equal to the adjusted intensity level in decibels of the second sound. The unit of loudness level is sometimes called the decibel also, but the loudness level of a sound in phons and its intensity level in decibels are not in general the same. They are equal, however, if the sound is a 1,000-cycle note. At 35 cycles per second, the threshold loudness (zero phons) corresponds to about 63 decibels (Figs. 1, 2).

Some of the bombing in the recent war was accomplished by radar without even a glimpse of the target. Bats avoid obstacles in the dark by means of sound waves whose frequency is too high to be heard by man. In a somewhat similar way in a dozen or more cases I have located slender meadow grasshoppers, *Conocephalus fasciatus,* by means of a meter for high-frequency sound waves. Both long- and short-winged species were located. Both are long-horned. Before it was known that these grasshoppers produced 40-kilocycle sounds, I was exploring with the meter in a general way when at a certain spot the meter deflected strongly. The spot could be located to the nearest 2-foot cube, but no insect was found. On the second afternoon nearly the same spot was located in the flower bed, and again no insect could be found. On the third afternoon a more cautious procedure

located the insect to the nearest few inches, and then it was seen. Its color matched that of the plant on which it sat, and it managed to keep almost hidden by the plant. In each case, maximum deflection of the meter occurred when it was tuned to 40 kilocycles per second. The intensity level 4 to 6 inches from the insect was about 75 decibels. Because there was also a smaller deflection at 20 kilocycles per second, an as-

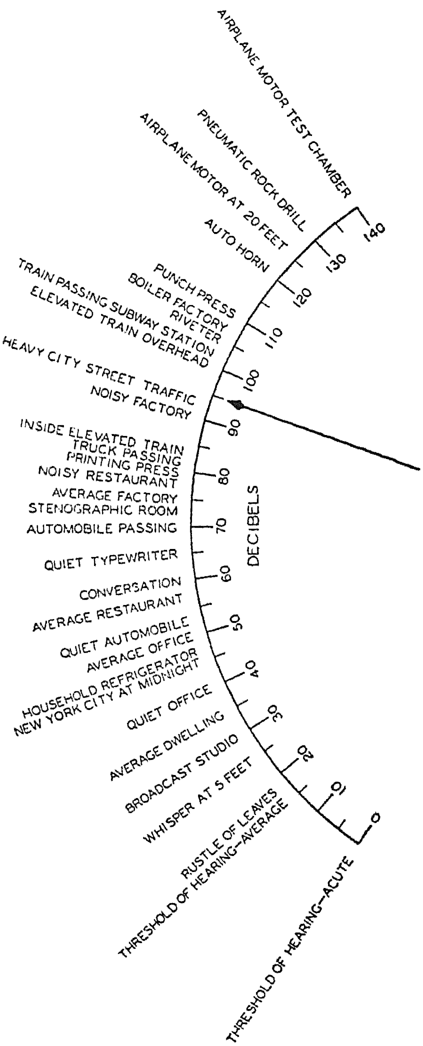

FIG. 1. INTENSITY OF NOISES
TYPICAL NOISES AS MEASURED IN DECIBELS.

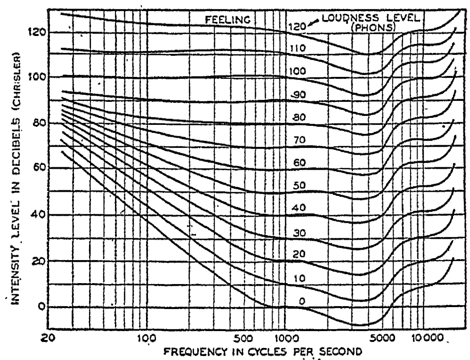

FIG. 2. FREQUENCY vs. INTENSITY
EQUAL-LOUDNESS CONTOURS FOR AVERAGE EAR.

sistant whose hearing range extends to 22 kilocycles per second was called to listen. He could hear the 20-kilocycles-per-second component quite clearly and also a weak sound of lower frequency. Then the grasshopper was caught. It was identified as *Conocephalus fasciatus* by Professors S. W. Frost and V. R. Haber, of our Department of Zoology and Entomology. They also identified the others named herein.

Later tests at several distances showed that the greatest intensity level in the 40-kilocycle band was 86 decibels at a distance of 10 cm. Long (*fasciatus*), intermediate (*gracillimus*), and short-winged (*strictus*) species of *Conocephalus* were tested. The output of the last excelled by 1 or 2 decibels—probably because the movement of its shorter wings is resisted less by the air. Since these grasshoppers are less than $\frac{3}{4}$ inch long, the inverse square law of intensity versus distance held in the range 3 to 6 inches. Beyond 1 foot the excessive absorption and ground reflection caused deviations. Dozens of tests showed that results could be duplicated to within 2 decibels. If our ears were as sensitive to 40 kilocycles as they are to 3 or 4 kilocycles, we could hardly carry on a conversation if several of these slender meadow grasshoppers were singing in our midst.

Since the slender meadow grasshoppers must be nearer than 3 feet to be heard by people with acute hearing, it is difficult to locate them by means of their audible sound. This sound accompanies, and is timed with, the supersonic (high-frequency) components, which cannot be heard by anyone. I found that a few of the larger species of the long-horned grasshoppers, which can be heard at distances of 40 feet or more, can also be detected at close range with the supersonic meter. One of these is *Neoconocephalus ensiger* and another is *Orchelimum vulgare.* The latter can often be found at the edge of cornfields. It frequently sits on the corn tassels and sings, "Tip, tip, tip, tseeeeeee." In neither case was there an output peak beyond 20 kilocycles per second. The response of the meter diminished consistently as it was tuned to higher frequencies. The slender meadow grasshopper, *Conocephalus fasciatus,* stridulates in about the same manner, but the audible part is much weaker, and the supersonic part is very intense at 40 kilocycles per second (76 db at 1 ft.).

It was suspected that the weak sound of 67 cycles per second from *Orchelimum vulgare* was produced by the scissors-like action of the wings. If so, one would expect the scraper on the right wing to go back and forth 67 times per second across the file on the under side of the left wing. (Of course the file would go through a similar motion.) When a strobolux was secured, the same grasshopper was no longer at hand, but another of the same species was tried, the frequency of the light flashes being varied over the range from 50 to 70 per second. As the rate approached 60 per second, the wings appeared to slow down. The stroke was about 1 mm. At 60 flashes per second, the motion was frozen. Another grasshopper of the same species was tested with the sound analyzer. A peak was found for it at 58 vibrations per second. Another was tried with the strobolux and was found to make 61 vibrations per second. This is conclusive evidence that the wings of this species work back and forth about 60 times per second during the few seconds of stridulation. The 7.3 and 14 kilocycles per second are probably resonance frequencies of small sections of the wings. The tympanum probably produces the intense 14-kilocycle sound.

EVAPORATION REGIONS IN THE UNITED STATES

By STEPHEN S. VISHER
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THE rate of evaporation of water is an environmental influence of major importance and merits continued study. Several prolonged investigations of local and regional differences in evaporation have been made by biologists. The accompanying maps present some recent data, chiefly gathered by nonbiologists, which supplement earlier studies in significant ways. This is my tenth article in THE SCIENTIFIC MONTHLY on various aspects of the climate of the United States.

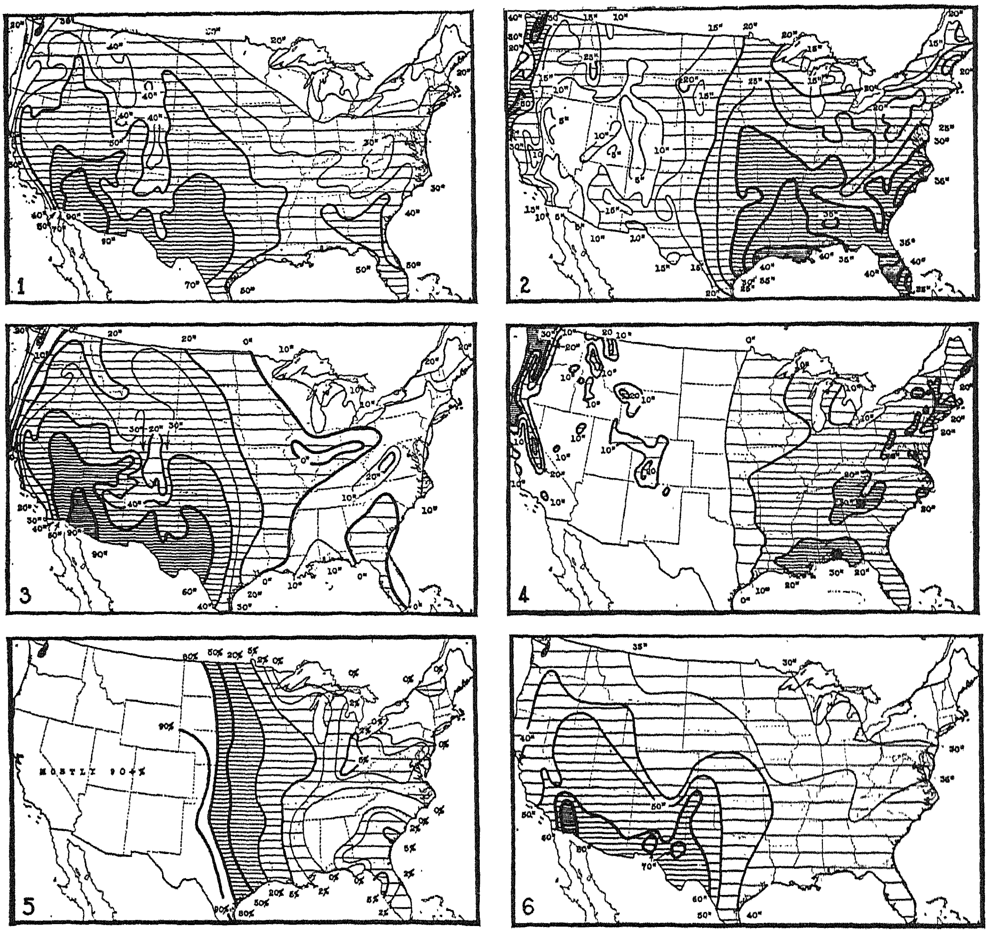
Map 1 is a somewhat simplified, shaded copy of a map published in 1942 by Adolph Meyer, the distinguished hydraulic engineer, of the annual loss by evaporation from shallow lakes and reservoirs.[1] It shows a southwestward increase from the Great Lakes, where the loss is about 20 inches a year, to the lower Colorado River Valley, where the loss is more than 90 inches. The North Pacific Coast also loses less than 20 inches a year, while much of Florida and Georgia loses more than 50 inches.

Although the coldness of Western streams and lakes is commonly ascribed solely to melting snow at their sources, it must be attributed in part to rapid evaporation. The cooling effect of evaporation helps to explain the occurrence in Western streams of trout that require cold water. The relatively small amount of evaporation in the Northeast permits many streams and lakes to exist even though the precipitation is not large. Conversely, despite an annual rainfall of more than 50 inches in the Southeast, many streams shrink badly in summer.

Map 2, a shaded copy of a 1938 map by Kittredge, shows the annual water loss due to transpiration and evaporation.[2] This map shows only a small loss in the Southwest, although reservoirs and other water bodies there lose heavily (Map 1). In this arid region there commonly is little water available for transpiration from vegetation or evaporation from the soil. Instead, the ground dries out deeply, and the vegetation is characteristically xerophytic, with remarkable moisture-retention. This map shows that the greatest losses are in the Southeast and in parts of the Northwest. This map resembles one of average annual precipitation much more closely than does Map 1.

Map 3, after Adolph Meyer, shows the difference between mean annual evaporation and precipitation. Heavy zero lines mark boundaries where, on the average, evaporation equals precipitation. One such line is situated not far from the Mississippi River, another extends from central California to north-central Washington, and a third lies in the Southeast. In most of the eastern half of the country the difference between annual precipitation and annual evaporation is less than 20 inches, but in much of the Southwest evaporation is more than 60 inches a year greater than precipitation. Conversely, in the Northeast and Northwest, precipitation is 10 to 20 inches or more greater than evaporation.

The unshaded area, where precipitation exceeds evaporation, is well forested whereas the shaded area, in which evaporation exceeds precipitation, ranges from subhumid prairie to desert. Roughly,



MAPS 1–6. EVAPORATION AND PRECIPITATION IN THE UNITED STATES
1: MEAN ANNUAL EVAPORATION FROM WATER BODIES (INCHES). 2: MEAN ANNUAL EVAPORATION AND TRANSPIRATION FROM LAND (INCHES). 3: MEAN ANNUAL PRECIPITATION VERSUS EVAPORATION: *unshaded,* PRECIPITATION IN EXCESS; *shaded,* EVAPORATION IN EXCESS (INCHES). 4: MEAN ANNUAL EXCESS OF PRECIPITATION OVER EVAPORATION ON THE LAND (INCHES). 5: PERCENTAGE OF THE YEARS THAT HAVE LESS PRECIPITATION THAN EVAPORATION AND TRANSPIRATION. 6: MEAN EVAPORATION FROM APRIL TO SEPTEMBER, INCLUSIVE (INCHES).

the zone in which evaporation exceeds precipitation by not more than 20 inches is subhumid and mostly a prairie; wherever the difference is more than 40 inches, aridity generally prevails, and scattered shrubs, with little grass, are characteristic. The dominance of evaporation in the West makes for salt lakes and saline or alkaline soil, with significant ecological responses.

Map 4 also depicts average annual excess of precipitation over evaporation. It is based on a map by Hoyt,[3] who is a specialist on water supply of the U. S. Geological Survey. It differs radically from Map 3 because it deals with the land, whereas Map 3 deals with water surfaces. The zero line of Map 4 lies near the western margin of the tall grass prairie or black-earth soil region. To the west of that line, the unshaded areas normally have no annual excess of precipi-

tation to leach soluble minerals from the soil. Hence an excess of minerals is usually present in Western soils, which are classed as pedocals, because lime is abundant. Conversely, in the Northwest, South, and East, where precipitation exceeds evaporation, there is much soil leaching, and lime and most other plant food minerals are inadequate.

Map 5, also after Hoyt, shows the percentage of the years which have less precipitation than an amount sufficient to meet the demands of evaporation and transpiration. It shows that in about half of the East precipitation is normally greater than evaporation and transpiration. Indeed, in parts of the Northeast and Southeast a deficit in precipitation occurred in less than 2 percent of the years studied. West of about the hundredth meridian, however, more than half of the years have a deficit, and in most of the West, nine-tenths of the years.

The amount of evaporation during the warmer half-year (April to September, inclusive) is indicated roughly by Map 6, an original map based on scattered records assembled by the U. S. Weather Bureau.[4] It shows that where water is available, as in the evaporation pans used to get these records, about twice as many inches of water evaporate in much of the Southwest as in the Northeast. In about half of the country, however, the water loss during this half-year is 30 to 40 inches. Thus, there is less regional contrast than might be expected. This is because most of the country differs only moderately in temperature in summer, and temperature profoundly affects evaporation. The northwestward extension of the zone of loss of 30 to 35 inches is associated with the relatively higher summer temperatures in the northern interior than in the cloudier East.

During the average frost-free season, which means a season of varying length in different parts of the country, the ratio of precipitation to evaporation (Map 7) is more than unity* only near the Atlantic and eastern Gulf coast, in the northern Great Lakes region, and in western Washington. For about half of the country, evaporation from suitably exposed evaporation cups is more than twice as great as the precipitation. In most of the West it is more than five times as great. This map is a shaded copy of one by Livingston and Shreve, botanists.[5]

Map 7 also shows that the chief deciduous forest region has a ratio of 70 to 100 between precipitation and evaporation during the frost-free season. The regions where the ratio is more than 100 are characterized by coniferous forests, or at least have many conifers mixed with deciduous trees.

The seasonal variations in evaporation from lakes and reservoirs is large wherever the annual temperature range is wide. For example, Maps 8 and 9 (after Meyer) show that in North Dakota about 15 times as much evaporation occurs in July (Map 9) as in January (Map 8), although July is one of the rainiest of the year and January has least precipitation. In Indiana the range between January and July is about eightfold, but in Florida it averages only about twofold. An interesting detail presented by these two maps is that there is much more evaporation from the Gulf Stream in January than in July because its water is warmer than the air in winter and cooler in summer.

Comparison of Maps 8 and 9 shows that most of the West loses less than an inch of water from a reservoir in January but loses from 8 to 12 inches in July. The large water loss in central Texas in July and the relatively heavy loss in January is an interesting indication of the aridity of that area, which resembles arid west-

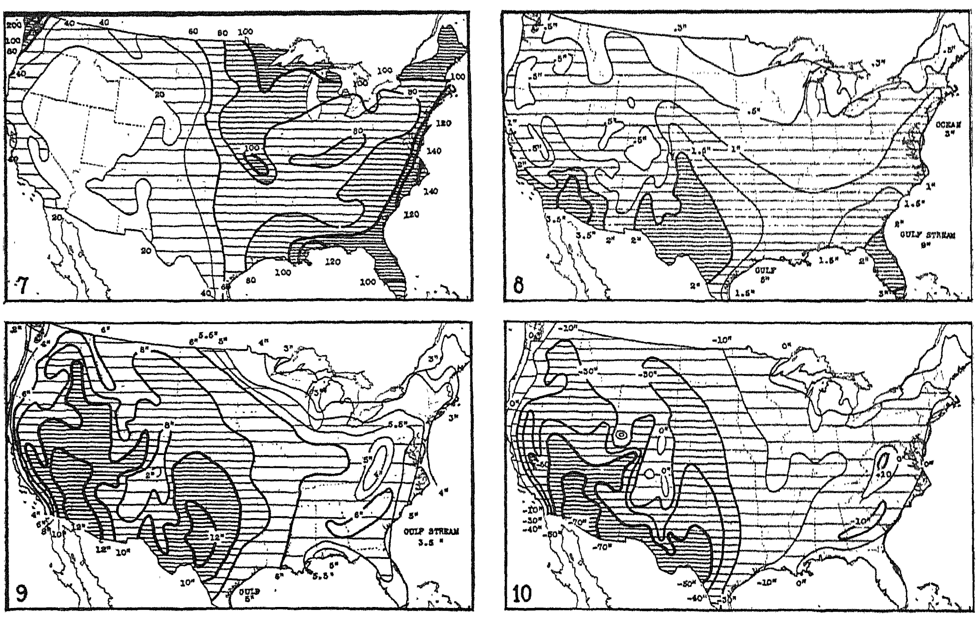
* The ratios are multiplied by 100 on Map 7; equality of precipitation and evaporation is therefore represented by 100.

ern Arizona more than it does most of New Mexico, for example.

The conspicuous southward increase in evaporation from reservoirs and shallow lakes in January (Map 8) reflects the sharp latitudinal difference in average temperatures and is associated with regional contrasts in biologic activity. In July the north–south contrast is much less, except near the Great Lakes (Map 9). This is a result of greater temperature uniformity in most of the country, due in part to the fact that the days are enough longer in the North in July almost to offset the effects of the lower noon height of the sun. The cooling influence in hot weather of the Great Lakes is reflected in Map 9.

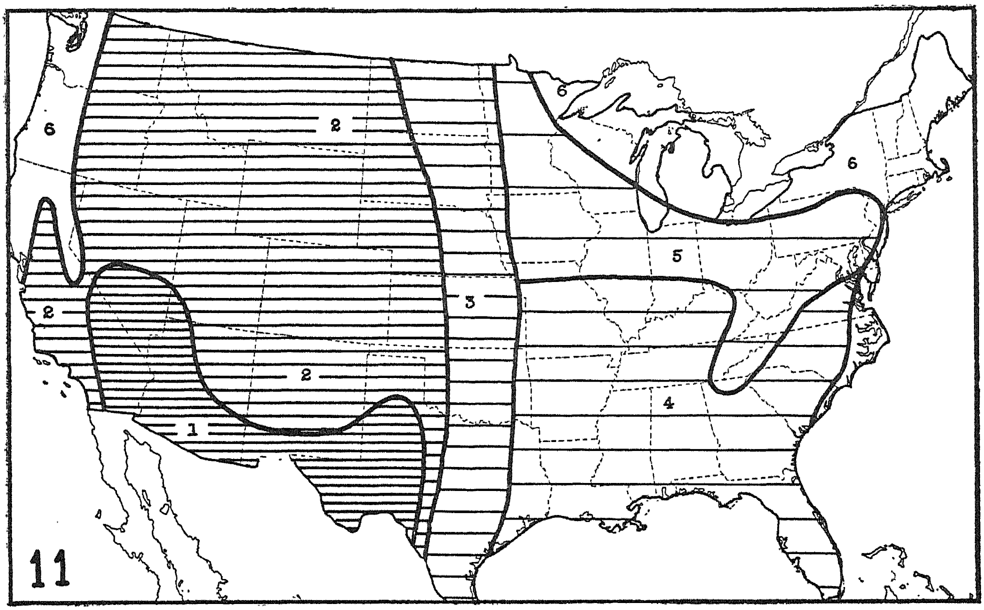
Map 10, also based on one by Adolph Meyer, shows the average differences in the totals of evaporation and precipitation during April to October. Only near the country's borders (northeastern, eastern, southeastern, and northwestern) does precipitation normally exceed evaporation from reservoirs in this period. The excess of evaporation over precipitation is, however, less than 10 inches in most of the eastern half of the country and in a Pacific belt. In the Great Plains region and in much of the West, it exceeds 30 inches; in the most arid section it exceeds 70 inches. The zero line near the east coast is situated approximately where the 100 ratio line is on Map 7 (for the average frost-free season). The zero line near the Pacific coast is, however, quite differently situated than the 100 line on Map 7 and more closely corresponds with the type of vegetation. Wherever there is more precipitation than evaporation during April to October, coniferous or mixed forests prevail.

The final map (11) is an attempted regionalization on the basis of evapora-



MAPS 7–10. SEASONAL EVAPORATION AND PRECIPITATION

7: PRECIPITATION-EVAPORATION RATIOS (×100) DURING THE FROST-FREE SEASON. 8: MEAN EVAPORATION FROM LAKES IN JANUARY (INCHES). 9: MEAN EVAPORATION FROM LAKES IN JULY (INCHES). 10: MEAN APRIL–OCTOBER RAINFALL COMPARED WITH MEAN EVAPORATION (INCHES).



MAP 11. EVAPORATION REGIONS IN THE UNITED STATES
1: EVAPORATION EXCESSIVE THROUGHOUT YEAR. 2: EVAPORATION EXCESSIVE IN WARMER HALF-YEAR. 3: EVAPORATION ONLY MODERATELY GREATER THAN PRECIPITATION. 4: EVAPORATION AND PRECIPITATION BOTH LARGE THROUGHOUT YEAR. 5: EVAPORATION MODERATELY EXCEEDS PRECIPITATION IN WARMER HALF-YEAR. 6: EVAPORATION GENERALLY LESS THAN ANNUAL PRECIPITATION.

tion. The Southwest, region 1, has excessive evaporation throughout the year. In most of the West, region 2, evaporation is excessive only during the warmer half-year. In region 3, evaporation normally is only moderately greater than precipitation. In the Southeast, region 4, both evaporation and precipitation are large throughout the year. In the Northeast and Northwest, region 6, precipitation generally exceeds evaporation except in the warmer weeks of summer. The typical soil type of region 4 is the yellow to red. Region 3 has black chernozem soil, regions 5 and 6 have characteristically gray-brown soils, except in the coolest areas, where podzols occur.

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SUNBURN PROTECTION, NATURAL AND ARTIFICIAL

By ARTHUR C. GIESE and JULIAN M. WELLS*

EXPERIENCE tells us that accommodation to sunlight is achieved by graded exposure to the actinic rays of the sun. Too rapid and extended exposure results in erythema, or intense reddening of the skin, which may be followed by blistering, desquamation, and tanning. In wide use at the present time are oils, lotions, and ointments which are supposed to prevent sunburn while allowing tanning to occur. Under certain circumstances such results are achieved. It is the purpose here to describe briefly, first, how natural accommodation occurs and, second, how sunburn protection is artificially obtained and how it enables the normal process of accommodation to occur at the same time.

In the popular mind the protection from sunburn is due to tanning, the pigment serving to absorb the actinic rays, and so preventing their penetration through the skin, as was suggested by Finsen (1901). While pigment may protect to some extent, the fact that individuals already tanned may be severely burned by exposure to the sun indicates that this is not the whole story. Furthermore, blonds, without developing pigment, may nonetheless gain some accommodation to the sun's rays. This seeming paradox is clarified by a histological study of the skin, which shows that the pigment resulting from sunburn is largely below that layer of cells mainly affected in sunburn and thus cannot afford much protection to these cells.

A cross section of the human skin is shown diagrammatically in Figure 1. Skin consists of two layers: epidermis and derma, differing in embryonic origin. The epidermis varies in thickness from .07 to .12 mm., except on palms and soles, where it is thickened. It is made up of many layers of cells, cylindrical at the base, dead, flattened, and cornified at the surface, and of intermediate shape between. The cylindrical cells at the very base of the epidermis are active cells, in which mitoses are frequent. They are overlaid by cells with a soft, granular appearance, know as the prickle cells because of their intercellular linkages. Above the prickle cell layer is the granular layer, the cells of which, in the thickened parts of the epidermis, are filled with granules of keratohyalin or eleidin. Above the granular layer are the flattened layer and the scaly layer, the latter at the surface of the skin. In very thick epidermis, as on the palm or sole, a clear layer called the *stratum lucidum* lies between the granular layer and the flattened layer.

The derma, which lies beneath the epidermis, consists of a cushion of connective tissue carrying the blood vessels that supply the derma. It has an average thickness of 1 to 2 mm., except on the palms and soles, where it is thicker. The epidermis has no blood vessels and must receive its supplies by diffusion from the derma. Below the derma is the fibrous connective tissue which binds the skin to the muscles or other structures. In the dark races pigment occurs in the derma as well as in the epidermis, and it is distributed through the latter, making it much more useful as a protection against sunburn than the pigment developed by the white race.

Histological study shows that sunburn

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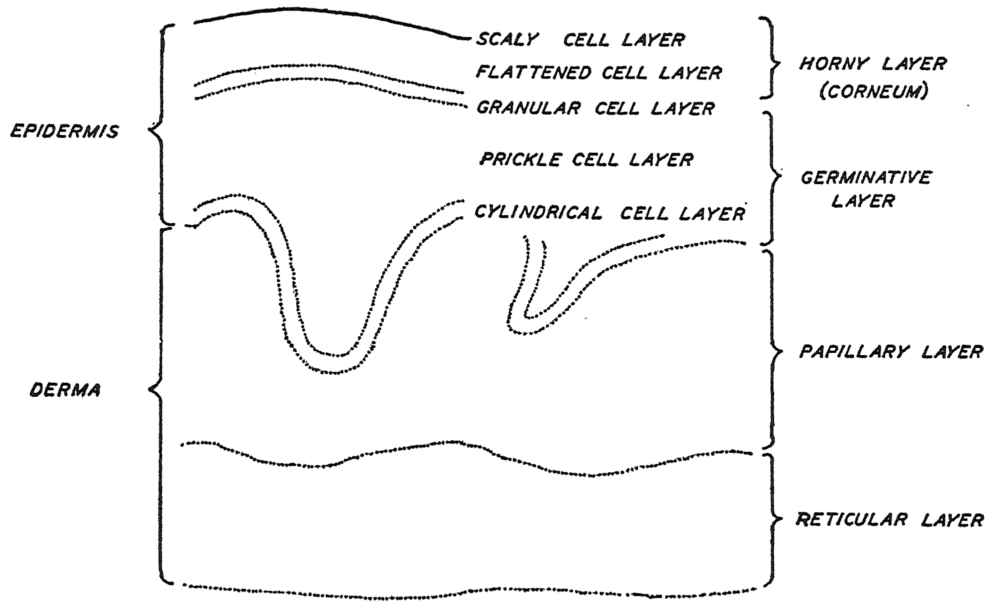


FIG. 1. DIAGRAM OF A SECTION OF HUMAN SKIN

SUNBURN, OR KILLING OF CELLS, OCCURS IN THE PRICKLE CELL LAYER; ERYTHEMA OCCURS IN THE DERMA, ESPECIALLY THE PAPILLARY LAYER, AS A RESULT OF THE DILATATION OF THE BLOOD VESSELS. PIGMENT DEVELOPS CHIEFLY IN THE CYLINDRICAL CELL LAYER (SEE MAXIMOW AND BLOOM, 1942).

injury is largely localized in the prickle cell layer. These cells, in absorbing the light and becoming injured, protect the active, or germinative, cylindrical cell layer below. The production of breakdown products on injury and death of some of the prickle cells is thought to result in the diffusion of chemicals, causing relaxation of the blood vessels of the derma and leading to erythema after a latent period. Considerable injury to the prickle cells is followed by infiltration with lymph, leading to blister formation. Recovery is brought about by the proliferation of a new cover of cells from the germinative epithelium and the shedding of the dead cells (desquamation). Tanning also follows the erythema, but the melanin pigment is laid down by melanoblasts in the basal layer of the epidermis. In this position it cannot protect the prickle cell layer from radiations. Only as the pigment granules later migrate outward in small quantity does the melanin serve to protect the prickle cells.

The thickening of the corneum of the skin subsequent to exposure to sunlight has been shown by Guillaume (1927) and Miescher (1930) to be the main mechanism by which the accommodation to sunlight occurs. While the corneum has little pigment or color, it absorbs strongly in the ultraviolet part of the spectrum. Furthermore, the granular inclusions in the cells, as well as the surfaces of the flattened cells, reflect and scatter the light effectively, preventing it from reaching the sensitive cells below. The greater the thickness of this absorbing and scattering layer of cells, the greater the protection to the prickle cell layer below. The soles of the feet and palms of the hands are not naturally burned, being protected by the thick layer of cornified epidermis; yet by excessive experimental dosages they could be burned.

Since erythema and tanning and thickening of the corneum seem to go hand in hand, all occurring consecutively after irradiation with ultraviolet light, how can we explain the apparent anomaly of sunburn in a tanned individual? The problem is not simple. First of all, the action spectrum, i.e., the relative effectiveness of different wave lengths, is not the same for erythema and tanning. These differences are shown in Figure 2. In both cases, it is true, the wave length of maximal efficiency is the same, 2967A (Coblentz and Stair, 1934; Luckiesh and Taylor, 1939). However, short wave lengths which have erythemal effectiveness do not induce much pigmentation, and wave lengths longer than those which cause sunburn may result in pigmentation. Pigment is formed in the basal layer of the epidermis, and the differential action of different wave lengths may be in part due to differential penetration. Therefore, it is possible that tanning might occur without such action of the radiations on the prickle cells as would evoke thickening of the corneum. Certainly this would be true of the "direct or immediate pigmentation" of Henschke (1943). This type of tanning is produced maximally by wave lengths between 3400–3500A and is presumably due to direct oxidation of the precursor of melanin. It is induced only by a dosage several hundred times as large as that at 2967A (Luckiesh and Taylor, 1939). It can be produced in dead skin; therefore it appears to be a photochemical change. It appears immediately after exposure, as shown in Figure 3, whereas the pigmentation following irradiation with shorter wave lengths appears only after the erythema begins to subside. Furthermore, exposure with large dosages at still longer wave lengths (3600–4800A) may produce erythema and tanning, but this erythema involves a

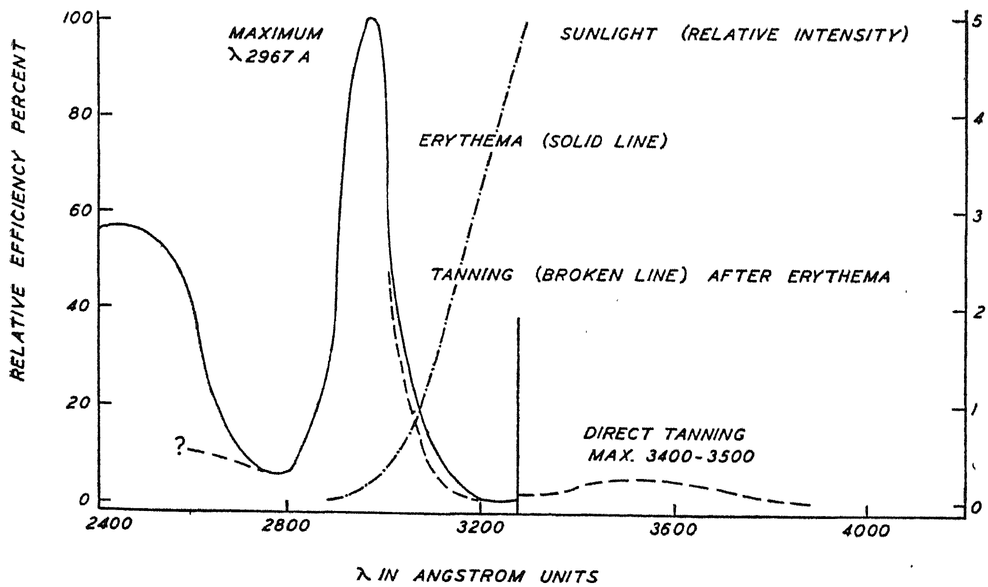
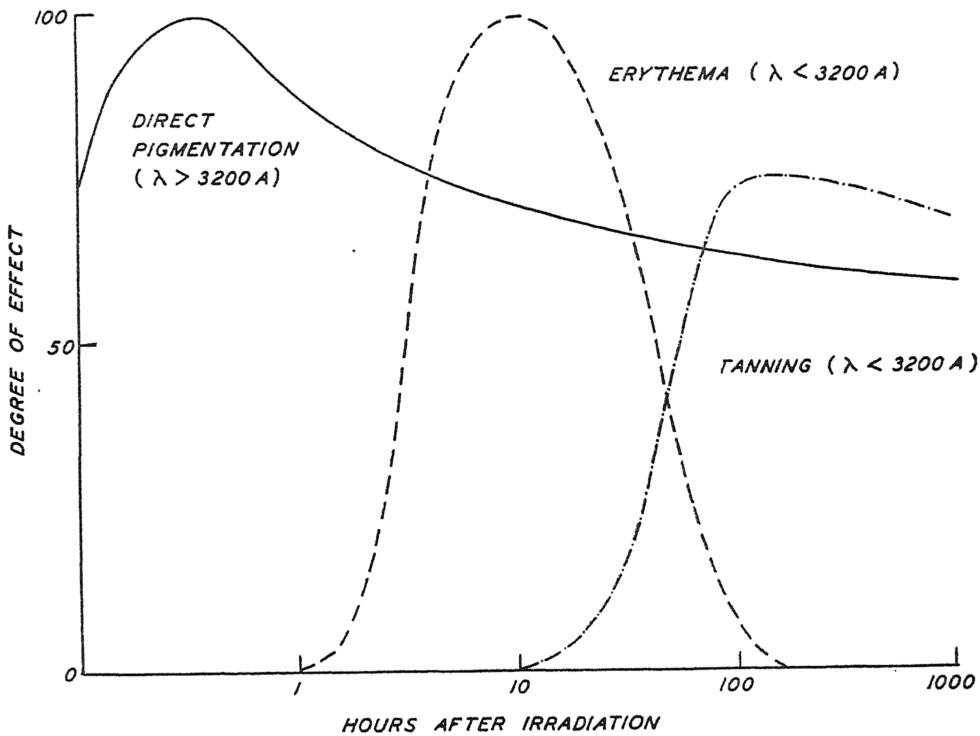


FIG. 2. ERYTHEMA AND TANNING CURVES

ERYTHEMA AFTER COBLENTZ AND STAIR (1934), TANNING AFTER LUCKIESH AND TAYLOR (1939) AND HENSCHKE (1940). THE QUESTION MARK ON THE TANNING CURVE AT SHORT WAVE LENGTHS INDICATES LACK OF ACCURATE INFORMATION. THE VERTICAL LINE INDICATES A CHANGE IN ORDINATE TO THE PERCENT SCALE ON THE RIGHT. FOR LONG WAVE LENGTH ERYTHEMA SEE HAUSSER (1938).



After Hamperl, Henschke, and Schulze (1939)

FIG. 3. TIME OF APPEARANCE OF ERYTHEMA AND PIGMENTATION

mechanism other than the destruction of the prickle cells. The wave length maxima at 3600, 3850, and 4080A—3850 the strongest—suggest hemin absorption. No increase in thickness of the corneum following action of long ultraviolet wave lengths appears to have been demonstrated (Hausser, 1938; Hamperl, Henschke, and Schulze, 1939).

The picture is further complicated by the fact that sex hormones are involved in tanning (Hamilton and Hubert, 1938). After castration only a pasty coloring develops on exposure to the sun. However, injection of testosterone in both sexes is followed by a healthy tanning of previously exposed skin. Estrone also seems to be involved in tanning of women (Hamilton, 1939).

Even if both tanning and increase in thickness of the corneum had occurred simultaneously, a more rapid loss of the corneal thickening than of the pigment might also result in a pigmented individual who did not have much resistance to ultraviolet light. In some individuals tested in our laboratory, pigment once formed tends to remain for years; in others it may disappear in a few weeks. The thickened epidermis subsequent to a single effective exposure lasts at best for about a month (Ellinger, 1941). As previously pointed out, pigment granules are formed extracellularly in the basal layer of the epidermis and are found to migrate into the outer layers of the corneum, from the surface of which they are ultimately shed. The amount of the pigment formed and the rate of shedding varies from individual to individual. Some regions of the body, such as the arms, lose their pigment much more rapidly than do other regions, such as the abdomen, of the

same individual. Apparently, even when no pigment can be detected, the change in the nature of the skin produced by ultraviolet radiations may be revealed by observing the skin under ultraviolet light of long wave lengths, in which the normal skin fluoresces more than the affected skin (Luckiesh and Taylor, 1939).

If prolonged exposure to sunlight or other ultraviolet sources is unavoidable, how can the skin be protected from injury? We can imitate nature by covering the epidermis with something that will both scatter and absorb the active rays of the sun, thus reducing the intensity of the rays reaching the skin to a level that can be tolerated. Materials must be chosen that will absorb or scatter the radiations of sunlight causing erythema and sunburn. These radiations are present in sunlight only to a limited extent—under the most optimal conditions for sunburn only 0.2 percent of the total radiation is erythemic. The relative efficiency of different wave lengths of the ultraviolet in producing erythema is shown in Figure 2 (Coblentz and Stair, 1934). From this graph it is evident that wave lengths not present in sunlight are also effective in producing erythema. The limits of the sunburn radiations in sunlight are also shown.

An ointment or lotion made to protect from sunburn usually contains a compound that scatters light effectively and one that absorbs the sunburn radiations, the latter usually being spoken of as a screen. Talc, kaolin, zinc oxide, chalk, magnesium oxide, and titanium dioxide have been used as light-scattering agents, but titanium dioxide is perhaps most suitable and has been widely employed. The screen is difficult to choose because of the many desirable properties it must possess and the large number of compounds that have been suggested but not yet fully tested. The screen should absorb the erythema-producing radiations very strongly, but it must be nonirritating to the skin. It must not be photolabile, i.e., readily destroyed by the absorption of sunburn ultraviolet, in order that it may remain potent for some time. It must be nontoxic, even if used repeatedly for long periods of time, and it must not be decomposed on the surface of the skin. A very large number of compounds absorb in the region of the erythemic radiations. Quite a few have probably been tried, but unfortunately data on most commercial ointments are not available; and in the case of foreign patented preparations, the formulae of the ointments, as well as the nature of the screens, are not disclosed.

A detailed list of chemical screens that have been tried or suggested would be out of place here, therefore only the main families of compounds need be mentioned as examples of the types of chemicals used: p-amino benzoates, anthranilates (o-amino benzoates), salicylates (o-hydroxybenzoates), cinnamates, pyrones (e.g., esculin), benzimidazoles, carbazoles, naphthol sulphonates, and quinine disulphate. Most of the compounds used are aromatic compounds, and the benzene rings and double bonds usually present are the chromophores which absorb the erythema-producing rays of sunlight. A fairly detailed list of compounds appears in deNavarre's book (1941), and others are to be found in *Chemical Abstracts* from about 1930 to the present, the greatest number appearing just before the war. The relation of the molecular configuration to absorption of ultraviolet is discussed in such books as Brode's *Chemical Spectroscopy*.

The scattering agent and the screen must be put into some vehicle—either an oil, an ointment, or a lotion. Oils generally contain a screen, or absorbing compound. Lotions and ointments generally have both scattering and absorb-

ing agents. The lotion is generally hydroalcoholic and dries rapidly, leaving a thin film of active material on the skin, bound in some film-forming compound such as ethyl cellulose. The ointments are either oil in water emulsions (vanishing creams) or fatty or greasy bases, usually anhydrous in nature. Generally lotions and ointment are made to be relatively easily washed off with soap and water (deNavarre, 1941, gives formulae of bases or vehicles). Pigments are usually added to match skin color, and preservatives to prevent the ointment from decomposing.

Ointments have been used in the armed forces whenever excessive exposure to sunlight has been encountered. Exposure to sunlight on life rafts is particularly severe, but it is also a problem in the desert and in the tropics. Ointments for different regions must have different properties. In general the ointments and lotions for use in the armed forces must have greater resistance against abrasion and sweating than those desired for civilian use.

No ointment, lotion, or oil protects the skin completely—it merely acts as a screen to reduce the intensity of the sun's ultraviolet. The effectiveness of the ointment may be tested in the following manner: The skin is covered with a thin, flexible lead plate containing perforations about .5 inch in diameter. Successive circles are exposed in a graded series to determine the minimal erythemal dosage (hereafter referred to as M.E.D.), or the dosage which causes a just perceptible reddening of the skin. It is important to use skin not previously exposed or skin evenly exposed. Once the M.E.D. has been determined, measured areas of the skin are coated with a known weight of the substance to be tested in order that the comparison may be made in a standard manner. The coated areas are then treated with a graded series of exposures to determine the increase in time of exposure required for a M.E.D. through the ointment or lotion. As a source of radiations a carbon arc or a quartz mercury arc with an appropriate filter such as corex D, to match the quality of sunlight, may be used. If possible, the same intensity should be employed in successive runs. If feasible, final tests should be made with sunlight, since even with a filter the artificial sources are quite different from sunlight.

Calculations show that even under the most extreme conditions no more than 20 M.E.D. units of erythemic radiations fall on a person on a given day (Blum, 1943a). Therefore, to confer complete protection under such extreme conditions, the ointment or lotion should reduce the intensity of the light to about one-twentieth. Some ointments will achieve such protection, but the ones we tried did so only in layers thicker than are practical. However, the exposure is usually likely to be less than 20 M.E.D. Ointments which in thin layers afford a protection of about 10 M.E.D., i.e., requiring an exposure of ten times the M.E.D. before an erythema appears, are available.

Under most conditions considerably less than this protection is required. However, commercial ointments and lotions ought to be graded as to their protective value, and the ingredients used ought to be listed. A convenient method of grading would be the ratio of the M.E.D. values with and without ointment. Ointments made with different bases, or vehicles, differ in their resistance to sweating, rubbing, and washing. Under given field conditions some ointments are rendered useless in a short time and must be reapplied. It is likely that an ointment which is satisfactory for exposure in the snow will not be satisfactory for the beach, the desert, or the mountains in the summer. No ointment should protect so completely

as to prevent some erythemic response after long exposure, since only with exposure is natural adaptation acquired.

Ointments and lotions are made to absorb the potent sunburn-producing radiations and to transmit the direct tanning radiations, since tanning is believed by the public to be not only aesthetically desirable but healthful. The absorbing screen is therefore carefully selected. Scattering agents with selective action over a narrow span of the spectrum are not available.

Sunburn can be a source of suffering, and almost every person seems to require personal initiation to it before he learns to avoid overexposure. Furthermore, continued exposure to sunlight or ultraviolet light from artificial sources may result in malignant tumors (Findlay, 1928). When continued and excessive exposure to intense sunlight is anticipated the individual should protect himself to avoid sunburn. When exposure is gradual and progressive the skin builds up its own protection, mainly in the corneum, or outer horny layer, which becomes thicker.

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SCIENCE ON THE MARCH

AN OLD CHEMICAL COMPOUND REVEALED AS AN EXCEEDINGLY POTENT INSECTICIDE

When Michael Faraday in 1825 treated benzene with chlorine and produced a compound with the comparatively simple chemical formula $C_6H_6Cl_6$, he did not anticipate that in 1946, more than a century later, this material would prove to be the most powerful insecticide known.

Currently referred to as "benzene hexachloride," but more accurately as 1,2,3,4,5,6-hexachlorocyclohexane, this compound is several times more toxic to most insects than the widely publicized DDT, which so effectively served to save thousands of human lives during the recent World War. As in the case of DDT, many years elapsed between the original compounding in chemical laboratories and the discovery of the amazing insecticidal potency of $C_6H_6Cl_6$.

Owing to the exigencies of war and the widespread successful use of DDT in clearing large areas—even entire islands—of disease-bearing pests, that chemical was given well-deserved publicity. The many reports emanating from battle areas too frequently referred to DDT as the "miracle insecticide," and the impression became general that this excellent insecticide is a perfect control for all types of insect pests under all conditions. There are, however, no such insecticides known to trained, experienced entomologists at the present time and scarcely a remote possibility of their future development. Insects are too variable in type, life history, feeding habits, and susceptibility to chemicals for the entomologist to hope for one uniform control measure for the thousands of destructive and dangerous pests.

In comparison with the long chain chemical formulas of pyrethrins I and II, which have been effectively used throughout the world as insecticides, $C_6H_6Cl_6$ is simple indeed. However, this apparently simple formula is complicated from the use standpoint by the existence of at least four recognized "isomers," indicated as *alpha, beta, gamma,* and *delta.* Strangely, the *gamma* isomer, which usually comprises only 10 to 12 percent of the total bulk, carries practically all the potency as an insecticide and, with the *delta,* is reported to make up approximately 25 percent of the total product, the remaining 75 percent being composed of *alpha* and *beta* and residues with little insecticidal value.

Fortunately, the *gamma* and *delta* isomers are soluble in varying percentages in more than 40 recognized chemical solvents, thus affording a ready means of segregating the portion having insecticidal value. This *gamma* isomer will be used extensively in aerosols, general flysprays, and in those operations requiring a purer product than the crude material, which has a peculiar musty, though not disagreeable, odor. The crude product, which is rather inexpensive considering its high insecticidal value, can be used in combination with various dust diluents such as clays, talc, etc., for applications to many field crops.

Preliminary tests of these dusts on cotton indicate great efficiency against cotton boll weevil, cotton flea hopper, sucking bugs, aphis, and thrips. Acting as a contact, stomach, and fumigant insecticide, $C_6H_6Cl_6$ appears in some phases of insect pest control to combine many of the values of arsenicals, nicotine, and rotenone and the several fumigants now in use.

English scientists report that only one part of *gamma* isomer to 1,000,000 parts of grain killed 100 percent of the common grain weevils (*Calendra*) under controlled fumigation conditions. And re-

cent tests in the United States indicate similar fumigation results on the same species of weevils in grain from the use of one part of the crude (containing approximately 10 percent *gamma*) in 100,000 parts of grain.

In the control of mosquitoes and flies under varied field conditions as well as in human habitations, the *gamma* isomer has shown amazing toxicity, with a killing power 8 or 9 times that of DDT and about 18 times that of pyrethrins. In field tests, including some in Western Africa and Ceylon, one-half pound per acre of the crude material in the form of dust produced 100 percent kill of mosquito larvae in 24 hours—an indication of the usefulness of benzene hexachloride in controlling these disease-bearing pests over large areas with applications made from airplanes. When incorporated in grasshopper baits the crude chemical indicates a toxicity approximately 10 times as great as sodium arsenite, according to reports from European authorities.

Benzene hexachloride apparently does not have the long residual effect in controlling houseflies and mosquitoes that is characteristic of DDT. The more rapid evaporation of benzene hexachloride is advantageous in that it will permit its general use in the spraying of fruit and vegetable crops without the danger of residues remaining which, in the case of certain insecticides, must be removed in order to meet Federal and State marketing and food regulations.

Many questions regarding the proper utilization of this new insecticide in many phases of crop pest control remain to be investigated; results of the rather scattering preliminary tests have made this entire problem most intriguing to economic entomologists. Several years will be required before fairly complete information regarding its possibilities in pest control will be available because of the various climatic and seasonal conditions under which our crop pests thrive and develop. Temperature and moisture often strongly influence the development of pests and the value of control methods. Many experiments are being planned by Federal and State entomologists for the approaching growing season to test the efficiency of the insecticide in various parts of the country.

The excellent and informative lecture by R. E. Slade, published and illustrated in *Chemistry and Industry,* October 13, 1945, should be carefully studied by everyone who proposes to carry on studies and experimental work with this promising insecticide. The article is replete with useful information on the chemical and physical properties of the compound. Reported results of early tests on a fairly wide range of injurious insects point to a broad field of usefulness in the control of insect pests of agricultural and horticultural products, domestic animals, and mankind.

Several brief reports by investigators in America have appeared in recent months, all confirming the exceptional insecticidal values of this long-neglected simple chemical compound. Entomologists everywhere sincerely hope that the public will make use of benzene hexachloride according to the recommendations of trained Federal and State entomologists.

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MILITARY GEOLOGY

World War II was an all-inclusive war. It made use of global terrain and most of the natural and manufactured resources of the entire world. Tides of battle were often influenced or altered by the nature of soils, waters, forests, and mineral resources. Beach and terrain configuration and composition obviously were of fundamental importance. It would require a long narrative to re-

count properly the value and use of these groups of natural resources, not only in the theaters of combat but also in the far-flung supporting hinterlands. Those accounts cannot be given here.

The conflict just ended was, as never before, a war of applied geology and of the use of mineral resources. One may not glean that impression from the recitals of invasions and of violent contests in the air, on land and sea, and under the sea, for those fundamentals were only a part of the primary background. However, the vital role of certain mineral resources—for example, coal, iron, and petroleum—in winning the war for the Allies and losing it for the Axis is rather generally known to scientists and to thoughtful laymen.

The accomplishments of "military geology," in close and constantly accelerated application to countless war problems involving world-wide terrain, beaches, soils, rock supplies, water resources, and mineral commodities, are largely contained in technical maps and reports prepared for governmental agencies and the armed forces. Much of the assembling and interpretation into military terms of terrain and other geologic intelligence for direct military use, at both strategic and tactical levels, was done by a highly organized special group set up for that precise purpose. That group was the Military Geology Unit of the U. S. Geological Survey.

In addition, scores of geologists and engineers with the necessary geologic training were literally scouring the earth for new supplies of critically needed raw mineral resources demanded by the multitude of industries charged with the duty of supplying the unprecedented, stupendous, and incredibly varied sinews of war to the armed services at home and around the world. They were using their special skills to ferret out and put into production the mineral deposits from Alaska to Argentina and from Australia to Africa. Many of those "treasure hunts" are thrilling tales aside from their contributions to the winning of the war. It would be difficult to overstate their value, for this was a global war based on an endless supply of many kinds of mineral resources.

In a more special sense, military geology dealt directly with the compilation of technical geologic data and their interpretation to officers of the Army, Navy, and Marine Corps. It must be borne in mind that few of the men charged with the grave responsibility of over-all and detailed planning had a working technical knowledge of geology. But they understood maps and charts of all kinds. Obviously, then, geologic information could be most useful if placed on maps which showed quickly, comprehensively, and as accurately as both data and time permitted, those field facts of military importance. Army GHQ came to depend upon these maps in planning campaigns and invasions. Field commanders found them indispensable in the execution of tactical maneuvers. In some instances, a large folio of maps was prepared and on its way within twenty-four hours after an urgent request had been received from a theater of operations.

The conventional topographic map in four colors showing all of the surface features of an area is most useful in war as well as in peace. But it is far more useful when supplemented by the geologic data to which the topography is intimately related. The character and structure of the "hard" rocks and the nature of the soils derived from them were of crucial importance in many places. The potential amount and character of surface and underground water supplies could be forecast, in the main, prior to an invasion; well sites could be selected so that combat troops would not lack an ample supply of water; large installations with the water requirements

of cities could be planned. The sites of airfields, a *sine qua non* of World War II, and the sources of stone for their construction could be selected well in advance by the application of geologic knowledge. Even the suitability of the terrain and the soils for the digging of foxholes could be predicted. The location of observation posts, of the most favorable routes for the movements of troops, and of protected advance lines could often be indicated. Disasters on invasion beaches were often minimized or prevented by the foreknowledge of surface features and materials that must have resulted from the working of geologic processes on the rocks in those areas. Striking examples were last-minute changes in the carefully prepared plans for the invasion of certain Pacific islands and the construction of airfields on them, as soon as terrain intelligence became available.

All these valuable data, and many more, were supplied by an extremely busy group of about 100 military geologists in secret quarters in Washington. Their efforts were constantly supplemented by the work of overseas teams assigned to field duty with combat units and by other geologists who were members of the armed forces and who were assigned to several specialized service units. Geologic work on the ground in the combat areas was particularly necessary where data pertaining to those areas were not available in advance of operations. Still other geologists throughout the nation and around the world were getting other essential information.

Maps depicting terrain intelligence were constantly needed. They included maps showing topography in perspective; terrain appreciation, that is, the appraisal of terrain features that affected the movement of ground forces; trafficability of areas; prospective airfield sites; water supplies; sources of construction materials; and all other geologic data of use to military forces.

How could this be done, even with unlimited time at the disposal of research geologists? And how, especially, could some herculean task be accomplished for all practical military purposes within the space of one day or, at most, several days? In ten days the Military Geology Unit prepared their maps for the invasion of Sicily! It could be done effectively because in most countries outside of the United States geologic maps of much of the country had been prepared in the normal course of peacetime surveying of the soils, mineral deposits, and other natural resources upon which national economy depends. As a rule, copies of those maps were available in libraries in this country. They were supplemented by many maps made by American and British geologists and engineers and by photographs taken by them, as well as by aerial photographs taken on the spot. In the words of a report summarizing this work:

> All of this underscores the main lesson of the war: that in preparing for the future—be it war or peace—American geologists can accomplish nothing more important than to complete areal mapping of geology and soils throughout our own country and in some places abroad.

Yet, the United States is only about 7 percent mapped!

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BOOK REVIEWS

BIOLOGICAL FIELD STATIONS

Biological Field Stations of the World. Homer A. Jack. Chronica Botanica, Vol. 9, No. 1. 73 pp. $2.50. The Chronica Botanica Company, Waltham, Mass.; G. E. Stechert & Co., New York. 1945.

THIS paper is a very welcome addition to the literature on an important aspect of education and research. Field stations seem destined to expand greatly as the application of scientific methods is extended to the biological and ecological resources of the world and as their utilization comes under intelligent supervision. The impulse given to science and its application by the war is unprecedented and should be utilized to make needed improvements. Such an advance will be in direct proportion as textbook teaching and urban laboratory methods are balanced with field studies of biology and ecology. Decentralization will facilitate this. There has been much lip service paid to the direct study of living nature, and much less actual practice. This is because it is more difficult to make the field studies on account of the inadequate facilities available for other kinds of study. Administrative reorganization is needed before changes can be made. We may look upon field stations as a social invention devised to assist teaching and research, which in general can be done better by an institution than by the isolated individual. Stability, continuity of policy, and coordination are the advantages of such an organization.

It would be a mistake to consider these biological field stations merely as specialized or technical institutions, and overlook the fundamental fact that their direct and objective method should be extended over much, if not most, of the educational system, from the bottom to the top. Nurseries, camps, and summer schools are needed at the elementary level, and corresponding camps, schools, field stations, excursions, and travel are likewise required at the intermediate and all higher levels, before these methods can be integrated and absorbed into the whole educational system. The natural history sciences and agriculture have done a great deal of the pioneering in this field, and others should now study their methods and apply them to other fields, particularly to the social sciences. The words of John Dewey on this general subject cannot be repeated too often:

> One of the only two articles that remain in my creed of life is that the future of our civilization depends upon the widening spread and deepening hold of the scientific habit of mind. . . . The other ''article'' is faith in democracy as a social mode of life.

About half of this paper on field stations is devoted to a discussion of the broad general aspects of the subject, including such phases as their purposes, history, administration, equipment, facilities, instruction, and research plans. This appears to be the most comprehensive discussion of their functions that has been produced. Every phase of their work is discussed briefly, including condensed references to the literature (so condensed, however, as to make them difficult to use in a private library).

This discussion deserves careful study, particularly by those educators who are seeking to improve present educational conditions. It is just these persons and those who have huge funds to make educational studies and surveys who need to give serious attention to the broader implications and applications. I have yet to see the first of such studies that has given any adequate recognition to this whole field and its bearing on educational methods in general. This without question is a critical period for making these new evaluations.

Every former student of Dr. C. O. Whitman will welcome the recognition of the role he has played in grasping and expressing the fundamental functions of these field stations and the conditions most favorable for their success. In 1902 (*Biol. Bull.*, **3**, p. 214) Whitman said:

> The biological laboratories of today in design, equipment, and staff, are almost exclusively limited to the study of *dead* material. Living organisms may find a place in small aquaria or vivaria, but they are reserved as a rule, not for study, but for fresh supplies of dead material. . . . These fundamental problems require, therefore, to be taken to the field, the pond, the sea, the island, where the forms selected for study can be kept under natural conditions, and where the work can be continued from year to year without interruption.

Later on he adds (p. 216):

> The functions of a biological farm are not summed up in experimentation. That old and true method of natural history—*observation*—must ever have a large share in the study of living things. Observation, experiment, and reflection are three in one. Together they are omnipotent; disjointed they become impotent fetishes. Biology of today, as we are beginning to realize, has not too much laboratory, but too little of living nature.

No one has better expressed these relations than Dr. W. K. Brooks, who said:

> To study life we must consider three things:
> *first*, the orderly sequence of external nature;
> *second*, the living organism and the changes which take place in it, and
> *third*, that continuous adjustment between the two sets of phenomena which constitutes life.

These statements were made before the active expansion of modern ecology with its insight in biological problems.

The remaining half of the paper is devoted to the 1939–1940 status (qualified by war conditions) of these stations, including a condensed descriptive directory of the field stations or laboratories arranged alphabetically in 59 political units, beginning with Alaska and ending with Yugoslavia. When available, a brief sketch is given of each station, and reference is made to official and other publications. The author has visited 79 of these in 18 countries out of the total of 271 listed.

The list appears to be reasonably complete, but one notices the absence of Hastings Natural History Reservation in California, the Roosevelt Wild Life Forest Experiment Station, with its 15,000 acres of Huntington Forest in the Adirondacks, the Edmund Niles Huyck Preserve near Albany, N. Y., and the Petuxent Wildlife Research Refuge, conducted by the United States Fish and Wildlife Service (1939) in Maryland.

This study is without question an important and useful contribution. The editor of Chronica Botanica, Dr. Frans Verdoorn, has expressed a willingness to publish a historical account of these stations which merits hearty commendation.

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PRINCIPLES AND PROBLEMS OF PUBLIC MEDICAL CARE

Public Medical Care. Franz Goldmann. 226 pp. $2.75. Columbia University Press. New York. 1945.

PUBLIC medical care has grown to be an immensely important facet of society. Its growth has been largely haphazard and therefore asymmetric. The first and major portion of Dr. Goldmann's book traces the development of public health services and public care of the sick. It presents a broad historical perspective upon which the second part, entitled "Directed Growth," is based. This second part deals with plans for hospitals, clinics, laboratories, and the like, with special emphasis upon centralized administration and some consideration of the economics involved. The book presents the development of public medical care as a social movement. It serves a useful function in presenting some of the deficiencies of existing public medical services, though certain fundamental inadequacies are completely ignored. That

there is need for critical analysis should be obvious. There can be no question but that there is ample room for improvement, but organization alone will not suffice. The book is written from the viewpoint of the social worker primarily concerned with institutions, not from the viewpoint of the physician concerned with individuals. It is notably lacking in appreciation of the truly American tradition of individual initiative. Its basic philosophy will not appeal to scientists.

The concept that medical care is a government function has many strenuous advocates today in addition to Goldmann. They fall chiefly into three groups:

(*1*) Those whose general inadequacy and immaturity (inability to carry responsibility) keeps them in the numerically, and therefore politically, important class of the "have-nots" in an economically competitive society;
(*2*) politicians, both elected and appointed, fostering paternalistic schemes to bolster their political power by offering something for nothing; and
(*3*) idealistic dreamers who escape reality by conjuring up an ideal state in which all responsibility rests upon the government.

The primary premise of these proponents of socialistic medicine is clearly stated in the opening sentence of the *Preface* of Goldmann's book: "Adequate medical care is a fundamental human right." This dogmatic assertion is of very questionable value. Health is a privilege and not a right. As a privilege it inevitably entails responsibility. To evade responsibility it is easy to transpose privilege into right. The primary responsibility for the maintenance of health in an adult must be accepted by that individual, or else all schemes for medical care will fail. No amount of medical service can "give" health, unless individual men and women take sensible care of themselves. We have schools for all, or nearly all; but schools cannot assure the development of good sense in all the pupils. Learning requires study; health maintenance also demands individual effort. To deny this is to retard the development of human capacities and encourage dependency.

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PROLEGOMENA TO AN INQUIRY INTO THE PROBLEMS OF MEDICAL CARE, II*

Government in Public Health. Harry S. Mustard. xx + 219 pp. $1.50. The Commonwealth Fund. New York. 1945.

AWARE of the unsatisfactory situation with reference to the purchase and distribution of medical care, the Council of the New York Academy of Medicine established in the winter of 1942 the Committee on Medicine and the Changing Order. The Committee began its work in February 1943, charged with the following duties:

(*1*) To explore the possibilities and to formulate methods of maintaining and improving standards of quality in medical service, including medical research, medical education, the maintenance of health, both physical and mental, the prevention of disease, and the treatment of disease.
(*2*) To study the means of making available to larger groups of people and to the country as a whole the best-known practice in preventive and curative medicine.
(*3*) To explore the possibilities and to formulate proposals of distributing these services not only to a larger number but also at a lower per capita cost than the present system permits.

The Committee, recognizing the need for objective data, early enlisted the cooperation of a number of experts in the preparation of a series of monographs on the reciprocal effects of medicine and the technological, social, economic, and political changes that have taken place in American life. The Committee believed that such monographs will offer

* Monograph I reviewed in THE SCIENTIFIC MONTHLY, 60: 319–320 (April 1945).

not only a survey of the present situation, but will also indicate its evolution and possible future trends.

In addition to the publication of the monographs, the Committee intends to issue a report presenting any conclusions that might be drawn from its deliberations and studies.

The present monograph by Dr. Mustard is the second of the series to appear. The author set himself the multifaceted task of investigating the genesis, growth, and future of the public health movement in the United States with reference particularly to various political and social forces. The book contains six sections: Certain Preliminary Considerations, Federal Health Services, State Health Departments, Local Health Departments, Activities of Government in a Public Health Program, and a Summary of Trends and a Consideration of Certain Needs. For supporting evidence there is included a set of well-chosen and informative tables showing, among other things, the total and per capita annual expenditures for health activities by state and territory, according to various important categories. Two historical documents are carried by an Appendix.

The first section is devoted to groundwork material with emphasis on the biological and social factors in health and disease, the character of public health problems, factors that express the seriousness of a public health program, aesthetics and public health, relationships between official and voluntary health agencies, professions that participate in public health work, education for public health work, and government in its relation to public health work.

The longest section, Federal Health Services, refers to "unwise separations" and "unnecessary overlapping of functions" at the Federal level and gives a political history of the various agencies participating in health activities. Activity interrelationships are pointed out. The material is well documented; the reader interested in the political aspects of the Federal health movement will find the references sufficiently complete to guide him in further study.

In the section on the state health department the author indicates that the situation with reference to the state level is to some extent static when contrasted with the Federal level where "great forces are stirring," and from which "new pressures and resources may come." The greatest public health power, according to Mustard, resides in the state health department.

In the analysis of the development of the state health department, attention is drawn to two significant facts. First, in the establishment of the Constitution, the states did not cede to the Federal Government the responsibility and authority for the preservation of health within their borders; and, second, local action, so far as public health measures are concerned, preceded state-wide programs. State-wide action appears to have been precipitated in most instances by the recognition of an epidemic.

Attention is directed to the health work in the state carried by agencies other than the health department. This work, according to the author, while important, is in many instances on the periphery of the public health program.

In his discussion of expenditures by the state health department, Mustard refers to the matter of Federal grants-in-aid for health purposes. He points out two sociological problems that are involved, the soundness of the general principle of Federal grants, and the effect of such grants upon the appropriating bodies and the state health officers.

In the author's presentation of his material on local health departments, which includes origins of local health services, types of local health organizations, basic activities in local health programs, and costs of local health service,

he reminds the reader that "there are comparatively few local jurisdictions, outside the cities, that can alone support an adequate health program." Reference is also made to the author's conviction that if the belief is regarded as "old-fashioned" that people and local government ought to pay a part, at least, of the costs of benefits received, "then there could be no objection to direct rendition of all local health service by the state provided that (a) it is adequate in amount and (b) it is a part of a continuing, coordinated, balanced, and effective program."

In the section devoted to activities of government in a public health program reference is made to vital statistics, sanitation, acute communicable diseases, tuberculosis, venereal diseases, laboratory services, nutrition, hygiene of maternity and young childhood, school hygiene, industrial hygiene, health education, public health nursing, dental problems, mental hygiene, cancer, and heart disease.

The last section of fifteen pages presenting "A Summary of Trends and a Consideration of Certain Needs" is of especial interest. Among the trends referred to are two which are considered by the author to be particularly strong, the trend toward a more powerful Federal Government and the trend toward a more socialistic Federal Government. The paramount need, according to Mustard, is an adequate health service for citizens of every community.

The known deterrents to the establishment of a plan for the providing of adequate local health service are listed. In the opinion of the author these difficulties, while real, may be overcome. There are three requisites involved:

(*1*) The granting to a state of Federal subsidies for health work only if that state submits an over-all plan which will insure effective local health service in each of its local jurisdictions;

(*2*) mandatory state legislation requiring each unit of local government to participate financially in providing its citizens with an effective local health service; and

(*3*) state legislation, when indicated, that would require combined administration of the public health activities of local units of government so that service might be performed on an economical basis and with reasonable completeness.

Dr. Mustard has performed a difficult task extraordinarily well. As scientifically as it is possible in an inquiry of this kind he points out the good and the bad and makes practical suggestions for future developments based on his findings and his wealth of experience.

The book is adequately indexed. The typography and binding are uniform with the first monograph of the series.

This timely, well-documented, interestingly written, and thought-provoking book is recommended to public health workers, legislators, and all others interested not only in the specific problem of the provision of adequate health services to the citizens of this country, but also in the genesis, growth, and future of the public health movement in the United States.

W. M. Gafafer

Washington, D. C.

INSECTS AND THEIR FOOD

Insect Dietary. Charles T. Brues. 466 pp. $6.00. Harvard University Press. Cambridge. 1946.

The diversity of insects is so great that only an experienced entomologist of vast erudition and literary talent could succeed in writing a readable account of insects in relation to their food. Professor C. T. Brues has accomplished this feat at the culmination of his distinguished career in entomology. Primarily a naturalist, he is noted for his broad interest in insects. Whereas most entomologists nowadays are specialists, some even devoting years to the study of the control of a single species, Professor Brues has roamed over the whole field.

Taxonomy, morphology, phylogeny, physiology, pathology, paleontology, and the biology of insects in the field have engaged his attention. He has refused, however, to become involved in the chemical control of insects and is not regarded as an economic entomologist. Furthermore, he has not tried to become a modern experimentalist, using the new techniques and equipment of statistics, physics, and chemistry. The reader of *Insect Dietary* will find that Professor Brues is a natural philosopher—a man who has gathered innumerable facts about insects and has fitted them together to give them meaning.

Insect Dietary can be read with interest by anyone who has studied general entomology, but it will be particularly useful to graduate students of entomology, both for its stimulation to research and for its facts and interpretations, which may be called for in a general examination. And in spite of the multitude of facts contained in it, the reading of it will not be tiresome because Professor Brues writes smoothly and often interpolates quizzical comments on the human scene. For example:

> A complete survey of the materials which furnish food for insects would involve a compilation of stupendous proportions, . . . suited only for a Federal Aid or Economy Program.
>
> This competition [between plants and insects] is really a swinging or fluctuating relationship which developed over the course of countless years, long before our ancestors conceived the Utopian dream of making the world over purely for human consumption.
>
> [Insects] see best by ultraviolet light and poorly by the longer wave lengths that enable us to appreciate the beauties of a tropical sunset or to halt at a traffic light.

Each of the ten chapters opens with an appropriate quotation, sometimes taken from an ancient tome, and closes with an extensive bibliography. The latter adds up to 193 pages in the whole book. Relatively few references to the bibliography are made in the text, and footnotes are rare; thus readability is enhanced.

The first two chapters set the scene, impressing the reader with the abundance and diversity of insects and the general types of their food habits. Professor Brues recognizes four types: herbivorous, carnivorous (predatory), saprophagous, and parasitic. He adds, "We might make a more elaborate classification of food habits but this would add confusion rather than clarity, for whatever types or definitions we may select, there always remains a residuum of forms which do not fall decisively into any one group."

The next three chapters deal with the vegetarians among insects: the herbivorous forms, the gall insects, and those whose true foods are fungi or microorganisms.

The last five chapters are on the carnivores: predators, external and internal parasites, and on insects as food for man and other organisms.

Insect Dietary is pleasing in appearance as well as in content. It is illustrated with 68 line drawings of insects and their parts. These illustrations, derived from various sources, were redrawn by competent artists to give a desirable uniformity of size and line. The 22 plates are excellent half tones, most of them from photographs taken by Professor Brues. The book ends with separate author and subject indexes.

To C. T. Brues and his wife, to whom he dedicated his book, I say "well done!"

F. L. CAMPBELL

COMMENTS AND CRITICISMS

Pro-Haber

Dr. Haber's "Basic English for Science" in the last SCIENTIFIC MONTHLY is excellent and deserves wide attention. It should be required reading for every writer who has ever been guilty of propagating scientific gobbledegook.

While the use of "person interested in the earth's history" is a bit roundabout as an equivalent for "geologist," I feel that many of the readers of SM will heartily welcome the thesis put forward by Haber.—IRA M. FREEMAN.

Haber, Schmidt, and Johnson

Referring to your statement that you would like to know what we readers think of Dr. Haber's article, I want to say that it is of a very high order, and just the sort of article which I should think SCIENTIFIC MONTHLY readers would enjoy.

Also, the article by Mr. Schmidt is "excellent," and he sets forth what a naturalist *is* more clearly than any other American writer.

You did not ask me what I thought of putting anything by Owen Johnson, B.A., in THE SCIENTIFIC MONTHLY but I am going to volunteer the remark that it is "bad medicine," for SM should not be a receptacle for hack writing.—WILLIAM PROCTER.

Gilding the Lily

THE SCIENTIFIC MONTHLY reached an all-time low when its editor stooped to publish an anonymous and libelous letter purloined from a freshman girl's notebook, even though its contents are approved by the author of the paper on "Basic English for Science." Tom Haber did not take the trouble to verify the alleged quotation from our textbook nor the source of the young lady's statistics. Mr. Haber labors under the impression that students in our classes study books rather than plants and plant processes. If he had examined the textbook he would have discovered that the book was written to supplement what is learned in the classroom. "Nonymous" had no need to suffer mental anguish over the word "primordium" since she had seen primordia and examined their structure before she had any occasion to read the chapter on leaves. Furthermore, where this word is first used it is defined and illustrated. Mr. Haber's general characterization of science teaching and science texts makes one wonder whether his competence as a critic is based on investigation, or on intuition. To exemplify the need for Basic English where could he have found an author that needs "translating" less than Sir Charles Lyell? This is indeed painting the lily!

If this process of denaturing literature is so important why does not your author apply it to the writings of Shakespeare, Milton, or Carlyle which must be read by junior college students?

I am writing this letter to you because it seems to me that if your selected authors lack judgment as to what is fair, honest, and accurate you will have to be held responsible for their statements.—E. N. TRANSEAU.

Conservation of Naturalists

I would like to compliment Karl P. Schmidt on the excellent article "Naturalists for the Foreign Service" in your valued publication THE SCIENTIFIC MONTHLY of March 1946.

I am quite sure that all of the good people who are truly interested in the conservation of the natural beauties and natural resources of various countries, and not those of us that are only interested in the mere exploitation of these places, would and should welcome any means, such as has been suggested by Mr. Schmidt, in the furtherance of that good will, by the aid of our naturalists in their midst.—HENRY B. CHASE, JR.

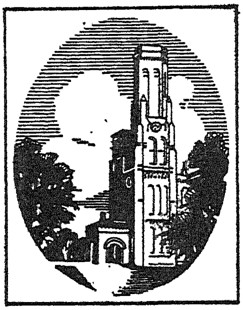
Scientific Diplomacy

The article "Naturalists for the Foreign Service" by Karl P. Schmidt in your March 1946 issue is a notable one, as it opens the possibility of utilizing an unused source for a foreign service personnel. Those of us who have carried on scientific exploration and field work in the Latin-American countries, as well as in other parts of the world, are keenly alive to the facts pointed out by Mr. Schmidt—that the nationals of other countries react in the most favorable manner to intelligent observers.

Naturalists, as a rule, have the special facility of understanding combined with the spirit of inquiry. It might be difficult to obtain a large number of people who are fitted both to handle foreign service problems and to carry on scientific activities. But there are some and they could do outstanding work.

In addition to following the suggestion of Mr. Schmidt, it would also be very wise for the Division of Cultural Cooperation of the State Department to explore the use of Scientific Attachés of the various Embassies—something for which there is a great need.—W. STEPHEN THOMAS.

THE BROWNSTONE TOWER



Although the view from the Brownstone Tower is extensive, I felt the need of a wider and different view of places and people. Fortunately I was given the opportunity to extend my horizon by flying to and from the meetings in St. Louis.

I shall not attempt here to report on the meetings, for one person can see only a little of so large a gathering, but perhaps I can record some impressions for those who could not go.

As the Boston meetings of 1933 are remembered as the frigid meetings, so the recent St. Louis gathering may be recalled as the shirt-sleeve meetings. Summer was treading on the heels of spring and driving out blossoms, leaves, and perspiration. The symbol of the meetings was the park bench on which I saw a man sleeping as I walked one morning from my hotel to the Auditorium. No doubt the sleeper was not a scientist, but many a scientist who had the fortitude to go to St. Louis without a hotel reservation must have wondered whether he would be forced to sleep on a park bench. So far as I know, everyone who came found a bed or cot under a roof, though the attendance was much larger than expected. Those who had rooms shared them with others, and the Association's Housing Bureau placed many visitors in private homes.

Having solved the housing problem, the visitor was free to sample the innumerable attractions of the meetings. On the first evening the large Opera House in the Kiel Memorial Auditorium was filled for the General Meeting of the Association. Following the adoption of the new constitution of the Association, the spotlight fell on that grand old warrior, A. J. Carlson, as he delivered his presidential address. Reading without glasses, he stood at the rostrum in a sack suit, giving the world a piece of his honest mind. More formally dressed ladies and gentlemen of distinction sat on the stage behind Dr. Carlson. Serving as hosts for the Association, they formed a receiving line at a reception that followed the General Meeting. In the same hall on the next night the biologists staged their annual mob scene. Clogging the entire room, they circulated by a sort of Brownian movement, literally bumping into acquaintances and emitting vast volumes of sound and smoke. After such confusion it was good to retire to the quiet of a hotel room.

I prefer the less frantic and friendlier gathering of specialists at a society banquet with good talk among friends around a table. After the dessert comes a tinkle of glass at the speakers' table and the performance begins. If the toastmaster is brilliant and the principal speaker entertaining so much the better, but regardless of the quality of their wit and wisdom the evening passes pleasantly because they are well-known men whom the audience is glad to see in a place of honor.

It is still better to attend a scientific meeting in one's own specialty, particularly if the meeting is not a dull parade of ten-minute papers but a planned program on subjects of current interest. With good speakers and a keen leader such a meeting creates informal discussion and brings out both the known and the unknown.

But best of all is the thrill of coming upon an old friend in the lobby, to sit down with him in a corner, or, better, to detach him from the crowd altogether and retire to a hotel room for a long, uninterrupted talk.

At the St. Louis meetings I moved among masses, crowds, groups, and individuals. I attended big meetings and little meetings, large dinners and small dinners, Council meetings and informal meetings, but of them all give me an individual meeting—the happy smile and the warm handclasp of an old friend.—F. L. CAMPBELL.

THE SCIENTIFIC MONTHLY

JUNE 1946

THE BIOLOGICAL BASIS OF IMAGINATION*

By R. W. GERARD
DEPARTMENT OF PHYSIOLOGY, THE UNIVERSITY OF CHICAGO

A SATISFACTORY interpretation of imaginative phenomena in terms of neural mechanisms may be presented by some fortunate author at a future time. But even now there is still much of substance to be said. Knowledge normally grows by such progressive steps as clarifying and isolating a problem, identifying the variables relevant to it, and following their correlations. Only later, often much later, does the nature of the basic entities begin to become manifest and does it become possible to grapple with them.

In the field of heredity, for example, Mendel isolated the problem in terms of simple characters and followed their behavior during inheritance. These results suggested separable inherited units, which remained as hypothetical for half a century as were the atoms of Democritus for nearly two millenia. Then chromosomes were seen; in another half-century the genes became visible; and studies are now proceeding in terms of the chemical properties of specific substances. In dealing with imagination it will be profitable similarly to examine its common meaning, to consider how psychological study has defined and measured relevant mental abilities, to note the relation of local brain damage to these abilities, and to develop the relation of these psychological phenomena to neural mechanisms.

WHAT IS IMAGINATION?

Imagination is more than bringing images into consciousness; that is imagery or at most hallucination. Imagination, creative imagination, is an action of the mind that produces a new idea or insight. "Out of chaos the imagination frames a thing of beauty" (Lowes's *The Road to Xanadu*) or of truth. The thing comes unheralded, as a flash, full-formed. We have all had this experience, and famous or important cases abound.

Kekule solved the chemical problem of the benzene molecule, a ring rather than a chain of carbon atoms, when in a fatigue- (or alcohol-) engendered daydream he saw a snake swallow its tail. Michelson's "intuition" gave him the equation for some complicated tidal phenomena, and when an expert mathematician reported a different result from his calculations, Michelson sent him away to find, as he did, an error.

Otto Loewi, recently awarded the Nobel prize for proving that active chemicals are involved in the action of nerves, once told me the story of his discovery. His experiments on the control of a beating frog heart were giving puzzling results.

* Based on a lecture in the series "Mathematics and the Imagination" in the Winter of 1945 at The University of Chicago.

He worried over these, slept fitfully and, lying wakeful one night, saw a wild possibility and the experiment which would test it. He scribbled some notes and slept peacefully till morning. The next day was agony—he could not read the scrawl nor recall the solution, though remembering that he had had it. That night was even worse until at three in the morning lightning flashed again. He took no chances this time but went to the laboratory at once and started his experiment.

Others have left brief or searching records of their experiences with ideas. Darwin tells us, in a bit of autobiography, of the resolution of an amorphous mass of facts into a crystallized hypothesis as to the origin of species: "I can remember the very spot in the road, whilst in my carriage, when to my joy the solution occurred to me." And Goethe relates how his mind, supersaturated with still unorganized material, responded to the shocking news of a friend's suicide. "At that instant the plan of 'Werther' was found; the whole shot together from all directions, and became a solid mass, as water in a vase, which is just at the freezing point, is changed by the slightest concussion into ice." Hamilton tells that his famous equations . . .

> started into life, or light, full-grown, on the 16th of October, 1843, as I was walking with Lady Hamilton to Dublin, and came up to Brougham Bridge . . . the fundamental equations . . . exactly such as I have used them ever since . . . I felt a problem to have been at that moment solved, an intellectual want relieved, which had haunted me for at least fifteen years before.

Coleridge composed "The Ancient Mariner" in four months, after years of brooding on a mass of travel lore and related material; and his own statement on the manner in which "Kubla Khan" gushed forth is worth another repetition:

> In consequence of a slight indisposition, an anodyne [morphine] had been prescribed, from the effects of which [the author] fell asleep in his chair at the moment that he was reading the following sentence, or words of the same substance, in "Purchas's Pilgrimage": "Here the Khan Kubla commanded a palace to be built, and a stately garden thereunto. And thus ten miles of fertile ground were inclosed with a wall." The Author continued for about three hours in a profound sleep, at least of the external senses, during which time he has the most vivid confidence, that he could not have composed less than from two to three hundred lines; if that indeed can be called composition in which all the images rose up before him as *things*, with a parallel production of the correspondent expressions, without any sensation or consciousness of effort. On awaking he appeared to himself to have a distinct recollection of the whole, and taking his pen, ink, and paper, instantly and eagerly wrote down the lines that are here preserved. At this moment he was unfortunately called out by a person on business from Porlock, and detained by him above an hour, and on his return to his room, found, to his no small surprise and mortification, that though he still retained some vague and dim recollection of the general purport of the vision, yet, with the exception of some eight or ten scattered lines and images, all the rest had passed away like the images on the surface of a stream into which a stone has been cast, but, alas! without the after restoration of the latter!

Imagination, not reason, creates the novel. It is to social inheritance what mutation is to biological inheritance; it accounts for the arrival of the fittest. Reason or logic, applied when judgment indicates that the new is promising, acts like natural selection to pan the gold grains from the sand and insure the survival of the fittest. Imagination supplies the premises and asks the questions from which reason grinds out the conclusions as a calculating machine supplies answers. Wood's story of how a plausible answer to a perplexing problem came to him while dozing, only to be later exploded by his experiments, is illustrative. Dryden, presenting *The Rival Ladies* to the Earl of Orrery, said:

> This worthless Present was design'd you, long before it was a Play; when it only was a confus'd Mass of Thoughts, tumbling over one another in the Dark: When the Fancy was yet in its first Work, moving the Sleeping Images

of things towards the Light, there to be distinguish'd, and then either chosen or rejected by the Judgment.

And Coleridge's artistry has compacted the matter into the phrase, "the streamy nature of association, which thinking curbs and rudders."[1]

Some footnotes are in order. First, many have insisted that the imaginative process is different in art and in science. I see no basis for such a position. On the contrary, the creative act of the mind is alike in both cases, as the above examples and later considerations fully evidence. Rather, the criteria for sifting may differ. Both art and science demand meaningful relations; but the one is satisfied more by pleasing structure, the other by logical validity.

Second, it deserves mention that imagination re-enters at all stages of intellectual endeavor, it does not merely deliver a mental foundling to the care of other faculties of mind. In science, as an example, imagination enters into the devising of experiments or of apparatus or of mathematical manipulations and into the interpretation of the results so obtained. But these are likely to be minor miracles compared with the major insight achieved in the initial working hypothesis.

A third point: imagination is not encompassed in reason. True, reason gives "the truths no mind is free to reject," and logic is an index, through function, of how the brain machine is constructed. But logic can never reveal all the laws of thought, as George Boole hoped. For, in mathematics or symbolic logic, reason can crank out the answer from the symboled equations—even a calculating machine can often do so—but it cannot alone set up the equations. Imagination resides in the words which define and connect the symbols—subtract them from the most aridly rigorous mathematical treatise and all meaning vanishes. Was it Eddington who said that we once thought if we understood 1 we understood 2, for 1 and 1 are 2, but we have since found we must learn a deal more about "and"?

A final interpolation concerns imagination and knowledge. Imagination is one manifestation or index of how the brain machine works, which in turn depends on how it is built. And since sensory data are shaped by such reworking, imagination pervades all thought and knowledge. This is far from saying, as some do, that imagination offers a separate avenue to truth or reality, one alternative to sensation and depending on some act of spiritual apprehension or revelation or of ancestral or racial insight. Leonardo well said, "All knowledge is vain and erroneous excepting that brought into the world by sense perception, the mother of all certainty." What is denied our senses (or their instrumental extension) and what escapes through the meshes of "the a priori net of the mind," in Eddington's phrase, is lost to us. On the other hand, since the properties of nerve fibers and nerve cells clearly determine the character of sensation and, only less clearly with present knowledge, determine the character of imagination and reason, and since these last are called into action directly or indirectly by sensory nerve impulses set up by receptors which probe the surroundings, it is not surprising that sensing and thinking do jibe with each other and have some de-

[1] The mathematician John von Neuman has recently told me of a striking self-observation which indicates that even the curbing may be unconscious. The steps of a proof commonly enter his consciousness as a linked chain of steps, obviously in proper sequence in the unconscious but dragged into awareness (as if from memory) in haphazard fashion. When the whole has emerged and he finally writes it as an article, he occasionally develops a strong distaste for continuing this chore beyond some point. Experience has shown him that such a block almost always results in his discovering a previously unrecognized error at that step where the unreasoned blocking occurred.

gree of valid correspondence with a real universe.

Now, returning to the attributes of imagination, since its product enters consciousness abruptly, its workings are at the unconscious or uncritical level. As Lowes puts it,

> [Coleridge's notebook] gives us some inkling of the vast, diffused, and amorphous nebula out of which, like asteroids, the poems leaped. It makes possible at least a divination of that thronging and shadowy mid-region of consciousness which is the womb of the creative energy. For it is the total content of a poet's mind, which never gets itself completely expressed, and never can, that suffuses and colours everything which flashes or struggles into utterance. Every expression of an artist is merely a focal point of the surging chaos of the unexpressed.

Simple imagination is observable in a pure and untrammeled state in dreams, in the hallucinations of drugs and other agents, in those hypnagogic states which interpose between wake and sleep or in the slightly-fettered daydreaming while awake, in the free fancies of the child and the less free fancies of the amateur. For ideas, like mutations, are mostly bad by the criteria of judgment, and experience or expertness suppresses them—unless imaginings get out of hand and displace reality, as in the insanities. But the imaginative hopper is fed from and feeds back to the conscious and critical level. There the heat of mental work transforms the soft ingots of fancy into the hard steel of finished creations. Baudelaire refers to "the labor by which a revery becomes a work of art," and Mary Boole has likened the alternate conscious and unconscious digestion of a problem to the rumination of a cow—as indeed our language does in using "rumination" for a loose form of mental activity. I may also recall the famous descriptions of their own creative processes by two outstanding thinkers and a great composer. Claude Bernard says:

> Apropos of a given observation, no rules can be given for bringing to birth in the brain a correct and fertile idea that may be a sort of intuitive anticipation of successful research. The idea once set forth, we can only explain how to submit it to the definite precepts and precise rules of logic from which no experimenter may depart; but its appearance is wholly spontaneous, and its nature is wholly individual. A particular feeling, a *quid proprium* constitutes the originality, the inventiveness, or the genius of each man. A new idea appears as a new or unexpected relation which the mind perceives among things. All intellects doubtless resemble each other, and in all men similar ideas may arise in the presence of certain simple relations between things, which everyone can grasp. But like the senses, intellects do not all have the same power or the same acuteness; and subtle and delicate relations exist which can be felt, grasped and unveiled only by minds more perceptive, better endowed, or placed in intellectual surroundings which predispose them favorably.

Mozart writes to a friend:

> What, you ask, is my method in writing and elaborating my large and lumbering things? I can in fact say nothing more about it than this: I do not myself know and can never find out. When I am in particularly good condition, perhaps riding in a carriage, or in a walk after a good meal, and in a sleepless night, then the thoughts come to me in a rush, and best of all. Whence and how—that I do not know and cannot learn. Those which please me I retain in my head, and hum them perhaps also to myself—at least so others have told me. If I stick to it, there soon come one after another useful crumbs for the pie, according to counterpoint, harmony of the different instruments, &c., &c. That now inflames my soul, namely, if I am not disturbed. Then it goes on growing, and I keep on expanding it and making it more distinct, and the thing, however long it be, becomes indeed almost finished in my head, so that I afterwards survey it at a glance, like a goodly picture or handsome man, and in my imagination do not hear it at all in succession, as it afterwards must be heard, but as a simultaneous whole. That is indeed a feast! All the finding and making only goes on in me as in a very vivid dream. But the rehearsal—all together, that is best of all. What now has thus come into being in this way, that I do not easily forget again, and it is perhaps the best gift which the Lord God has given me. When now I afterwards come to write it down, I take out of the sack of my brain what has been previously garnered in the aforesaid manner. Accordingly it gets pretty quickly onto paper; for, as has been said, it is properly speaking

already finished; and will, moreover, also be seldom very different from what it was previously in the head. Accordingly I may be disturbed in writing, and even all sorts of things may go on around me, still I go on writing; even also chatting at the same time, namely, of hens and geese, or of Dolly and Joan, &c.

And Henri Poincaré makes a superb summary:

> This unconscious work . . . is not possible, or in any case not fruitful, unless it is first preceded and then followed by a period of conscious work. . . .
>
> It is certain that the combinations which present themselves to the mind in a kind of sudden illumination after a somewhat prolonged period of unconscious work are generally useful and fruitful combinations, which appear to be the result of a preliminary sifting. . . . This, too, is most mysterious. How can we explain the fact that, of the thousand products of our unconscious activity, some are invited to cross the threshold, while others remain outside? Is it mere chance that gives them this privilege? Evidently not. . . .
>
> All that we can hope from these inspirations, which are the fruits of unconscious work, is to obtain points of departure for [our] calculations. As for the calculations themselves, they must be made in the second period of conscious work which follows the inspiration. . . . They demand discipline, attention, will, and consequently consciousness. In the subliminal ego, on the contrary, there reigns what I would call liberty, if one could give this name to the mere absence of discipline and to disorder born of chance. Only, this very disorder permits of unexpected couplings.

Clearly, then, pursuit of imagination leads us into the unconscious and its mechanisms. Nor is this any longer a completely uncharted wilderness, for psychoanalysis especially has even now developed a usable body of knowledge to guide the explorer. It has recognized and isolated such unconscious mechanisms as condensation, displacement, projection, and identification—as well as repression, sublimation, substitution, rejection, denial, introjection, suppression, and conversion, to extend the list—which often enable the student not only to see further into the how of imagining but even to account for what is imagined. This is true for the normal and perhaps more strikingly for the disturbed; the previously meaningless chatter of the schizophrenic patient, for example, is quite intelligible in terms of known dynamics. Condensation and identification, respectively, are clearly revealed in the following statements by Coleridge concerning himself: "Ideas and images exist in the twilight realms of consciousness, that shadowy half-being, that state of nascent existence in the twilight of imagination and just on the vestibule of consciousness, a confluence of our recollections, through which we establish a centre, as it were, a sort of nucleus in [this] reservoir of the soul." And: "From my very childhood, I have been accustomed to abstract, and as it were, unrealize whatever of more than common interest my eyes dwelt on, and then by a sort of transfusion and transmission of my consciousness to identify myself with the object." And Lowes, in a painstaking study of the materials Coleridge had immersed himself in during the years prior to his writing "The Ancient Mariner," was able to trace to these sources every word and phrase of the poem's most vivid stanzas. As Lowes says,

> Facts which sank at intervals out of conscious recollection drew together beneath the surface through almost chemical affinities of common elements, . . . And there in Coleridge's unconscious mind, while his consciousness was busy with the toothache, or Hartley's infant ills, or pleasant strollings with the Wordsworths between Nether Stowey and Alfoxden, or what is dreamt in this or that philosophy—there in the dark moved the phantasms of the fishes and animalculae and serpentine forms of his vicarious voyagings, thrusting out tentacles of association, and interweaving beyond disengagement.

This is not, of course, to detract a grain from Coleridge's achievement; it is only a recognition and demonstration of the sensory components on which imagination operates. For the components had to be integrated, the poem given form. Again to quote Lowes:

Behind "The Rime of the Ancient Mariner" lie crowding masses of impressions, incredible in their richness and variety. But the poem is not the sum of the impressions, as a heap of diamond dust is the sum of its shining particles; nor is the poet merely a sensitized medium for their reception and transmission. Beneath the poem lie also innumerable blendings and fusings of impressions, brought about below the level of conscious mental processes. But the poem is not the confluence of unconsciously merging images, as a pool of water forms from the coalescence of scattered drops; nor is the poet a somnambulist in a subliminal world. Neither the conscious impressions nor their unconscious interpenetrations constitute the poem. They are inseparable from it, but it is an entity which they do not create. On the contrary, every impression, every new creature rising from the potent waters of the [unconscious] Well, is what it now is through its participation in a *whole,* foreseen as a whole in each integral part—a whole which is the working out of a controlling imaginative design. The incommunicable, unique essence of the poem is its *form.*

And Hartmann says:

Thus works ordinary talent; it produces artistically by means of rational selection and combination, guided by its aesthetic judgment. At this point stand the ordinary dilettante and the majority of professional artists. They one and all cannot comprehend that these means, supported by technical routine, may perhaps accomplish something excellent, but can never attain to anything great. . . . Combination procures the unity of the whole by laborious adaptation and experimentation in detail, and therefore, in spite of all its labour, never accomplishes its purpose, but always allows, in its bungling work, the conglomerate of the details to be visible. Genius, in virtue of the conception from the Unconscious, has, in the necessary appropriateness and mutual relations of the several parts, a unity so perfect that it can only be compared to the unity of natural organisms, which likewise springs out of the Unconscious.

Form, structure, relationship, organism (or org in my usage), part-whole systems, gestalt, or closure is basic for the product of imagination and for its process. To see star groups, constellations, instead of unrelated stars—the literal meaning of "consider"—is the gist of closure, of a confluence of elements. Since imagination only regroups sensory material, there is truly nothing new under the sun. Perception is really a harder problem, for red rays and green rays, even falling on separate eyes, do give the "new" sensation of yellow; but imagination cannot conjure a hue for ultraviolet. A mermaid, griffin, or centaur, as Lucretius recognized, are only recombinations of familiar elements. Yet when we recall that a single inning of a chess game may offer some four hundred choices, that all literature is built from the same words and these of the same letters, as all material is of the same elements and their handful of subatomic particles, novelty in combination does not seem too barren. A new and fertile pattern of thought may come from a conceptual reslicing of the universe into fresh classes and the making of new combinations of them. A good insight is likely to recognize the universal in the particular and in the strange—perhaps overexemplified in this statement by Coleridge:

My illustrations swallow up my thesis. I feel too intensely the omnipresence of all in each, platonically speaking; or, psychologically, my brain-fibers, or the spiritual light which abides in the brain-marrow, as visible light appears to do in sundry rotten mackerel and other smashy matters, is of too general an affinity with all things, and though it perceives the difference of things, yet is eternally pursuing the likenesses, or, rather, that which is common [between them].

A good insight generalizes progressively, as is so well illustrated by the growth of mathematics and the formulation of ever more inclusive and freer equations (e.g., the Pythagorian theorem) which can then be applied to an increasing range of particular cases. George Boole, for example, introduced modern logic by recognizing class as basic to, and more general than, number. Finally, a good insight sees (or foresees) in a welter of impressions that which is relevant to the goal earlier indicated by reason; it winnows the important facts from the unimportant. But now we are reaching the

domain of more formal psychological studies.

The Psychology of Imagination

The gestalt school of psychologists, especially, has emphasized the importance of closure or structuring—of "considering"—in insight. Insight is an imaginative way of learning or problem solving, in contrast to the blind and buffeted way of trial and error, often called "at-sight" for contrast. (A neurotic behavior development, inappropriate to the actual situation and, in a sense, no longer goal-directed, might similarly be called "out-sight.") Beyond sensation and even simple perception, involving the correlation of current sense data and of past experience, closure is a basic property of mind. It is, in Goldstein's formulation, the ability to separate a figure from its ground, to formulate a gestalt, or form, to identify an entity. (It operates in seeing three separated dots as the corners of a triangle.) From this flows the setting up of classes and the recognition of spatial—or temporal—relations. Thus Conrad notes the ability to combine parts or elements into a whole, to integrate systems; and also the converse ability to identify parts or elements in the whole, to fragment or differentiate systems. And Wertheimer further recognizes the ability to shift from one whole to another one, to restructure a system.

These activities may seem tautological restatements and are certainly closely related intuitively; yet, as we shall see, they enjoy considerable independence and can be separately measured. Most immediately exemplifying imagination would seem to be the last, flexibility of structure; for Wertheimer correctly says, "Creative thinking is the process of destroying one gestalt in favor of a better one." It is the highest imaginative achievement to be able to restructure in useful ways the basic propositions or axioms on which our great logical thought edifices have been erected. And, as an indirect sign that even such intangible mind work may still be sharply tied to the properties of the brain, there is the observation (Brickner) that stimulation of just one particular small region of the exposed human brain is able to arrest movement in thought. A conscious patient counts smoothly except while the electric current is acting, when the same number is simply repeated. Thus (with the period of stimulation italicized) the subject says, "1, 2, 3, 4, *4, 4, 4, 4, 4,* 5, 6, 7, ... "

Some examples of imagination at the comfortable and familiar level of parlor problems will serve best, perhaps, to illustrate the points made above.

The victim is asked to draw four straight lines which shall pass through all nine dots arranged in a square of three rows of three dots. The presentation sets the gestalt of the square, but within that pattern a solution is impossible. When the imagination overcomes this restriction, however, and extends lines beyond the self-imposed margin of the figure, the answer (Fig. 1) is given almost by inspection. Entirely comparable is the problem of constructing from six matches tossed on a table four equilateral triangles, each having its sides the length of a match. So long as the solver limits himself to the suggested plane of the table he struggles in vain; as soon as he adds a third dimension in his consideration the tetrahedron almost leaps at him.

Another shift in structuring is demanded by the match problem in Roman numerals, VII = I, the object being to move one match and make a true identity. Only after the figure is seen in terms of Arabic symbols can the solution follow, $\sqrt{1} = 1$. A last example of this sort is a geometric problem, commonly muffed by mathematicians no less than

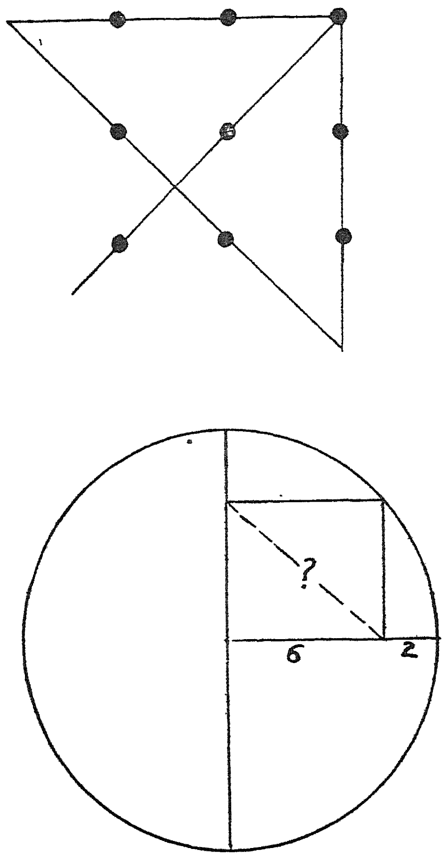


FIG. 1. PARLOR PROBLEMS

by ordinary folk. Draw the vertical diameter and a horizontal radius of a circle. The radius is, say, 8 inches, and at 6 inches from the center a perpendicular is erected from the radius to the circle, and from this point a horizontal line back to the diameter completes a rectangle (Fig. 1). The problem is to find the length of the diagonal connecting the corners on radius and diameter. The answer can be obtained by laborious calculation or, if one is able to flop over his pattern, by inspection; for the other and equal diagonal of the rectangle is simply the radius of the circle.

Of great elegance and beauty and demanding a high order of imagination or patterning is the problem of laying counters on a table. You and an opponent have an ample supply of round counters, all alike but of unspecified diameter, which you place at alternate turns in any desired position on a rectangular table of unspecified dimensions. A counter can be played so long as it does not touch one already placed (and not thereafter movable) or fall off the edge. The winner of the game is the one to play the last counter that can be placed. The problem is, Do you choose to make the first play or have your opponent play first? Surprisingly, there is a definitive and general solution to such a seemingly indeterminate problem, and it cannot be reached by trial and error but by an imaginative invocation of symmetry. You play first in the exact center of the table and you can always win. For at each round, wherever your opponent plays there is an identical position opposite (radially) open to you and will be until the end. Only the center is unique in a symmetrical figure; all other points can be paired.

The equation for sums developed by Gauss, supposedly when a school child, is a similar insight. Most of us, asked the sum of the numbers from 1 to n do an uninspired piecemeal job of addition. Gauss saw the problem in terms of equal number pairs: 1 and $n = n + 1$, 2 and $n - 1 = n + 1$, 3 and $n - 2 = n + 1$—which at once gives $n(n + 1)/2$ as the general solution.

A shift in frame of reference or structure is also involved in a good pun, which is similarly an imaginative creation. As examples, consider the victory garden slogan (spoonerized from the crap game cry), "Weed 'em and reap"; and a wit's responses when aroused from sleep to prove his pungency:

"Make a pun."

"Upon what?"

"On any subject, on the King."

"The King is no subject."

In contrast to the above instances,

many problems require essentially no imagination but either memory or reason. The great bulk of questions aired in quiz programs are ones of simple memory. A problem that can be solved without imagination is that of exchanging dimes and pennies. Three of each are lined up, with one central space vacant between the dimes and the pennies. Any coin can slide forward into a space or can jump over a single coin of opposite kind into a space. The necessary moves can be found by brute trial and error or, more simply, by a little reasoning; but the initial gestalt is retained throughout.

Numerous examples of insight could be given at the infrahuman level: Kohler's chimpanzees which "closed" two elements and brought a box and a stick to reach a banana; Yerkes' ape which, having accidentally whirled into the correct solution of a choice-of-gates problem, regularly thereafter whirled before trying the gate of his choice; Maier's rats which were able to combine "knowledge-of-general-maze pattern" and "block-between-me-and-food" and choose the shortest available path; and the innumerable animals that, during conditioning, seem suddenly to make the induction of "bigger-than" or "nearer-than" or "same-as" and thereafter give errorless performances. But a further pursuit of imagination in man will be more interesting.

If imagination is a definable property of the mind it should also be measurable; and as the definition progresses from the vague impressions of ordinary human dealings to that offered by standardized situations, so the measure moves from the subjective judgment of a person, as having a good or poor imagination, to a fairly quantitative statement about performance. Thurstone, especially, has pressed forward the analysis of mental abilities. By extensive testing with a rich variety of problems he has shown at least seven such abilities which are independent of each other. Thus, individual A may outperform individual B by ten- or a hundredfold on tests which utilize ability 1, while B may similarly outperform A on tests involving ability 2. A similar analysis has revealed some ten perceptual abilities, and others surely remain to be uncovered. Some abilities, such as those of word fluency or verbal understanding, depend for their exercise on learned language, and so performance improves over much of the life-span. But others, such as space visualization, show little improvement in their use after the age of six to eight years; in fact, performance may actually decline. The case for inborn capacities, of particular degrees for each capacity in each person, is thus strong.

Is imagination some one or several of these separable abilities or some common "power factor" underlying them? The answer is not yet available, but it is within easy grasp when persons of outstanding talents of various sorts are measured by such standardized tests. Meanwhile, some interesting guesses may be made. At least four of Thurstone's factors might be involved in imagination, and one of these seems almost to define it. The I, or induction, factor is the ability to see logical patterns or relations (and so would be less related to imagination than to reason). A convenient test for it is to have the subject supply the next item of a series. A very elementary series is: O X X O X X O X ?. A more severe demand is made by: 1, 7, 3, 6, 5, 5, 7, 4, 9, ?. The K factor, measured by the Rohrschach "ink-blot" test, is almost at the other end of the mental spectrum and, far from impinging on logic, plumbs the unconscious. It is of the free completion type; the subject is given an amorphous stimulus and allowed to react with no restraints—as when a person gazes into the flames play-

ing over a fire or at clouds drifting in the sky and "sees" castles or bears or witches acting out untold stories. It is suggestive that a group of successful executives performed (in richness, variety, etc., of responses to the ink blots) significantly above the average on this test.

Two other factors rather specifically deal with closure. The A factor is the ability to make a closure or complete a gestalt and is measured, for example, by having the subject identify partially erased pictures or words. The E factor is the ability to replace one closure by another and is tested by the Gottschalt figures (Fig. 2), or by "hidden faces" in a picture of different manifest content. The two abilities, especially E, are rather precisely those considered earlier in defining the act of creative imagination. It is impressive that two independent factors can in fact be isolated for such intuitively equivalent actions as making or remaking a closure! When such primary abilities have been measured in our Einsteins, Edisons, Toscaninis, Van Goghs, Masefields, and Lincolns we shall be far along the way. From descriptions of Coleridge, for example, there is little doubt that he would have performed very well indeed on tests for K, S (space), W (word facility), M (memory), and I and A.

"Zoology" of Imagination

The inheritance of imagination will be ever more easily studied as identification becomes more precise. Even now the comparison of the mental abilities in twins and in siblings is in progress. Pending such finer analysis, I may mention evidence that a strong hereditary element is present for "averaged" intelligence and for particular talents. Newman and Holzinger, for example, have found an average difference in I.Q. of 5.9 for identical twins raised together and that this value increases only to 7.7 for those raised apart. In contrast, fraternal twins raised together differ by 8.4, and sibs by 14.5 if raised together, 15.5 if raised apart. Orphan pairs differ by 17.7, whether apart or together.

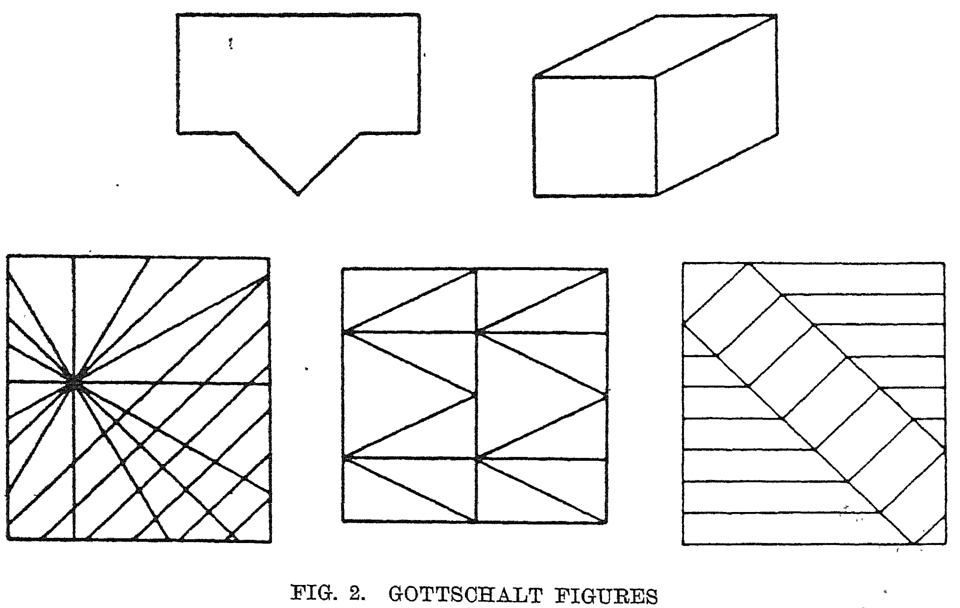


FIG. 2. GOTTSCHALT FIGURES

THE INSTRUCTIONS READ AS FOLLOWS: ONE OF THE TWO UPPER FIGURES IS CONTAINED IN EACH OF THE THREE LOWER DRAWINGS. ONLY ONE FIGURE IN EACH DRAWING SHOULD BE OUTLINED.

A study of outstanding contemporary virtuosi and singers by Scheinfeld shows that two-thirds of the parents of these artists possessed high musical talent. Conversely, in families with both parents talented, two-thirds of the children also were gifted, whereas in those in which neither parent showed talent (but of course one child was outstanding) only one-fourth of the offspring were gifted. He suggests that the minimal genetic interpretation of these facts is that musical talent demands the presence of at least two dominant genes. The importance of the hereditary factor is further attested by the age at which these outstanding musicians had clearly manifested unmistakable talent—an age under six years!

Interesting data in the field of science come from the starring results in *American Men of Science.* Mathematicians achieve their star at the average age of twenty-nine, with physicists close behind; botanists and geologists wait until they are fifty-two for the same kudos. All will probably agree that sheer imagination and intellectual power, as compared with experience and learning, are relatively more important in the former fields than in the latter. That the growth of mental *capacity* is more a matter of biological maturation than of life experience is suggested by all these findings, as well as by the high performance of children on some of Thurstone's factor tests. Similar conclusions in other fields of neural performance are justified by Coghill's evidence that salamanders kept anaesthetized during the developmental days when their fellow embryos struggle about "learning" to swim, swim well at once on ending their trance state; and by the fact that a normal human baby begins to smile at two months after birth, a seven-month premature, four months after birth.

A final comment in this area, on the evolution of mental abilities. Several men have attempted to construct a scale of comparative intelligence of animals in terms of such learning criteria as the maximum time over which a trace-conditioned reflex could be established, but without convincing success. That man's abilities differ in degree more than in kind from those of his slower-witted biological relatives is nonetheless probable. Apes show learning by insight as well as by trial and error and have even been taught to work for money as industriously as do their gifted cousins. Wolfe has trained chimpanzees to put counters into the slot of a vending machine to obtain food, different amounts for different colors. Having learned the purchasing value of these colored bits, the animals will do "chores" to obtain them and will work harder for the more valuable ones.

Since the gross and microscopic structure and the chemical and electrical functioning of the brain are measurably comparable in all vertebrates, reasonably alike in all mammals, and strikingly similar in the higher primates, where enormously detailed parallels have been demonstrated, a likeness in mental capacities is not surprising. The gray cortex of the cerebrum has swelled out from the primitive nerve cell groups to which came messages from nose, ear, and eye. These "distance receptors," sensitive to changes in the world at a distance from their possessor and so posing problems to the animal for a priori solution, somehow whipped into existence a brain capable of solving them. It is the same cerebrum in man and monkey; but man has a deal more of it, which permits rich additional permutations. And now that the brain is introduced into the picture, we must consider knowledge in the more medical areas.

The Brain and Imagination

Pathology. It remains sadly true that most of our present understanding of mind would remain as valid and useful if, for all we knew, the cranium were

stuffed with cotton wadding. In time, the detailed correlation of psychic phenomena and neural processes will surely come; but today we are hardly beyond the stage of unequivocal evidence that the correlation does exist. The neuroanatomist and physiologist are still crudely deciphering the architecture and operation of the organ of mind; the psychologist and psychiatrist are concerned with nuances in the overtones it plays. Yet the gap is narrowing, and a primitive bridge is offered by the grosser disturbances of brain and mind. Perhaps most dramatic are the aphasias, a group of disturbances in the ability to handle "meaning," associated with more or less sharply delimited regions of brain damage. Since disease or accident rarely destroys an exact division of the cerebrum and since different divisions have unique functions, the symptoms are commonly mixed and vary from case to case; but such a diagrammatic instance as the following has been reported.

An educated man, proficient in several languages, suffered a "stroke" which left him aphasic. At one stage in his slow improvement he could converse freely and intelligently but could not read. His vision was not disturbed; he could copy a paragraph correctly, but it carried no meaning to him. He was able, in fact, to take dictation in one language, translate in his mind, and write the correct passage in another tongue. But having written it, he could not read his own writing; it was Chinese to him! One is reminded of the small boy who, called on to read aloud in class, was asked the meaning of what he had read and gave the startled and startling reply, "I don't know. I wasn't listening." Another type of case, with disturbance more on the motor than the sensory side, could not give the word for 7 but could say it by counting aloud from one. Another, wanting to say "ruler" could not do so until he had made a sketch of one. Yet another could not say words but demonstrated, by holding up fingers for syllables, that he "knew" them. For example, "What is a baby cat?" No sound of kitten, but two fingers raised in response. Even when words remain, they are often inexact or roundabout, and the subject seems to be indulging in fancy speech or "overwriting," as shown by the following quotations from a patient during and subsequent to an aphasic episode: "I trust I am now learning to do my very best to secure the ideas to put myself carefully to operate the item to me which was seeming away when needed so much by me." And, later, describing his aphasic condition, "Personally I got dumb and could not remember things." (These cases are quoted from Weisenburg and McBride's *Aphasia.*)

As these instances show, there is commonly a disturbance in the use of language, but this is too limited a view of the defect. Language is man's main symbolic system, and aphasia has been considered as a disturbance in symbolism (Head) or in propositional expression (Jackson). But formal symbols are still but one avenue to meaning, and the others may also be disturbed in aphasia. A patient may fail to recognize familiar tunes, or may be unable to identify by touch a common object placed in his hand, such as a key or knife or pencil, although he recognizes well enough that some object is there and may name it at once on sight. Similarly on the motor side, a man could not at will move his tongue over his lip on instructions, which he understood, but could do so to remove a crumb placed there. Comparable defects in meaningfulness have been produced in monkeys by appropriate brain operations on the parietal lobes (Kluever and Bucy). Such an animal still sees and feels objects as well as ever, but it no longer recognizes them. It will pick up, bring to its mouth, and drop again, in interminable random activity, such normally intensely discriminated objects

as a banana, a stone, and a live snake. This behavior is in sharp contrast to that of a monkey whose visual sensory cortex (occipital) has been removed. Then, while light sensitivity is fully retained, as shown by eye reflexes, all visual perception is gone; the animal is effectively blind.

Thus meaning, in its widest sense, is imperiled by such brain insults, and the gestalt psychologists have not failed to point out that the very ability to create closures is damaged in aphasics. But, in man, language (with mathematics as one form of language) remains an especial index to the workings of mind; and Pick, combining philological study with his clinical observations, has formulated a series of stages in language use, which may be interrupted anywhere by the aphasic slash. On the sensory or receptive side there is, first, the perception of speech as distinct from mere sound. There follows the recognition of words as separate entities and then of the "musical" parts of speech, cadence, and intonation. Only then comes an awareness of meaning, followed by full understanding of sentences with their proper word relations and emphases. Turning now to the motor or expressive sides, the sequence is intuitive thought (also called verbalizing or inner speech), which becomes structured thought, and is then cast into the schema of a sentence, only after which are the actual words chosen and the result articulated. Aphasia may thus prevent sensation from emerging into meaning, meaning from eventuating in behavior, or meaning itself from coming clear. The last would be a disturbance in closure or structuring. This represents, perhaps, the basic disintegration of imagination. Imagination may be the word for that all-important no man's land between the end of the receptive process and the start of the expressive one.

The future is parturient with the answers. For the advance of neurosurgery is offering to study clean-cut cases of brain defects (or stimulation); patients with local brain amputations or incisions for tumors or infections or even, rather less soundly, for mental disturbances. And the advance of psychological measuring is supplying better precision tools with which to make the study. Thus, at the receptive levels, superficial damage to a region (17) of the visual cortex destroys color sensations but preserves pattern; more profound damage destroys pattern recognition as well while leaving (as in the monkey) light sensitivity. Comparably, direct stimulation of area 17 in a conscious patient produces an awareness of lights; when the next area, 18, is stimulated, the lights move about; and, if the next brain region is excited, complete pictures flash into consciousness —as of a man somersaulting toward the observer.

And at the integrative or imaginative levels of meaningfulness, we need only the results of applying the tests for primary abilities, especially for Thurstone's A, E, I, and K factors, to patients with specific brain operations to make a great step forward. Even now, Halsted has found a striking defect, in patients whose frontal lobes have been partly removed, in the ability to make categories. A normal adult, given a miscellaneous collection of familiar objects and asked to group them in as many ways as possible, can set up dozens of categories for grouping—by color, shape, material, use, and so on and on. The operated patient can make few, if any, groups. Now making groups or classes is a form of closure, and here again we see imagination crumbling along with the brain that spawns it. But we must look more closely at the structure of the brain and the problem of localization.

Anatomy. The introspective psychologists have distinguished between crude sensation, organized perception, and full-formed imagery on the sensory side;

reason, will, and action on the motor side. The boundaries are not sharp, to be sure, yet one can almost follow the one into the other on moving with nerve messages along the nervous system. From the single receptor, or sense organ —tactile corpuscle of the skin, eye, ear, etc.—comes but one modality of sensation—touch, light, sound. This has the attribute of intensity, given by the frequency or closeness with which impulses follow each other in each nerve fiber and, less, by the number of fibers activated. When the message reaches appropriate regions of the nervous system, the sensation also has its particular quality of touch, or pitch, and this much of pattern that a "local sign" is attached, so that the region of the body (touch) or receptor (eye) from which the messages come remains identifiable. As nerve fibers from receptors gather into nerve bundles (along with motor fibers for much of the way, but separating at the ends, especially where they join the central nervous system), sensory messages are grouped together either by modality, in special cases like those of seeing in the optic nerve and those of hearing in the auditory nerve, or more generally by region, as all the skin and other sensations from one finger in a particular nerve or nerve branch.

Yet as soon as these latter nerves enter the nervous system, mainly along the spinal cord, the relay fibers are shuffled about so that they also become grouped by modality. Thus, if a nerve to the leg is cut, some portion of the leg skin (and muscle) will have lost all sensation of touch, pressure, temperature, pain, position, vibration, etc. But if one of the relay bundles in the spinal cord is damaged, the entire limb will lose only the sense of touch or of pain or of position, as examples, depending on which part of the cross section of the cord is injured, while retaining the other senses unimpaired. When these second relay fibers pass on their messages to the third member of the team, in the thalamus at the base of the great cerebral hemispheres, there is another reshuffling so that region again enters strongly into the arrangement. And from here the nerve wires fan out to reach the cerebral cortex, each to its own particular spot.

Optic fibers run to the occipital lobe and are there ordered so that each region of the retina is represented at a roughly comparable position on the cortex. Fibers from the skin carry all the cutaneous sense messages, remixed as to modality, to the parietal lobe just behind the great Rolandic fissure, where the various body regions are neatly arranged in order; from the foot at the vertex of the brain to the head well down the lateral surface—as if a tiny and rather grotesque manikin of the body lay upside down (and right side left) on this region of the brain. Sound, smell, balance, hearing are similarly "placed" in given parts of the cerebral cortex, and, on the motor side, the body muscles are represented just in front of the cutaneous area, across the Rolandic fissure, and are ordered as a manikin in like fashion. Muscle sense, which tells us limb position, for example, overlaps the cutaneous and motor areas, again in the same order from foot to head. The spatial arrangement of entering nerve fibers in the auditory cortex is in terms of pitch, rather than of position and, just discovered, there is a double location for hearing—two distinct brain areas in each hemisphere.

These cortical areas to which sensory nerve messages are projected from the thalamus, or from which motor messages project through the thalamus, are called the projection areas. They occupy but a small portion of the cerebral cortex, being surrounded by various association areas; and indeed both the microscopic characteristics and arrangements of the nerve cells and the functional influences

that have been traced between them show that some half a hundred individual and distinctive areas are present in the cortex of man. Some of the association areas, in close relation to projection areas, are primary and concerned directly with an elaboration of the particular projected messages. More of them, the secondary association areas, are concerned with the most general interrelation and reworking of the elaborated sensory clues, present and past. Thus, referring again to the aphasias, destruction of the visual projection area (17) causes blindness; of the visual primary association area (18), a pure sensory aphasia (agnosia) for seen objects or symbols—inability to give meaning to written words; of secondary association areas, a greater or lesser loss in meaningfulness in general, an integrative aphasia (aconia). A pure motor aphasia (apraxia), like the pure sensory one, would involve a primary association area related to the motor area for, say, speech. Stimulation, conversely, gives lights (17), moving lights (18), and moving pictures, respectively, as described earlier.

Now what of sensation, perception, and the like, and especially imagination, in relation to this sketched-in organization of the nervous system? Clearly, a knowledge of structure and localization of function is not enough; for a single nerve impulse running in a single nerve fiber in one or another part of the brain is much the same thing, and a billion of them simply added together are only a billion of the same things. But nerve impulses are not simply added. Messages set up from a single hair on a cat's paw—by touching it with a hair on the observer's hand so lightly that the observer feels nothing—run up a sensory nerve fiber to the spinal cord and there "explode" into many impulses running up to the brain in many fibers, which further interact along the way. A person listening to a watch tick hears it as louder while a light is being looked at; and experiments on cats show a similar enhancement of messages in the auditory sensory paths when the nearby optic paths are simultaneously active. The point is that as sensory messages ascend toward and into the cerebrum they are not merely relayed and regrouped, they are also reorganized and reworked; in fact, we shall see they even reverberate.

What may be the conscious concomitants of these various stages of neural work is not known; but all the evidence suggests that they would rise in richness along with the intricacy of activity patterns in the nervous system. If awareness is the internal view of events or systems which are material to the external view, as many hold, then some protoconsciousness (probably not self-consciousness, or an awareness of being conscious) must exist in the simplest blob of living protoplasm or, for that matter, even in all substance. But, just as behavioral capacity leaps upward when a nervous system is present and again as each major improvement in it evolves, especially as the great cerebral cortex comes to flower, so subjective awareness does likewise. Some consciousness of sensation may exist in the spinal cord, as does some ability to recombine and learn, but this would be difficult to prove and is surely of negligible degree compared to what is experienced by man's brain. Nevertheless, the sensory messages from receptor through sensory nerve and spinal bundles probably represent pretty pure and raw (but unsensed) "sensation"—as suggested by some of the facts on the results of damage. And if they reach projection areas without much interaction with other activity patterns they will result in simple consciously recognized sensation. There is even some evidence that the most primitive undiscriminated "feelings," such vague discomfort as accom-

panies mild bowel cramps, may depend on older subcortical brain regions, such as part of the thalamus. If, however, they interact with other current sensory messages, and with the memory traces of past ones, then they are probably more of the character of perceptions, after moving on from the thalamus and into the projection areas. By the time the primary association areas are engaged, with their added complexity, imagery is probably also present.

A comparable but reverse sequence exists on the motor side, with drive or willing or maybe intuitive speech at the start and particular muscle contractions at the end; with the same possibilities of interruption along the way, grading from the aphasias to the out-and-out paralyses. Volition may be disengaged from motor expression in less drastic ways than by anatomical damage: a person recovering from the stupor due to inhaling concentrated carbon dioxide "wills" to move his hand in response to a request, but nothing happens for a minute or more when, to his surprise, the hand moves "of itself" (McCulloch). The leaden limbs of a nightmare, when the dreamer cannot run for his very life, may be a comparable neural block; at least in deep sleep the toe reflex from scratching the sole (Babinski) behaves just as it does when the motor pathways of the nervous system have been injured.

Between perception and imagery on the one hand and volition on the other lie the great mental territories of imagination and reason. It might be useful to consider imagination as the culmination of sensory events, reason as the origin of the motor ones. Or perhaps reason, with its attendant logic, verbalization, decision, and willing, is more properly the start of motor events, and imagination is the more pervasive and encompassing mind work which is the keystone of the sensory-motor arch. Men with moderately severe brain injuries may perform well on the usual intelligence tests, while falling down on those which sample imagination. Indeed, imagination may include a "power" factor of intelligence underlying the others, as Spearman believed, and depending on the mass functioning of the whole brain, as Lashley's work on animals suggests.

Certainly, as earlier outlined, imagination depends on sensory information. Man cannot see the world other than as it unfolds itself within the sensory projection areas of his brain. These determine his basic orientation to externality. In the very spatial arrangement of the areas of vision, skin, and muscle sense is embedded an unformulated geometry. The basic units of physical science are distilled from these areas: space (centimeters) from vision, touch, muscle sense, and the vestibular system (the balance organs located within the ear); substance (mass, grams) from smell, taste, touch, muscle sense, and, secondarily, vision—a congenitally blind person, on achieving vision, feels objects "hitting" his eyes until he learns to project his experience into the third dimension, as we all project the sense of touch to the end of a stick with which we explore the bottom of a pond—and perhaps, even, the notion of force comes from touch and muscle sense, of matter more from taste and smell; and time (seconds) most directly from hearing. At least, as evidence for this last, is the powerful reaction to heard rhythm, tapping to a tune, and the fact that a sound track of words or music run backwards is completely meaningless, whereas a reversed light track, though often ludicrous or impossible, is perfectly meaningful. Moreover, one's subjective judgment of time certainly depends on a brain clock, which runs fast in fever according to a precise mathematical function of the brain temperature (Hoagland). (In another, more fanciful, sense one might think of time running through the cortex from behind

forward. Sensations, from already past events, enter behind the Rolandic fissure; motor impulses, which will set off future actions, leave from in front of it.)

From space, mass, and time comes, in turn, the notion of entity—the basic gestalt of all and the first flutter of imagination. In this sense, that entity is given by the sensory organization of the nervous system, Kronecker's famous mathematical dictum takes on a profounder meaning: "God made the integers, man did all the rest." And, in supplying the substratum for thought, vision in man is surely of overwhelming importance. Our thought words are almost all of visual reference, although we do "apprehend" a meaning and refer to a "tangible suggestion" or a "weighty problem," and we may say of something, "it looks heavy or hard," but never that "it feels red." Brightman has written the telling paragraph, quoted below, on the indications from language; and Sherrington showed how the eye, the main distance receptor in the higher vertebrates, has dominated the evolution of the cerebrum.

A survey of language shows that many of the terms used by common men as well as by scientists and philosophers in expressing their fundamental concepts are words which point only to visual experience, away from the invisible experience of free, purposive valuing. . . . Even theoretic man has clung to the visual tradition, or as it has been called, "The Spectator Theory of Knowledge." A spectator is one who looks; theory means looking; knowledge is the only word here that originally allows a non-visual experience. Realism, of course, refers to the visible thing, the *res*. But even idealism is named from the root ἰδ, which means see. Intuition (*intueor*) is simply looking at what is seen; vision, that supposedly spiritual act, means only seeing (*video*).

No matter how intellectual we try to become, we cling to *insight*. We seem to walk by sight and not by purpose in our etymology. If our insight reaches its highest level, it is synopsis, in which the optical is evident (as vision is evident in the very word "evident"). The esthetic is often interpreted as having some relation to purpose; yet the Greek word means only what is perceptible to sense. Even imagination (*imago, imitor*), which originally referred only to likeness and might thus mean likeness in purpose or value, quickly came to mean the seen likeness of a visible object. A philosophy is a world view (*vue*), that is, something seen. Contemplation comes from *contemplor* (to gaze at). Consideration is from *considero* (to look at closely); an interesting hint at man's early stargazing, for *sidus* (star) is concealed in the word. Even the ultraphilosophical German word, *Vernunft*, derives from *vernehmen*, which originally meant to see.

The distinguished art critic Ivens has made the provocative suggestion that Greek art and architecture and mathematics are distinctly inferior to those of more modern times (a critical judgment which he supports in considerable detail) because the classic Greeks were essentially hand-minded (touch and muscle sense), and modern man, eye-minded. The former, he urges, gave the finite, discrete, and particulate; the latter, the infinite and graded. Aside from such historical evidence as the continuity implied in Zeno's paradoxes, this view seems unsound on a biological basis. Greek brains were built like ours, of the same human race; and an earlier race, Neanderthal man, had, if anything, a more emphasized vision than our own—at least the occipital lobe of his brain, with its visual areas, was mightily developed. True, individuals vary in the degree to which their imagery is visual, auditory, tactile, and the like (it might even be possible to measure this in terms of the relative strengths of the occipital (α) and the parietal (β) rhythms in their electrical brain waves); but this variation almost surely follows the chromosomes, alike in old or new Greeks

No, the more static constructs of the classical period are to be understood rather as an earlier phase of imaginative creativity. In all human thought, the constant is adumbrated before the variable (mathematics), statics before dynamics (physics), structure before func-

tion, and classification before relationship or evolution (biology). It is not surprising that this is so, for thus does the brain create imaginings: remember that stimulation of the visual projection area generates static lights; of the first association area, dynamic ones; and of the second association area, moving pictures!

Physiology. What, then, of the mechanisms of brain functioning, of the generation of thought? Granting, again, that the exact relation between neural processes and conscious events remains unknown, it is still possible to recognize some striking parallels. Are closure and patterning basic to imagination? They are simply shot through the entire feltwork of the nervous system! Not only in the large-scale organization we have already noted but in the small-scale one no less. True, particular nerve fiber bundles connect each of the separate areas of the cortex with all; many directly, the others by relays. True, some of the bundles carry messages which excite the nerve cells they reach, so that when cells in area X fire messages to area Y the cells in Y become active. But it is also true that comparable nerve bundles connect cortical areas with thalamus, with spinal cord, with all parts of the nervous system; so that a nerve impulse entering the central mass along any fiber path could, in principle, find its way by one route or another to every part of the nervous system. (And in fact, too, under some conditions; as when strychnine has rendered the whole neural apparatus more sensitive, and a slight irritation anywhere can set off a general convulsive reflex contraction of all the muscles of the body.) And it is further true that the nerve impulses running from area X may not excite but inhibit or suppress the cells in area Y so that these stop their current action and cannot be re-excited for a time. Thus, stimulating the arm region of the motor area (4) will cause arm movements; but stimulating a region (4–S) only a few millimeters forward will stop arm movements and even prevent further stimulation of area 4 from starting them. Surprisingly, although 4 and 4–S lie next to each other on the cerebral cortex, this suppressor action depends on a distant locus of interaction; and part of the interplay is via a complex relay path, from 4–S to deep cells in the cerebrum (basal nucleus) and from there to the thalamus and from there back up to 4.

Each nerve cell is so richly supplied by nerve fibers reaching it from all sorts of local and distant neural regions, reaching it and making functional connection (synapse) with it, that it is rather like an egg packed in sticky excelsior. Messages bombard it along these many paths, some pushing it to action and some to quietude, some perhaps powerful enough to tip the balance individually but most surely requiring the help of their like fellows. Further, the nerve cell is being influenced by the blood passing it, by the oxygen and sugar it receives, the salts that bathe it, the electric currents from its neighbors, the temperature at which it finds itself, by drugs which reach it. And from this welter of influences—its state of health, the condition of the environment in which it is living, and, particularly, the clamor of allied and opposed messages reaching it—from all this comes a single result: the cell fires messages along its own fiber to still other cells, or it does not fire. There is, to be sure, some gradation in number and frequency of impulses sent or in duration of inactivity and depth of inactivability, but essentially the balance is between action or no action. Just so the judge, depending on the state of his stomach, or the temperature of the courtroom, or the bombardment of arguments on each side of the case, renders a single decision for or against. (Freedom of the individual to make the decision is equally easy or

hard to discover in the nerve cell and in the judge.) It is the collective and patterned actions of the several billion nerve cells of our brains that determine our behavior and accompany our thoughts. We must explore further this neural patterning.

A few years back, the only well-recognized pattern was the reflex arc. A message entered along a sensory nerve, continued through the nervous system along direct or relayed connections, and finally emerged in a motor nerve. Except as messages were in transit, the nervous system was presumably quiet. Today we know, largely from the electrical pulses of the "brain waves," that nerve cells are continuously active in wake or sleep, and many beat on like the heart. In part, this beat depends on the chemical and physical state of the cell and its surrounding fluid; in part, on the nerve messages playing upon it. Suppose cell *A* sends its fiber to connect, among others, with cell *B*, *B* with *C*, *C* with *D*, and *D* with *A*. If *A* were once activated by a message from *X* it would excite *B*, and so through *C* and *D* be re-excited itself. Another branch from *D* might excite *Y*. Then, once started, such a circuit might continue active, with excitation going round and round like a pin wheel and throwing off regular sparks of activity on each cycle. Of course this picture is too simple—the circuit would not be set off so singly, it would vary in its path and speed of spinning, it would have to stop by cell fatigue or other impulse interference, it would involve many more cells and connections, were it to accord with the actual behavior of the brain. But what is important is that just such circuit patterns, with all the needed complexities, have been shown to exist and function in this manner (Lorente de No). Closure in mental processes, did we say? Here is closure woven into the very fabric of the nervous system!

These closed circuits are mostly over minute distances, in single centers of the nervous system, but comparable ones exist on a gross scale. In many cases, also, a nerve cell cannot be made to fire by impulses reaching it along a single fiber but requires a nudge from two or several arriving at the same time (the main effect of a single impulse is expended in a few ten-thousandths of a second) and even from different regions. Again, what a beautiful basis for making new gestalts or recombinations of sensory material! As one example, recall that light can make sounds seem louder; as another, how association areas rework and embroider the activity of projection areas. A further instance shows that messages from the frontal lobe of the brain, as well as from the optic nerve and thalamus, must reach the visual centers for them to become fully active; for after injury to the front of the cerebrum the field of vision is narrowed, even though the retina and its immediate brain connections to the optic brain areas remain intact (Halsted).

Several important interactions occur between the cerebrum and thalamus, besides those already mentioned. Through the latter pass all sensory messages on their way to the projection areas and to full consciousness; and in another part of the thalamus are coordinated the bodily responses and perhaps the subjective aspects of emotion and other primitive feeling. When the cerebrum of an animal is removed, affective behavior is grotesquely exaggerated; so nerve paths from the cerebrum hold the thalamus in check. Other fibers from the cortex can activate the thalamus, and, indeed, even as sensory messages relay up through this part of the brain, other messages coming down to it from the cortex can block or enhance their passage (Dusser de Barenne). Perhaps what we call attention is in action through these paths which functionally open or close the gates

of the thalamus and allow now one, now another, group of sensory messages access to the cortex and full consciousness while relegating the others to the fringe of awareness or even to the unconscious. (This is not to say that all cortical activity is conscious or self-conscious, for such is not the case. James's figure of consciousness, as a single lighted candle carried from place to place in the cavernous darkness of a great building, is still a good one.) And, a final example, certain paths from the thalamus radiate out to much of the cerebral cortex and, when stimulated, set the whole cortical sheet into vigorous electrical beating (Morison and Dempsey). Perhaps this mechanism is responsible for the overactive mind work that follows an emotional shock. Perhaps just this occurred in Goethe's brain when news of his friend's suicide "crystallized" the plan of "Werther" as, "the whole shot together from all directions and became a solid mass." And surely here again is a neural basis for closure.

Besides such provocative nerve messages, able to influence the action of millions of nerve cells, other integrating mechanisms exist in the brain. Waves of action can be made to travel slowly over the cerebrum, for example, even when all anatomical connecting paths have been severed. Electric currents are probably involved here, and, indeed, these are a major factor in that environment which influences the discharge of the single nerve cell and the coordination of the many. Electrical fields have been richly demonstrated in brains; have been shown to vary their pattern with state of activity, chemical environment, drug action, and the like (Gerard); and have even been successfully invoked to explain in detail a variety of optical illusions in man (Köhler). By such various mechanisms, then, great masses of nerve cells—the brain as a great unity—act together; and not merely do two or a billion units sum their separate contributions, but each is part of a dynamic fluctuating activity pattern of the whole. This is the orchestra which plays thoughts of truth and beauty, which creates creative imagination.

Plenty of problems remain; some demand attention. Most urgent to our present theme is how novel neural patterns originate, since they must accompany novel thoughts or learning in general. Much attention has been given to the phenomena of learning: by "at sight," the slow cumulation of a new "correct" response in the course of conditioning experience, the conditioned reflex; and by insight, the sudden grasp of a solution and abrupt performance of the correct response, the gestalt or closure or imaginative act. They seem very different, and, as Terman put it, conditioning serves admirably to explain stupid behavior; gestalts, intelligent behavior. The mechanisms may indeed be quite different, but it is possible, perhaps probable, that they are basically quite similar. In both cases, new functional connections must be established in the brain; and this process may be more gradual and cumulative in the case of insight than appears. For here, also, much brain work precedes the imaginative flash—the theory of gravitation may result only when the metaphorical apple falls on the prepared mind—and only when the process has progressed to some threshold level does it overflow into a conscious (self-conscious) insight.

So long as our picture of the nervous system was that of the telephone exchange, with reflex plugs all set and each sense organ subscriber connected with, and able to call to action, its allotted muscles, the appearance of new responses seemed to demand the presence in the brain of rather mysterious telephone operators to shift the plugs. Now, with our discovery of a far more fluid nervous system, one unceasingly active and with

neural and electrical messages rippling the whole into dynamic patterns, which flow from one contour to another as present influences play upon the condition left by past ones—with such a picture the arrival of new neural relationships is no great problem. Schemata have been offered—in terms of nerve impulse balance, electrical fields, fiber growth—which at least indicate reasonable avenues for further exploration. More difficult still is the question of whether the new closures come to occupy particular neural regions, whether experience is parceled out in brain cubbyholes from which memory can withdraw and examine one package or another.

The answer seems to be mainly No, but with considerable reserve. A conditioned reflex established exclusively via one eye or executed by one hand can be at once elicited through the other eye or with the other hand; a learned response to a particular figure will be given unhesitatingly when the figure is changed in size, color, intensity, position, and even, within narrower limits, contour. Yet in each of the shifts indicated, different particular nerve fibers and connections are involved, at least in part. Further, Lashley has shown that the learning ability of rats parallels the total brain mass and is decreased as the brain is whittled away by operations. But the loss is not greater when any one region is destroyed as compared to another nor even when extensive thin cuts are made rather than removing a compact lump. Yet even here there begins to appear some suggestion of localization, for, though removal of the visual cortex does not prevent a rat from learning a light-discrimination problem, it does wash out a previously learned problem of this sort. And recent work on animals like the dog, with more elaborated brains, suggests some striking localizations.

Thus Culler established a conditioned leg flexion by sounding a given tone when an electric shock was administered to the paw. The tone alone then led to flexion, unless this conditioned response was elicited for a number of times without being "reinforced" with the shock, in which case the response was temporarily "extinguished." This is all routine; what is startling is his report that he found a region of the cortex only two millimeters in size and lying in association areas well away from cutaneous, hearing, or motor areas, which, on direct stimulation, caused leg flexion in animals with an active conditioned reflex but which was inactive in animals in whom the reflex was extinguished or had never been established. Another report, by Martino, is perhaps even more dramatic. He performed his conditioning so that the right eyelid blinked when red light was shown, the left eyelid with violet light. He then put strychnine locally on the optic cortex of either the right hemisphere (connected to the left field of vision) or the left one. With the left side rendered overactive by the drug, red (but not violet) light led to eye spasms and convulsions; with the right side drugged, only violet light produced the response.

If such indications hold up, a rapid advance in understanding in this field is imminent. Perhaps learning is initially a function of the whole brain and as ephemeral as a pattern of activity. But even activity leaves some more permanent change in the active part—think of the hypertrophy of an exercised muscle. And brain regions which are most active in particular patterns—think of the nodes and internodes of crossed wave trains—might well acquire, with repetition of these patterns, alterations which are both more local and more enduring than the initiating disturbance. With such regions located it will become practicable to look for the kind of change which endures; change in chemical composition or metabolism, electric potential or resis-

tance, cell structure or connection, or whatever it turns out to be when found. The figure of a river and its bed, used so vividly by Child in picturing the general relation of structure and function, is apposite here. The river carves its bed and the bed controls its waters; only by their continual interplay can a particular system develop. The spring floods are mass responses of the whole to environmental conditions and are transitory dynamic patterns, yet they leave local and lasting changes. Where the waters pile up most and the currents are swiftest—where the activity disturbance is most extreme in a particular total situation, as the potential fields in the occipital lobe on visual stimulation—there are produced the washed-out banks or the undercut cliffs which determine the river's flow for decades to come—the concrete regional changes wherewith the past directs the future, the basis of memory.

A final problem before coming to the implications of our analysis: What is the neural basis for the striking quantitative differences between man and man in intelligence or in the several abilities which constitute intelligence or its component, imagination? Surely brain size as such is not the answer, as many studies have demonstrated. Perhaps absolute or relative size of the association areas would show better correlation with intelligence; or perhaps the richness of fiber connections and the architectural intricacy—as the more elaborate circuits make the better radios, large or small. And the factor of activity level is almost surely involved; not only the size and number of nerve cells but their rates of beat, maintained potentials, irritabilities; their functional vigor. This, in turn, depends on their composition (make what you will of the fact that the brains of women contain a higher percentage of lipins—fats—than those of men) and on their metabolism; and this, on the blood supply and the amount of oxygen and sugar it brings, on the salt and acid and other components of the tissue fluids, on particular stimulants or depressants, as the thyroid hormone or anesthetic drugs, and the like. The influence of caffeine, alcohol, strychnine, cocaine, morphine, hashish, absinthe, and mescaline on brain metabolism and activity are being steadily worked out; their dramatic effects on the mind, especially on hallucinations and imaginings, are commonly enough known and are also being further studied (Kluever). As the sets of facts are brought together new understanding will arise. Possibly from this direction we shall get a clue as to the finer differential between brains: what gives one man a vivid imagination but a poor memory, another an encyclopedic memory but dull imagination. And when that answer is at hand science will indeed have established the biological basis of imagination.

Implications

Without awaiting these riper fruits of research, some immediate morals are worth plucking. The ideas tossed into consciousness by imagination are, we have seen, overwhelmingly bad—untrue or unbeautiful—and must be curbed and ruddered by reason. Here, surely, lies a difference between the more imaginative initiator and the more rational critic. Formal education is directed to our conscious reason, which can at least be supplied with content and practice; if the more intuitive and unconscious imagination can be cultivated we have yet to learn the secret. There is the danger of reason stifling imagination, that "enterprises of great pith and moment" will be "sicklied o'er with the pale cast of thought." From the young, the naive, the dreaming, the drug users, comes a great spate of fresh imaginings, overwhelmingly dross but with those rare

grains of great insight yet more common than from the old, the critical, the staid, or the sophisticated. To teach rigor while preserving imagination is an unsolved challenge to education.

Again, each important advance in form, in structured truth or beauty, is the result of a new closure, of a fresh set of axioms; a better set, resulting from the greater knowledge and understanding built with the aid of those dying. The forming mind of the young can use the new as comfortably as the old, but the formed mind of the teacher cannot readily run along the new-gauge tracks. The concepts of infinity, relativity, indeterminism in the physical realm, as evolution in the biological, were difficult for the established generation, simple for the oncoming one. Yet unless we forever question the basic imaginative constructs of our predecessors we condemn ourselves to working at progressively more detailed and trivial levels, to filling in further digits past the decimal point. Recall Trotter's provocative statement:

> When, therefore, we find ourselves entertaining an opinion about the basis of which there is a quality of feeling which tells us that to inquire into it would be absurd, obviously unnecessary, unprofitable, undesirable, bad form, or wicked, we may know that that opinion is a non-rational one, and probably, therefore, founded upon inadequate evidence. Opinions, on the other hand, which are acquired as the result of experience alone do not possess this quality of primary certitude. They are true in the sense of being verifiable, but they are unaccompanied by that profound feeling of truth which belief possesses, and, therefore, we have no sense of reluctance in admitting inquiry into them.

In ethical and religious attitudes, even more, the axioms are set at childhood; the re-education of a generation of "Hitler Youth" gives little promise of success. Why, even in aesthetics we learn our particular values; the dissonances of a mere generation ago are consonances to ears of today. To preserve open-mindedness while teaching current systems is another unsolved problem of education.

A final word on creative imagination. Besides the intellectual factors, certain emotional ones are demanded. The unconscious work goes on only over problems that are important to the waking mind, only when the mind's possessor worries about them, only when he cares, passionately. As Pavlov wrote shortly before his death at 87, advising young men on the requisites for effective pursuit of science: "Third, Passion. Remember that science demands from a man all his life. If you had two lives that would not be enough for you. Be passionate in your work and your searchings." This is related to the conscious work recognized by Poincaré as preceding the unconscious work of imagination; another emotional factor is involved with the second period of conscious work which follows: courage. It takes courage to face the unfamiliar, to espouse the different; courage to fight one's own prejudices only less than those of others. Was it not a little child who first dared call the emperor naked? It took great fortitude for Kepler to adhere to his new notion of infinity (as the second focus of a parabola), for, as he said, "The idea seems absurd, but I can find no flaw in it"; just as it did for Galileo to murmur among his inquisitors, "Yet the world does move." Most of us will never achieve great imaginative insights; we might at least attempt to be tolerant of those offered us by others.

Somehow, "this power of human thinking . . . seems in times of emergency or conflict to leap ahead to new truth" (Dummer). Sometime, when research in this "constructive power of the unconscious" has increased our understanding of insight, man will more effectively guide his onward movement.

SCIENCE AND WORLD COMMUNITY

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H. G. Wells once remarked that the fate of the world depends upon a race between education and catastrophe; but in view of the atomic bomb, he has more recently announced that catastrophe is obviously winning, since science and technology are developing more rapidly than man's ability to control them. Others, even more alarmist, have predicted that atomic fission will ultimately set off a chain reaction which will explode the entire planet.

Possibly we can dismiss this particular nightmare but we cannot easily dismiss this *type* of nightmare. Instead of predicting the total destruction of our planet, we may, like Wells, speak of the leveling of civilization in a vast chaos. All that we cherish may be destroyed because man's capacity to make scientific discoveries outstrips his capacity to control them.

Looked at in this context, the question what men shall believe acquires an incredible poignancy. Now as never before we must go back to fundamental questions. What is man—a soul, a mechanism, a psychophysical organism? What is man's place in the cosmos? What is the essential value of human life? What is the nature of the good society? What is the essence and destiny of civilization? Every struggle loses its meaning, every social movement its orientation, unless these questions be faced. Without a world view, strong enough to be the basis of civilization, we cannot substitute reintegration for the disintegration which has been at work for several centuries and which has led great nations into moral and intellectual barbarism. Without a different *sort* of world view than has been prevalent, we cannot make the arts of life prevail over the techniques of death.

It is possible that we shall have to go down to the very bottom of the pit where all faith and reason are lost. But it is a steadying thought that science has vast potentialities for good as well as for ill. Pure as well as applied science can help steady our nerves by giving us a new interpretation of the nature of things.

Apart from its technological applications, science has contributed in two essential respects to the establishment of a better type of civilization: first, in its content and, second, in its method. In content, science is providing a new view of the world and man's place in it, a realistic vision which emphasizes the interrelatedness of things and which thereby reinforces the cooperative ideal of human life. In method, science involves a broad community of interpretation, a world-wide fellowship of truth seekers; and it thus prefigures the cooperative organization of mankind. Its proper utilization, moreover, requires a gigantic effort at synthesis, so that the vast and ever-accumulating mass of scientific information can be integrated into a unified interpretation of reality.

As we study contemporary science and the philosophy based upon it, we begin to see an emerging synthesis of thought and action, a possible new basis of civilization. The core of this synthesis is a world view based upon two ideas. First, the world is a complex of interacting processes—everything is relational—nothing exists in bare abstractness, parted off from the rest of existence. Second, the world as evolving has various levels with distinct, irreducible

characteristics, such as the mechanical, the organic, the conscious. These two ideas coalesce into a single concept: the evolving multidimensional community of all human beings and, in an infinitely wider compass, of all the interacting entities in the universe.

Admittedly, we are here discussing not the technical content but the ideological implications of science. The two must be distinguished. For example, we can distinguish between the technical content of Newton's astrophysics and the ideological implications which Locke, Pope, Voltaire, the French Physiocrats, and Newton himself read into it. Society will always discover such implications, and we need not regret the fact. Certainly science is a better foundation than superstition for a comprehensive interpretation of reality. Admitting that "the new world view" which I shall discuss contains speculative elements, I believe that it involves no such misreading of science as the supposed implications of Newtonian physics to which I have just referred.

As far back as the Victorian period, the discovery by Maxwell and Faraday of the electromagnetic field represented a fundamental departure from the older physical notions of relatively discrete unchanging particles and external relations. Subsequently, physical scientists have built up a new type of "field physics" in which atoms or electrons are conceived to be interdependent events functionally dependent upon their spatio-temporal environment. As a result of the new concepts of space-time, of relativity, of quantum and nuclear physics, of the time and space rate of energy, of Heisenberg's principle of indeterminancy, the universe depicted by physical science has become radically different from the "world machine" of Galileo and Newton. This difference primarily involves the increased recognition of wholes and integral processes. The region, the context, the given totality, the space-time configuration, the emerging levels of organization have taken the place of the old rigid atoms, the empty, featureless space, the static order of events, the mere positions without contextual reference.

Similarly in biology there has been an increasing emphasis upon integral part-whole relationships, upon the idea of the organism, in which the whole in a sense is prior to and determines its parts; the idea of the ecological community, in which organisms socially live and function; and the idea of emergent stages in evolution, in which there are real creative syntheses and not mere additive resultants. Psychology has been slower in responding to formism, but even here there has been an increasing tendency (represented by many besides the gestalt psychologists) to think in terms of organic wholes and structural relationships. Likewise in the social sciences there has been a general revolt against discrete individualism and a marked tendency to think in terms of interdependence. Modern history and anthropology, moreover, have made us aware that our competitive economic order is only one among many historical systems, and that consequently our system is no more natural or immutable than very different cooperative systems, which similarly arise and perish. Everywhere in science the old fixed concepts and counters have broken down and a new world view is emerging.

If we analyze what is involved in this world view, we find four essential propositions. First, the universe exhibits a series of levels of increasing complexity and wider integration: electrons, atoms, molecules, simple cells, plants, animals, personalities, human communities. Second, there is a tendency for these levels to succeed each other in time, the more inclusive and complex levels emerging at a later stage of cosmic development;

and hence reality is dynamic and evolutionary. Third, the integration also occurs as morphological envelopes in space: societies enveloping societies which in turn envelop societies. For example, the body envelops the organ, the organ the cell, the cell the molecule, the molecule the atom, and the atom the electron. Fourth, each envelope and integrative level exhibits its own emergent qualities and laws. This means that every science has its distinctive methods and subject matter, corresponding to a certain level of emergence, so that it is a false reduction to maintain that sociology is "nothing but" psychology, or that psychology is "nothing but" physiology, or that physiology is "nothing but" chemistry, etc.

It follows from these propositions that idealism—the theory that all reality is mental—is improbable, since mind apparently emerges out of, and is dependent upon, a high level of biological complexity. It also follows that any *reductive* mechanism or materialism—the theory that all reality is merely physical—must be rejected, since biological, mental, and social qualities, with the values and purposes to be found at the higher levels, are quite as indefeasibly real, although not as widely dispersed, as any chemical and physical processes. Thus both a *leveling up* and a *leveling down* interpretation of science and reality must be abandoned in favor of a theory of *distinct, graded, and progressive levels.* Finally, it follows that the world is to be interpreted in terms of a flexible organicism, since nature as a whole exhibits a nisus toward social organization.

A possible objection to the above interpretation is that discrete mechanism never has existed in pure science, and that real scientists have always taken account of systematic wholes, interactive elements, and emergent characteristics. Professor Gustav Bergmann, one of the ablest of the logical positivists, for example, objects to "the exaggerated importance" that the gestalt psychologists such as Wertheimer attach to the idea that a composite effect is not the additive sum of partial effects. He writes:

> What Wertheimer and his students so emphatically assert is essentially this. If C_1 causes E_1 and C_2 causes E_2, then the simultaneous occurrence of C_1 and C_2 does not as a rule cause the occurrence of E_1 and E_2. . . . To give an illustration which is drastic but, I believe, not unfair: a dog which has been appropriately trained runs away from his master when shown a stick and approaches a piece of food held out to him. If both stick and reward are shown to the animal, will it both flee and approach his master? Obviously not, but whoever said it would? . . . There is never any a priori reason to assume that the joint effect will be the logical conjunction of the partial effects. If addivity is the name for such unjustified inference then all empirical laws, whether elementaristic or organismic, are superadditive, and the thesis fights a straw man (Holism, Historicism, and Emergence. *Philosophy of Science,* Vol. II, 217, October 1944).

Such criticism is extremely valuable in forcing scientists and philosophers more carefully to define their concepts, but it does not—nor was it intended to—disprove the fact that a fundamental shift in the ideology of science is occurring. I have already distinguished between the *ideology* of science and the *technical content* of scientific laws. Granted that science in its laws has always taken account of the superadditive composition of forces, it remains nonetheless true that ideologically we are shifting to a greater and greater emphasis upon form, pattern, creative synthesis, communal relationships, and total configurations. Even in its strictly technical content, science has been affected by, and has participated in, this shift. The insight that the influence of the whole must be kept in mind in order to explain the character and activity of any actual part has had an increasing effect upon both laymen and scientists,

and consequently such fictitious entities as rigid atoms and completely egoistic human beings have been banished to the limbo of superstition. The extent of this shift can be exaggerated, but the shift is indubitable.

Many laymen and some few scientists, however, still interpret plant and animal life almost exclusively in terms of an individualistic struggle for survival. This tendency goes back to fundamental Darwinian concepts: "chance variation," "natural selection," and "survival of the fittest." One of the classic, although inaccurate, interpretations of Darwinism is Thomas Henry Huxley's essay *Evolution and Ethics,* in which "the gladiatorial theory of existence" is presented in terrifying terms. The law of nature, he tells us, is "ruthless self assertion," the "thrusting aside or treading down of all competitors." In this process, the strong and aggressive trample down the weak, and in this sense only do the "fit" survive.

The ethical reactions to this theory differ according to individual predilections. Huxley, for example, believed that man's duty is to maintain, with lonely and pathetic idealism, a Promethean defiance of nature and of nature's laws. "The progress of society," he declared, "depends not on imitating the cosmic process, still less in running away from it, but in combatting it." The more common view was that of Herbert Spencer, who argued that if human beings break with nature, nature will break them. Darwinian concepts, when applied to society, were almost always used to support individualism and predatory behavior: to oppose social humanitarianism and to rationalize war, imperialism, and exploitative capitalism. Even in very recent years, the same point of view has been maintained by Oswald Spengler, with his theory that man is "a beast of prey," and by the fascists, who adapted this doctrine to their own purposes. Hitler declared: "The whole of nature is a continuous struggle between strength and weakness, an eternal victory of the strong over the weak. . . . There is no humanitarianism but only an eternal struggle, a struggle which is the prerequisite for the development of all humanity" (*Hitler's Words.* Washington: American Council on Public Affairs. 3–5. 1944). In less exaggerated form, the same doctrine has become part of the folklore of American capitalism. "You can't make the world all planned and soft," says the typical businessman of Middletown. "The strongest and best survive—that's the law of nature after all—always has been and always will be" (R. S. and H. M. Lynd. *Middletown in Transition.* 500. New York. 1937).

Since the time of Darwin, however, a vast amount of evidence has been amassed to show that not only struggle but mutual aid is a major factor in evolution. A notable succession of books, such as Drummond's *Ascent of Man,* Cresson's *L'Espèce et son Serviteur,* Kropotkin's *Mutual Aid,* Thomson and Geddes' *Life: Outlines of General Biology,* and Conklin's *The Direction of Human Evolution,* have maintained that parental care, kin sympathy, altruism, and mutuality are potent factors in survival and evolutionary development.

In an article which reviews the evidence, Dr. W. C. Allee, a distinguished American biologist, concludes:

> The picture that emerges from the cumulative studies on social biology is one in which cooperations and their opposite, disoperations, both exist. There are both egoistic and altruistic forces in nature, and both are important. The question arises insistently as to which of these is the more fundamental and potent. . . . After much consideration, it is my mature conclusion, contrary to Herbert Spencer, that the cooperative forces are biologically the more important and vital. . . . Under many conditions, the cooperative forces lose. In the long run, however, the group-centered, more altruistic, drives are slightly

stronger. If cooperation had not been the stronger force, the more complicated animals, whether arthropods or vertebrates, could not have evolved from the simpler ones, and there would have been no men to worry each other with their distressing and biologically foolish wars (Where Angels Fear to Tread. *Science,* 97, 521. 1943).

Allee's careful conclusion is fairly typical of the opinion of contemporary biologists, who regard the views of Huxley, Spencer, Spengler, and Hitler as a one-sided exaggeration and caricature of nature. At the 1940 meeting of the American Association for the Advancement of Science in Philadelphia, there was a symposium on "Science and Ethics" in which some of America's most distinguished scientists, including a number of eminent biologists, agreed that human cooperation is a more important factor in survival than human competition. The more sophisticated biologists, moreover, realize that the life of man in society, although it necessarily rests upon a biological foundation, exhibits many cultural characteristics which cannot be reduced to merely biological terms. The direct study of human society by anthropologists and other social scientists has proven that "human nature" is remarkably plastic, and that the highly cooperative society of the Arapesh in New Guinea, for example, is quite as "natural" as the fiercely competitive society of their near neighbors, the Head-Hunters.

In view of the newer trends in both the physical and biological sciences, we can say that our tendencies toward community are as innate and as well founded in nature as any of our competitive drives; and that our problem is to keep competition in its proper place, directing it toward worthy ends and subordinating it to, or coordinating it with, the even more fundamental tendency toward organic and communal relationships. The most fruitful kind of competition, such as the friendly rivalry of scientists in the discovery of truth, is the sort that contributes to a common cause and that consequently is not an alternative to cooperation but an aspect of it.

The change in the ideology of science is correlated with a very profound transition in social thought and action. A new collectivistic order has been growing for many decades within the womb of the old order. We can see the signs of this growth in the social-mindedness of various modern thinkers—for example, in far-seeing economists such as Veblen and Keynes, in adventurous sociologists such as Geddes and Mannheim, in influential philosophers such as Dewey and Whitehead. We can see the same trend in the increasing emphasis upon planning, organization, and social control in all of the social sciences. We can discover the signs of transition, sometimes cruel and perverse, in the collectivist revolutions and political upheavals of the modern world.

After considering recent scientific and social tendencies, the English biologist, Joseph Needham, declares:

It may be that we are on the threshold of a long period, lasting perhaps for several centuries, in which the organic conception of the world will transform society, giving it a unity more comradely and equal than feudalism, but less chaotic and self-contradictory than the centuries of capitalist atomism (A Biologist's View of Whitehead. In A. Schilpp, Ed., *The Philosophy of Alfred North Whitehead.* 251. Evanston. 1941).

This very interesting sentence requires elucidation. Needham, I am sure, does not mean to imply that "the organic conception of the world" will, in and by itself, transform society. He elsewhere recognizes the decisive role played by economic causes in any such fundamental change. He means that the "organic conception" prefigures and facilitates this profound alteration.

Also he does not mean to imply that society is a giant organism, a kind of

monstrous leviathan with a soul of its own. The term "organic" is so loaded with biological connotations that we must use it with extreme caution in connecting it, as Needham does in this passage, with a human social organization. Society is to be conceived as an integrative structure the members of which are bound together by social relations which determine their essential personalities. Outside of these relations, they would be very different, if indeed they could even exist. Such "internal," or essential, relations, as opposed to merely external and inessential, are often called "organic," and there can be no valid objection to the term if the reader clearly understands that groups without being literally organisms may have a touch of the organic, some more and some less.

If we are not to misunderstand Needham and to misinterpret the nature of the new scientific "world view," we must recognize that "organic," as he and I employ the term, must be understood in terms of different grades and types of unity. First, a part may be related to a whole in such a manner that its normal characteristics and activities will remain virtually unchanged whether it is "in" its normal whole or not. An example of a part thus externally related to its "whole" is provided by the archaic conception of a rigid, unimpregnable atom grouped with like atoms in the extrinsic, indifferent receptacle of space and time. Any such whole is a mere aggregate, and no one would call the functional and existential *independence* of its part an "organic" relationshp. Second, a part may be related to a whole in such a manner that it undergoes an *internal* change, small or great, when it is isolated. Such *functional dependence,* which admits of infinite degrees from near independence to very close integration, can be called "organic" in the wide and sometimes metaphorical use of the term. Third, a part may be related to a whole in such a manner that it becomes quite unrecognizable, or cannot even exist, when isolated. This *existential dependence* can be called "organic" in the strictest sense of the term. Science is now recognizing that, although some wholes are mere heaps or collections in that they exhibit only the first type of unity, a vast number of wholes, and indeed such fundamental "kinds of things" as atoms, molecules, cells, organs, organisms, minds, and societies, can be fully described only in terms of the *second* and/or *third* types of unity. The increasing emphasis in science and philosophy upon functional and existential dependence constitutes a profound revolution in human thought; and it is this emphasis that I have in mind when I speak of the rise of "a new world view," and that Needham has in mind when he speaks of "the organic conception of the world" that modern science and philosophy have given us. (*Cf.* J. Needham. Integrative Levels: A Revaluation of the Idea of Progress. *Modern Quarterly,* Vol. I, 32–33, January 1938.)

He is surely right in prophesying a revolutionary change in human thought and institutions. We seem to be involved in the most rapid and profound change that humanity has ever experienced. Some kind of new order, therefore, is inevitable, whether for better or worse; but we can understand its necessities, appreciate its potentialities, and contribute to its values. If we are guided by understanding and motivated by good will, we can not only mitigate the cruelty of the transition but gradually build a humane and cooperative order. There is nothing, as we have seen, in our scientific understanding of man and his place in the universe that need discourage us in such an attempt. The ideal of a universal human fellowship is based upon the nature of things.

If we turn from the *content* to the *method* of science, we find that it involves, to employ the suggestive phrase of Josiah Royce, "a community of interpretation." To understand what is meant by this phrase, it will be helpful to analyze the nature of ordinary thought.

If we attend to any immediate object of awareness, to *this* patch of color, *this* rustle of leaves, *this* twinge of pain, it is always a particular and private datum: a different impression than appears at any other moment or to any other man. As soon as we *think* about the object, however, we relate it to various other objects: we connect the immediately given with the non-given. "This moment" is identifiable as *this* moment only because it stands in contrast to a moment ago which I now remember and the next moment which I anticipate. "This room" means "this" to me only because I think of some other room, and it means "room" to me only because I think of the house and of the fact that this is *a* room: an example of a class, a particular instance of a universal. To sever all such connections would be to render experience meaningless, to eliminate experience at the *human* level of insight. All our thinking, as Kant long ago demonstrated, is thus contextualistic: every object has meaning only through its social relations with other objects.

The context within which an object acquires meaning, moreover, is not merely the individual's private experience but the shared experience of a group of people. Despite the fact that individuals see objects in egocentric perspective, they interpret the objects as the "same" for all perceivers. At dinnertime, you and the other members of your family, for example, believe that you are in the same room and are eating at the same table, and the stubborn privacy of each of your immediate sensations does not prevent the subconscious creation of this community of interpretation. The ordinary objects of touch and sight and sound are regarded as essentially common objects, the "same" for all who experience them, because all of us fit our experiences into the wider social context of group experience, interpreting our private data as "aspects" of a common world. Without collating our experiences with those of others, we could engage in no conversation, no intercourse, no communication of any meaningful kind with other human beings.

Without this social interchange and correlation of impressions, moreover, there can be no meaning attached to the "external world": the environment in which we all move, breathe, and have our being. As Royce pointed out, the only reason we have for supposing that the moon is not just a chimera of private perception, such as "the nonexistent color" seen by the color-blind person, is that others see the "same" bright disc and its effects: it is witnessed by the testimony of innumerable human beings and hence is part of the "real world."

Thus there is a close connection between the quest for truth and the creation of a community. This is the reason why, again and again in the history of human thought, the masters of truth have created a community of "seekers." Pythagoras, Plato, Epicurus, Jesus, Confucius, Buddha, Marx: these are only a few of the men that founded communities for the cultivation of insight. An integral part of the "Socratic method," for example, is inquiry in common among a group of men bound together by love of philosophy and love of each other. If it is no longer necessary for a modern thinker to create a community, it is because science and education as now organized are essentially communal: but the community of learning is as yet embryonic and needs to be nourished.

Science is essentially a community of

interpretation when elaborated systematically and critically. The effort of science is primarily *to connect:* it is concerned with the whence and the why and the whither of things; with the causal relations of an object with a great many other objects in the universe. In elaborating its web of causal connections, science makes use of hypotheses which become established theories when verified. The degree of verification increases when the theory unites a greater number of relatively independent data, when it is, in this manner, both coherent and comprehensive. Newton's mechanics gains immensely in cogency when it not only serves to unify the area of mechanics but the hitherto unrelated area of astronomy. Darwin's theory of organic evolution similarly acquires strength when it not only unifies the data of paleontology but the previously independent data of embryology. Thus the method of science, like the method of common sense, is *contextualistic:* it always seeks to establish interdependence between hitherto independent data; and it is a better theory if it does not sacrifice unity for diversity or diversity for unity. In this respect, science is like art: in both fields, greatness is the result of the most perfect reconciliation of "opposites": unity and variety, parsimony and adequacy. The very structure of science is a kind of organized "community" of facts: it is a form that is more inclusive without being less harmonious, a rich variety brought to a focus.

This method of contextualistic interpretation always involves reference to the perceptions and judgments of others. Science is the coordination of facts, and the very meaning of a fact is that it can meet the test of *social* verification. The distinguishing mark of scientific theory, moreover, is its public character, the result of a process of abstraction, generalization, and collective verification; because no belief has scientific validity so long as it remains private, the esoteric object of a single individual's perspective. It must be transmitted and interpreted to others and substantiated by them: it must be verified by stubborn and irreducible facts, admitted to be such by all qualified observers. It must, moreover, be consistent with established laws and theories, which in turn have been verified by scientists living and dead.

Because of the social nature of scientific method, every scientist must submit his personal observations and conclusions to the scientific community. For example, Professor Charles S. Minot, a distinguished anatomist of the last generation, concluded a series of lectures with these words:

> I do not wish to close without a few words of warning explanation. For the views which I have presented before you in this series of lectures, I personally am chiefly responsible. Science consists in the discoveries made by individuals, afterwards confirmed and correlated by others, so that they lose their personal character. You ought to know that the interpretations which I have offered you are still largely in the personal stage. Whether my colleagues will think that the body of conceptions which I have presented are fully justified or not, I cannot venture to say (quoted by J. Royce. *The Problem of Christianity,* Vol. II, 225. New York. 1913).

This statement is the mark of a scientific man.

Therefore science, like common sense, is essentially social in method: the difference being that science involves a much more elaborate correlation and comparison of social perceptions and judgments: a collective historic process knitting together many generations of all races and nationalities. The members of the scientific community are nurtured in a common tradition of thought: they are joined together not by force or herd-mindedness but by a common interest: the capacity of each for original thought and invention is a source of enrichment

to all. These are the marks of a very high type of community. Such unity-in-diversity, transcending the barriers of race, nation, and class, is the very prototype of the ideal community: the worldwide voluntary fellowship of mankind.

THE specialized and analytical character of most contemporary science, however, tends to counteract the communalizing and integrating effect of scientific method. So prodigious has become the accumulation of scientific data and research that it requires innumerable digests, abstracts, indexes, bibliographies, and bibliographies of bibliographies merely to keep some record of the ever-increasing mass of specialized research. Since life is too short to master more than a tiny fraction of this material, each scientist is inclined toward an ever-narrower specialization. Learning "more and more about less and less," he at last reaches the rather pitiful state of being unable to communicate with, or understand, his close professional colleagues. His "research" is understood by only a few initiates and is abacadabra to all other scientists, not to mention the general public. Digging away in his narrow vein, he may penetrate ever deeper into the mine of knowledge but he never comes up to the surface, to the point of vantage for a general survey. If not all scientists are to be characterized in these terms, at least far too many can be so described.

In consequence, the fast accumulating mass of information remains an uncoordinated aggregate, a chaos of "facts" rather than a unified body of culture. The layman, even more than the scientist, finds that the universe of knowledge has broken apart into a chaotic multiverse. In books, magazines, newspapers, and radio broadcasts he receives innumerable scraps of information about the nature of the world in which he lives; but the very multiplicity of facts that crowd in upon him serves to confuse him the more. His knowledge, in consequence, is as superficial and uncoordinated as it is diversified. He can no longer simply fall back upon scripture and immemorial tradition: the problems that confront him are too new and complex thus to be resolved; the transition from an old world to a new has gone too far. Lacking the traditional anchorage and any fresh grasp of the inner coherence of reality, he is an easy convert to irrationalist and antisocial ideologies, such as anti-Semitism. If we are to escape such intellectual and moral anarchy, we must somehow diffuse among both scientists and laymen an understanding of the ultimate and general frame of existence. Unless in thought we can master our world we shall not be able to control it. What is required, therefore, is a great work of synthesis and general education.

Maintaining that scientists must create "an orderly world mind" to supplant the present "world dementia," H. G. Wells has suggested various devices to facilitate this task of synthesis. He has re-emphasized the importance of an international language, such as Esperanto or Basic English, which, like Latin in the Middle Ages, would serve as a basis for the free international exchange of ideas. He has proposed a World Encyclopedia, not a mere mass of unrelated articles arranged in the specious order of the alphabet but a coherent expression of the spirit and best insight of the age. He has also suggested a great World Institute, which would prepare digests and syntheses based upon research and thinking everywhere and which would systematically serve as the "world's memory." Such an Institute would employ the most modern methods for the recording and dissemination of information, as for example microphotography, which enables the contents of a large

library to be condensed in a small box and inexpensively to be reproduced in various parts of the world.

Such proposals could prove extremely valuable, but less mechanical and more fundamental is a profound intellectual reorientation: "a unified approach to knowledge and life." The conception of knowledge as an organic unity, "a system consisting of parts, related to one another, and interpretative of one another and the unity of the whole," must penetrate all the levels of science and education. (*Cf.* L. Mumford. *The Unified Approach to Knowledge and Life.* Stanford. 1941.) As a distinguished English physicist, J. D. Bernal, remarks, specialization has been carried so far that there is

> an enormous lag in the appreciation of the relevance of one field of science to another. For instance, chemists for a quarter of a century have failed to recognize that advances in physics and crystallography require not merely the revision but the complete recasting of the fundamental structure of their science, nor have the mathematicians appreciated the extraordinarily rich fields offered to them in the recent studies of the development of organisms. One effect of this is to hold back science just at those very places where its advance is most needed: the regions between recognized sciences (*The Social Function of Science.* 114–115. New York. 1939).

Already the interpenetration of the sciences, however, is being more fully recognized, as for example in recent biochemistry, biophysics, psychosomatic medicine, social geography, and broad humanistic studies such as culture-history. In consequence of this increasing appreciation of the unity of knowledge, a considerable number of educational institutions are abandoning the chaotic "elective system," whereby bewildered students encounter a haphazard succession of isolated "subjects," and are substituting an integrated curriculum.

Although such cross-fertilization of the sciences and integration of educational courses are extremely valuable, they must ultimately depend upon a comprehensive and dynamic synthesis. A detailed synthesis, as contrasted with the brief sketch which I have included in the present article, is very difficult to achieve. If we are to have a reliable "world view," the known universe must be "our oyster." We must intellectually possess it; we must understand it entire. To achieve this grasp is the task of a Titan, beyond the compass of any living man. Aristotle in ancient Greece or Aquinas in the Middle Ages could sum up the total view, but the world as then understood was far more simple and synoptic. We must now rely not upon a single great interpreter but upon a community of interpreters. We must no more give up the search for unity of thought than did men in ancient times but we must realize that the task now devolves upon an organized band of truth seekers, supplementing and complementing and qualifying one another's vision.

Many scientists have cried out with alarm against the proposal that they work together in an organized way, as in large measure they have been doing in wartime. These protests are parallel to the many attacks upon planning in the economic and political sphere. They are based upon the false premise that freedom is inconsistent with organization. The fact that this premise is false we can see from many ordinary activities, such as a public dance, a baseball game, or an orchestral concert—from any cooperative activity in which order and individual expression are reconciled.

There is danger, of course, that men of thought will be tragically constricted by the forms of social organization: certainly this has occurred to a terrifying degree in certain parts of the world. The remedy is not intellectual anarchy because it brings the very tyranny to which we object: the real remedy is the interpenetration of so-called opposites,

the achievement of free organization and organized freedom; not the buckling of minds to a preordained plan, but the creation of a plan by the free interaction of minds; in short, the attainment of a *free* community of interpretation. Such a community will not be achieved and preserved unless it is an integral objective of the plan. As Karl Mannheim has pointed out, freedom in a planned society means freedom actually provided within the plan—the provision of mediums for the free expression and interaction of minds and for the democratic "planning" of the planners—the guarantee that the community of interpretation will be free and democratic because it rests upon a community of persuasion. The task is not easy, but upon it depends the hope of the human race.

I began with a note of pessimism. I end with a note of optimism. It seems unlikely that mankind could achieve the magnificent structure of science and yet could so wantonly misdirect it as to destroy the human race. The grandeur of the achievement is an earnest of its proper use. Yet salvation does not come easily, and the burden is laid upon us all.

THE ARGONAUTS

All night the astronomers at their telescopes
Explore the continents of the upper air,
Pursue the Lion down the western slopes,
Or up the east, in Aquila, Altair.
All night they wander on the mountainous moon,
And scale Copernicus or afar descry—
Piercing the blazing, breathless lunar noon—
The rim of Tycho sharp against the sky.

O epic journeys of the eye and mind
Whereon these heroes of our time embark!
They quit this earth and in the zenith find
A Golden Fleece that frees us from the dark:
Searching that center, that atomic core
Which, once controlled, will make us kings, and more!

HAROLD LEWIS COOK

WHERE ARE AMERICA'S RESEARCH RESOURCES?

By ROBERT B. DOWNS
UNIVERSITY OF ILLINOIS LIBRARY

In his report to the President of July 1945, Vannevar Bush, Director of the Office of Scientific Research and Development, states that "adequate technical libraries are an indispensable tool for research workers." The second World War emphasized Dr. Bush's conclusion to a degree never before realized, as scientists drew heavily upon the world's accumulated knowledge to furnish a basis for further research and progress. Scholars in the humanities and the social sciences have, of course, long recognized the fundamental importance of library facilities for their activities. The extent and distribution of American library resources, with which the present discussion is principally concerned, are therefore of vital interest to investigators in every major field.

For background purposes one might first look at library resources in general. Several years ago, in connection with a study of American union catalogs, a detailed statistical analysis was made of world book production from the beginning of printing through 1940. This work supplemented that of M. B. Iwinski, published in 1911. Iwinski calculated that to the end of the year 1908 there were 10,378,365 books in existence. This figure was based upon a careful examination of bibliographical and publication records. Unfortunately, statistics are lacking for certain countries and for certain periods; for example, book production figures for China are unknown. Another difficulty is the lack of agreement between countries as to what constitutes a book. National statistics sometimes include, sometimes omit, pamphlets and other lesser materials. Annual statistics compiled for the United States by the *Publishers' Weekly* are generally considered authoritative but are approximately tripled by the United States Copyright Office's reports. Government publications are excluded from nearly all statistics of national book production, and in most countries, as in our own, the government printing office is by far the largest individual publishing organization.

These facts illustrate the difficulty of arriving at any acceptable figures. Taking the Iwinski estimate as a base, however, LeRoy C. Merritt, an investigator for the union catalog survey, carried the study forward. The determination was reached that the total book production for those countries and those periods for which data are available is 15,377,276 titles. The total is 5,000,000 more than Iwinski counted through 1908, and represents an average annual world book output of 156,000 titles during each of the 32 years from 1908 to 1940. No claim is made for the complete accuracy of these figures, though they are the closest we have. It is astounding to observe that nearly one-third as many books were published in those 32 years as in the previous 450 years.

Assuming that approximately 15 million books can be accepted as the total number printed since the invention of printing, what proportion of the world's literature is now held by American libraries? To ascertain this fact Mr. Merritt sampled the national union catalog in the library of Congress, plus various regional union catalogs and the catalogs of about 100 large individual

libraries. On the basis of the sampling it was estimated there are 10,000,000 separate titles in American libraries, or, in other words, copies of about two-thirds of all books in existence are now available in the United States.

Coming next to distribution, where are the materials for library research in the United States? The distribution is extremely uneven, with the chief concentrations of American library resources to be found in the Northeast, the North-Central West, and the Pacific Southwest. By contrast the Southeast, Southwest, Rocky Mountain area, and Northwest are poorly equipped with research materials. A study of book distribution by states, which I made a decade ago, found the heaviest concentrations of books in New York, California, Massachusetts, and the District of Columbia, in that order, with Ohio, Illinois, and Pennsylvania trailing not far behind. The relative rank has remained unchanged. At the opposite end of the scale, the six states with fewest books are in the sparsely populated Northwest and Southwest, namely, North and South Dakota, Wyoming, Nevada, Idaho, and New Mexico. Near these are three Southern states, Arkansas, Florida, and Mississippi.

Some revealing figures on book distribution were supplied also by Louis R. Wilson. Dean Wilson found that there are 77 centers in the United States which contain, within a 50-mile radius, 500,000 volumes or more. Of these centers, New York, Washington, Boston, and Chicago possess the largest number of volumes. If a map of the United States were divided into four quarters, 57 of the 77 library centers would come within the Northeast quarter. A total of 113,000,000 volumes are concentrated there — about four and one-half times more than the other three quarters combined.

Another approach was made by W. W. Bishop, now Librarian-Emeritus of the University of Michigan. On the basis of their subject holdings, Dr. Bishop concluded that only three major collections of Americana in the form of books are to be found west of the Allegheny Mountains, and only two of the seven leading collections of English literature are outside the Northeast.

Wide publicity has been given by the American Library Association to a situation in which 40 or 50 million people of the United States are without access to public libraries, and another 30 million have inadequate facilities. Much less has been said about the scarcity of materials for advanced study and research in many parts of the country. The fact affects fewer people directly. Nevertheless, it is highly significant from the point of view of the progress and development of those areas. Unfortunately, also, inequalities in the distribution of library resources tend to be perpetuated. Recent figures show that college and university libraries in well-equipped library areas are spending twice as much money, in proportion to population, as the college and university libraries of poorly supplied regions. The gap between the two is therefore widening rather than narrowing—relatively the rich are getting richer and the poor are getting poorer.

A MAJOR aspect of the subject of library resources, and one which deserves separate consideration, is the distribution of materials among different kinds of libraries. The student of American library resources soon becomes aware of the many types of libraries to be found in the United States and of their varying facilities for research purposes.

Standing at the top of our system of libraries, from the point of view of advanced study and research, are the university libraries. The most important and most obvious characteristic of a university, distinguishing it from other educational institutions, is its research

program. Naturally the library of the university is expected to support this program. Using quantitative standards only, 11 American university libraries now hold more than 1,000,000 volumes each. The largest, Harvard, possesses over 4,500,000; the second, Yale, nearly 3,500,000; and Columbia and Illinois contain approximately 2,000,000 volumes each. In addition to these 11 major collections, there are 19 other university libraries with collections ranging from 500,000 to 1,000,000 volumes. Perhaps 20 more have resources that are significant from a research standpoint. In the United States we have, therefore, some 50 university libraries whose collections are of considerable importance to the scholar and research worker.

Closely related to the university library, but of comparatively slight importance for advanced study and investigation, is the college library. Few college libraries have the funds, the incentive, or the need for developing such research materials as are found in the university. Some of them, however, have built up special collections of more than ordinary value. Especially is this true of the New England colleges such as Amherst, Bowdoin, Colby, Dartmouth, Wesleyan, and Wellesley. A few libraries elsewhere—Oberlin in Ohio and Claremont in California, for example—have done likewise. Research resources in collegiate libraries are usually limited, however, to one or two specialized fields, and there are probably not over 10 such institutions in the United States of genuine significance from the scholar's point of view.

Another large group, numerically, are the public libraries, found in every city and town of substantial size in the country. Eight public libraries own over 1,000,000 volumes each, and 16 others run from 500,000 to 1,000,000 volumes. With perhaps two exceptions, these are of far less importance than one would assume from their size. The principal reasons for this fact are that public libraries must serve the needs of large numbers of general readers, who require less specialized material than university professors and graduate students; and, second, the public library must do an excessive amount of duplicating for its branches and to meet the demands of the large clientele served. An instance of the result is the Chicago Public Library, which in 1939 was found to have 1,718,867 volumes representing only 140,000 titles. By comparison, the Ohio State University Library, owning 496,806 volumes—not much more than one-fourth as many as Chicago—had 330,927 titles, more than twice as many. The qualitative difference for research purposes, if measurable, would doubtless have been equally wide.

The two exceptions to the statement about the research importance of public libraries are the New York and Boston public libraries, both quite untypical of the group. The New York Public Library is one of the world's great libraries, holding over 4,000,000 volumes and possessing rich collections in many fields. Similarly, the Boston Public Library has been growing over a long period, is well endowed, and has collections of major importance in a number of divisions. A few other public libraries have outstanding special collections, e.g., the White Folklore Collection in the Cleveland Public Library and the Burton Collection of Midwestern History in the Detroit Public Library. Aside from a limited number of such instances, public library collections are only of local significance.

A fourth group of considerable size are the state libraries. The first state libraries to be established in the United States were those of New Jersey and Pennsylvania in 1796. Subsequently, such libraries have been organized in every state. Many of the states have also set up library extension agencies, legislative reference libraries, state law

or supreme court libraries, state archives, and historical commissions. On the whole, state libraries have not lived up to the high expectations with which they were founded. It has been a common spectacle for them to be regarded as political plums, and, with a few notable exceptions, they have never had the financial or other forms of support necessary for the development of important research collections. The chief types of material of a research nature usually held by state libraries are local newspapers and state public documents. By law, it is the general practice for these libraries to have the exchange privilege for publications of the state government, and they automatically acquire extensive collections of publications of other states. If there is a supreme court library, as is frequently the case, that institution is ordinarily given exchange rights for session laws and court reports, thereby accumulating sizable law collections.

One of the important types of state agency is the organization to preserve archival records, known variously as the state department of archives, state historical commission, or state historical society. This division is given legal responsibility for saving the state's primary records—executive, legislative, and judicial archives, historical manuscripts, and other nonpublished documents. From the viewpoint of state and local history, these collections have assumed increasing importance in recent years. Most of them, unfortunately, do not cover early periods adequately. Among the reasons is neglect: until a generation ago in only a few states was there any genuine interest in preserving archives, and older records were frequently discarded as of no value. Another major cause of destruction has been the numerous fires which have burned down capital buildings in nearly every state, taking with them the accumulated official records. In the Southern states, some older files were lost during the Civil War years.

In summary, the state libraries, state archives, and state law libraries are important for a limited number of fields and types of material, chiefly for newspapers, public documents, law, and local history. They are not significant for research activities of a more comprehensive nature.

A major group of libraries which have grown rapidly in the past generation and are assuming increasing importance in many fields are the libraries associated with the Federal government. Standing at the top of the system, of course, is the Library of Congress, holding notable collections in many subjects, but especially outstanding for the social sciences, history, fine arts, and music. The Library of Congress is now the largest library in the world by a considerable margin. In addition to the national library, which the Library of Congress is for all practical purposes, virtually every government department and bureau has a library of its own pertaining to its specialized interests. Among these are some of the world's leading libraries in their fields: the Army Medical Library contains the largest collection of medical literature in the world; the Office of Education has the largest collection on education; the Department of Agriculture, the largest agricultural collection; the Army War College, the second largest collection on military science; the Geological Survey, the largest geological collection in the United States; the Labor Department, the most complete library in America on labor and social welfare; and there are dozens of other federal libraries of similar rank connected with various branches of the government. Among the reasons for the rapid development of the Federal library system are the generous financial support the libraries have received, their devotion to highly specialized fields, and

their favorable position as part of the national government. In exchanges with foreign countries, for example, the libraries in the governmental organization receive publications, as a rule, before any other institutions in the country.

In addition to the types of libraries named previously, there exists a small but important group usually called "reference libraries," privately endowed and controlled and not ordinarily affiliated with any educational institution. Two of the principal examples are in Chicago: the John Crerar Library, specializing in science, technology, and the social sciences; and the Newberry Library, which is restricted to literature, history, and the arts. Famous examples in the East are the J. Pierpont Morgan Library in New York City, the John Carter Brown Library in Rhode Island, the American Antiquarian Society in Massachusetts, the Folger Shakespeare Library in Washington, and the Peabody Institute in Baltimore. Another great library of the type is the Huntington Library in California, rich in English literature, early printing, and other fields. None of these libraries is large relatively, but from the point of view of the rarity and value of their collections they rank at the top.

A seventh category of libraries which have significant resources for research is a very heterogeneous lot. The term "special libraries" is generally applied to them. One of the largest divisions of this class is the libraries formed in connection with business and industry. Such libraries are usually gathered for the needs of persons actively engaged in the search for information useful in a business, a laboratory, a newspaper, etc. The whole emphasis is on current material and up-to-date information. Consequently, such libraries seldom contain much material of a historical nature, that is, the retrospective or background literature. They are therefore of limited use to the scholar, regardless of the completeness of their files of recent publications. Business and industrial libraries are mainly concentrated, naturally, in highly industrialized areas of the country.

A second large division of special libraries are those owned by societies, associations, and similar organizations. In many cities, for example, bar associations have established law libraries, and medical associations have set up medical libraries for the use of their members. Some of these are among the foremost institutions of their kind; instances are the Association of the Bar of the City of New York, one of the largest law libraries; the New York Medical Academy Library, the second largest medical collection in the United States; and the Engineering Societies Library, maintained by four national engineering societies, one of the country's leading engineering libraries. Hundreds of other examples could be cited, some of only local interest, others of national importance. Ordinarily these collections are highly specialized and are therefore of first-rate value to research workers in the fields covered.

A final group of libraries which possess resources for research are private collections. Some of the most valuable books and manuscripts in the country are in the hands of private collectors. It is extremely difficult, however, to obtain information about them, they are seldom available to outside users, and are constantly changing, being broken up, for example, by auctions, disposal of estates after the deaths of owners, or by transfer to public institutions.

The eight types listed—university, college, public, state, Federal, reference, special, and private—include all the noteworthy research libraries in the United States. Excluding the private collections, whose number is legion, there are at least 12,000 libraries in the first seven

categories. Only a fraction of these, however, can be said to be of national importance for the scholar and research worker. Exactly how many is a debatable point, depending upon the definition used. In my estimation, there are as a maximum, 250 libraries in the country holding collections of more than local or regional significance. Other estimates have generously stretched the number to 500, far too high, and others have stated it to be around 50, probably too low.

UP TO this point, an attempt has been made to describe in broad terms the present status of American library resources. What of the future? Can we expect research libraries to continue to develop along traditional lines, or is there a likelihood of radical changes?

When microfilm came into general library use a decade ago, it was predicted by enthusiastic advocates that the new medium would revolutionize library methods, even leading us to discard the book in its orthodox form. Until now, it is hardly necessary to say, the prophecies have not been fulfilled, though microphotography has been accepted by librarians as an important auxiliary tool. More recently Fremont Rider's provocative and stimulating volume on microcards (*The Scholar and the Future of the Research Library*. New York: Hadham Press, 1944) has opened up vast prospects for the utilization of microprint techniques for reproducing and storing research materials. Vannevar Bush carries these ideas a step further by proposing the "memex," placing at the fingertips of the scholar and research worker whole libraries of microfilmed records, made instantly available by indexing mechanisms. The coordinator of American scientists' war efforts believes such a development is entirely feasible.

Less sensational in nature, but considered by many social-minded librarians as providing keys to a number of problems, is another contemporary movement, namely, a wide program of library cooperation. Among the manifestations, some of which have achieved marked success, are: the growth of bibliographical centers in Denver, Philadelphia, and Seattle; establishment of dozens of union catalogs, ranging from local to national levels; the growth of the interlibrary loan system; publication of numerous union lists and other records of library collections; agreements between libraries for dividing fields of collecting, that is, accepting certain fields for specialization; cooperative publishing enterprises, such as issuance of the Library of Congress catalog in book form; surveys of library resources for advanced study and research; plans for regional library development; central storage warehouses, cooperatively maintained by libraries; exchange systems for duplicates and other publications; and cooperative cataloging of material.

Space is lacking here to discuss the library cooperative movement in detail. Such efforts as those listed originated through a widespread interest in increasing the availability of our bibliographical resources, and that is their primary reason for being.

This discussion offers sufficient evidence to show the extent and variety of American library collections. These resources certainly exceed in quantity—and in many respects in quality—those of any other nation. We will continue to fall short of perfection, however, so long as one-third of all books in existence are not represented in the United States, or until we strengthen the research facilities of poorly equipped regions, or until we adopt further measures to speed the research worker's access to his materials. Without being overly optimistic, it can fairly be claimed that progress is being made in all these directions.

MEN, MASTODONS, AND MYTH

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For over a hundred years men have been describing the "coarse brown hair" of the American mastodon, and for over a hundred years the mastodon's hide and hair have been regarded by textbook writers as irrefutable proof of its recent existence. In the midst of this constant repetition of what, through the sheer prestige of age, has come to be accepted as undeniable fact, it has never been pointed out that American institutions of science do not possess the tangible evidence which alone could justify such wholehearted faith in the exact appearance of this long-vanished beast. In addition, it has not been sufficiently noted that the eyes of competent scientists have never beheld this phenomenal spectacle of remains surviving under completely adverse conditions. Furthermore, it has escaped attention that in the one instance in which hair was apparently noted by a reputable scientist that same scientist dismissed it as illusory and satisfactorily explained it.

It is true that some paleontologists have limited themselves to what they regard as a single reliable instance of hair preservation, but it is also true that with the passage of years one or two have quietly dropped the subject from their books. Nevertheless, the story persists and continues to be reprinted down to the present day. It has become, in fact, part of the folklore of paleontological science, flourishing with renewed vigor because of existing archeological interest in the exact time of extinction of the American mastodons.

Extenuating circumstances have admittedly contributed to our gullibility. It is known, for example, that the sloth has left undoubted and unquestioned remains of hide, hair, and bits of cartilage in the insulating dust of the dry American caves. Why, then, should the similar early reports upon the mastodon, long extant in the literature, be questioned? Like my colleagues in paleontology and archeology, I had accepted this impressively documented "fact" until quite recently. Then research on another subject happened to lead me back into the source material of the early nineteenth century. The results of this investigation have completely and thoroughly convinced me that neither "hide nor hair" of the American mastodon has been found under conditions which would justify belief in the authentic nature of such discoveries. Moreover, it is my considered judgment that there exists a completely adequate explanation of these finds—one partially supplied by a paleontologist of note—buried away in the literature and never adequately treated in the light of other, European, discoveries which were agitating the minds of naturalists during this period.

To explore the reasons for assuming this iconoclastic attitude, it will be necessary to examine our subject in minute detail. Perhaps the best method of approach will be to list all those finds where claims are made as to the survival of soft parts, including hair, of the American mastodon. Few would claim validity for all, and, oddly enough, it will appear as our investigation proceeds, that some, at least, of this material was more vigorously questioned in its own day than in modern times.

To pursue certain of these early accounts back into the early nineteenth or late eighteenth century is a tedious and

difficult task. Some, though deriving ultimately from a single source, have been widely scattered through numerous secondary sources from which the accounts have found their way into modern literature. Often the early references are vague, inadequate, or completely lacking. As a result, some detective skill has had to be exerted to discover whether finds reported in two different works actually were distinct discoveries or represented differing accounts of a single early find. The results of these labors, which I think are reasonably complete, have yielded the following items.

Rembrandt Peale, having given an extended account of the excavation of the bones of what we now know to have been a mastodon from a bog near Newburgh, N. Y., goes on to comment: "The only instance of hair being found with the remains of this animal, occurred in a morass belonging to Mr. A. Colden in the neighborhood where this skeleton (the Peale mastodon) was found. The hair was coarse. long and brown, a large mass of it together, and so rotten that, after a few days' exposure to the air, it fell into powder." (This find is sometimes directly ascribed to Peale's mastodon. It should be noted that it is distinct from this find, and that Peale does not claim to have seen the hair in question. Curiously enough, the only other material associated with these remains was a mastodon tooth, according to the account given by Dr. J. G. Graham. It seems strange that this hair, recovered at a depth of four or five feet should have outlasted every other portion of the animal but a single tooth!)

In 1805 Dr. B. S. Barton, writing of bones from Big Bone Lick, quotes from a letter written by John Bartram to James Logan. Bartram's letter gives an account of some "Shawanese" Indians who had brought to Pittsburgh an elephant tooth and a fragment of tusk of which they were attempting to dispose. Describing similar remains, the Indians mentioned a head with a long nose and a mouth on the underside. Though there are no data to show that the Indians specifically mentioned soft parts, this item has been repeated as proof of the survival of fleshy tissue about which the Indians could have known nothing.

In the same journal a year or so later the discovery of mastodon remains in Wythe County, Va., was reported by Barton. The information had been supplied him by Bishop James Madison, at that time president of William and Mary College in Virginia. This find, because of Bishop Madison's prominence, was widely quoted, and numerous lengthy accounts of it are available. One of the best is that to be found in John Godman's *American Natural History*.

In brief, the Bishop claimed, on the authority of gentlemen whose veracity could be relied upon, that amidst the bones of the Wythe mastodon, the stomach, containing a large amount of half-digested food, was discovered in a perfect state of preservation. Neither the bones nor the "stomach" survive. Robert Bakewell, in the first American edition of his *Introduction to Geology* (New Haven. 1829) added mention of flesh to the original version.

Another account, whose exact date I cannot verify, but which has been mentioned by Howorth, Blainville, and Hay, describes the discovery of mastodon remains in Posey County, Ind., in the depths of a sixty-foot well. One of the "curators of the library" at Vincennes, Ind., stated that some skin and hair were found with the specimen.

In 1839 there appeared in the *Philadelphia Presbyterian* of January 12 an account of a mastodon discovery in Missouri. The account was unsigned, but it is known that Albert Koch was the au-

thor. I quote that section pertaining to the soft parts:

> Also between the rocks that had sunk through the ashes was found large pieces of skin, that appeared like fresh tanned sole leather strongly impregnated with the ley from the ashes, and a great many of the sinews and arteries were plain to be seen on the earth and rocks, but in such a state as not to be moved, excepting in small pieces, the size of a hand which are now preserved in spirits.

It may be added that though this animal was later disposed of in Europe, we hear nothing further of arteries, sinews, and hide.

Following this time, fresh accounts of similar discoveries lapse out of the serious literature of science, though the old ones continue to be handed down. Subsequent discoveries of bones were made, but, strangely, hide and hair discoveries are no longer made, the last attempt at that sort of thing being, perhaps, a find at Paw Paw, Ill., recounted in 1914. Here, because of "certain streaks and mossy fibers," the excavators assumed that their specimen (*Elephas columbi*) had been clothed with hair.

Taken together, these accounts present a fairly formidable array of evidence, and it is easy to understand how they have been passed along through the textbooks of science. Yet before beginning our analysis of these discoveries, it would be well to bear the following facts in mind: (*1*) No person of real scientific repute ever saw these remains. At best the accounts have been repeated or retold by scientists. (*2*) The accounts proliferated during a period when the world of science was excited over the discovery of refrigerated remains of the mammoth *Elephas primigenius* in Siberia. This inevitably biased Cuvier, whose works were widely read and quoted both here and abroad, toward the acceptance of such accounts from the New World. (*3*) The stories are old. Though mastodon remains are still found in bog deposits in the Eastern United States, many years have passed since anyone has claimed the discovery of mastodon hair under such peculiar circumstances. This suggests, obviously, that a certain naive credulity and nationalistic enthusiasm have quietly faded out of American biology. A closer analysis will reveal these facts with greater clarity.

In analyzing these early reports it will be well to treat the more easily disposed of discoveries first and reserve the find at Colden's farm till the last. Colden's discovery is early enough to escape the charge of deliberate faking which can be leveled against certain later accounts. Second, it concerns the hair of the mastodon and more or less sets a pattern for later stories. In addition, it is the only find accepted as valid by some writers. If we can show cause for rejecting this particular discovery, the strongest link in the chain of accounts purporting to demonstrate the survival of hide and hair of the American mastodon will have been destroyed. No one has reason to attribute this find to attempts at conscious deception. Its 1800 dating is too early to have been influenced by European accounts of the hairy mammoth. The same cannot be said for certain later discoveries.

Much has been made of the Shawnee Indian account of a head with a long nose. It has been pointed out that the Indians, unacquainted with the Proboscidea in life, would have been incapable of imagining this feature of elephant anatomy unless it had, in reality, been observed in the flesh. (This view has also been expressed in regard to certain Indian legends I have discussed elsewhere.) Actually a perfectly rational explanation on other grounds is possible.

It is to be noted that the Shawnee spoke merely of a long nose. They did not say how long nor did they give any details or even claim it to have been of

fleshy consistency. If one does not allow preconceptions about elephant trunks to obscure the issue but, instead, considers the appearance of both mastodon and mammoth skulls as they would appear to the untutored eye when the tusks had dropped from their sockets, a simple interpretation of the Indian story becomes apparent. If one turns to a good photograph or drawing of the mammoth or mastodon skull as presented, say, in Osborn's *Age of Mammals* or the full-page plate in Warren's *Mastodon Giganteus,* it can be seen that the most reasonable view that one untutored in paleontology could adopt would be that the empty protruding twin tusk sockets overhanging the anterior portion of the mouth were nasal cavities. In comparison with ordinary animals they would constitute, even in the unfleshed skull, a "long nose." No amount of writing can demonstrate this as clearly as a reference to a good anatomical plate.

The Wythe County, Va., find, also recorded by Barton, was challenged and dismissed in its own day by several competent naturalists. Yet, perhaps because the details were supplied by a churchman of high standing and because corrections of error never receive as wide publicity as the original statements, this find is still occasionally referred to as valid.

Whether or not the account of the contents of the animal's stomach as being present is true need not concern us greatly. Twigs and similar plant remains may survive for long periods under bog conditions. In the light of the general inaccuracies accompanying the account, however, it is by no means unlikely that even this item is the result of misinterpretation of materials in the stratum where the animal lay. Our real concern is with the report that the stomach itself was present. Considering the fact that all the other soft parts had vanished—cartilage, hair, and hide—this preservation of so delicate an organ as the stomach ceases to be merely possible; it takes on an aspect of the miraculous. Nor was it long before the discovery was challenged.

A very competent and astute, though anonymous, reviewer treated the whole subject with vigor and thoroughness during the course of a review of the first American edition of Robert Bakewell's *Introduction to Geology.* Bakewell had collected and repeated in a rather uncritical fashion, stories of the type we have been examining. Some were obtained from Cuvier's *Ossemens Fossiles,* Cuvier himself being also somewhat credulous about items received from faraway America. Portions of this anonymous review are worth reproducing here:

> . . . we should not any the sooner believe that this was a stomach and that its possessor died in the act of digesting its food; still less that it belonged to the mastodon itself whose bones lay around. The idea of the stomach enduring the destroying action of long inhumation, which no other parts except the bones could withstand, not even the integument, cartilage nor tendon seems really too ridiculous to have ever been seriously related. . . .
>
> We are sorry to see such things in a work of this kind, many of the readers of which may be incapable of investigating the subjects themselves, and the wrong impressions which they get may not soon be corrected. We know not how it is, but really, naturalists over sea have the luck of knowing a great deal about the mastodon that we never dreamt of here.

Even prior to this critique, however, Dr. John Godman had given an extended account of the Wythe County find and recorded the fact that Bishop Madison later tried to correct the impression made by his ealier statement "acknowledging that his information was inaccurate, and his conclusions too hastily adopted." The find was also dismissed by James Hall, the great nineteenth-century geologist, as "altogether fabulous." Since, in spite of refutations, the story continues to float through the literature, let us finally reiterate: the supposed stomach does not survive; the bishop

who had the story from gentlemen who remain anonymous "recanted" from his previous position. Probably it is through the medium of *Ossemens Fossiles,* which ran through several editions, that certain of these stories have gained such currency in scientific circles that there is a continued tendency to accept them, with scant realization that they were challenged successfully even in their own day.

Before passing on to a discussion of the remaining finds, note should be made of factors which, both here and abroad, were contributing to a more credulous attitude on the part of public and scientists alike toward the sort of discoveries we have been discussing. The discovery of fossil elephants in the New World had greatly stimulated the birth throes of the young science of paleontology. A cluster of great names—Buffon, Cuvier, Jefferson—had focused attention upon the subject. Collections of fossil bones were gathered up, exhibited in private museums for a fee, and then taken abroad for exploitation. The great public museum of today did not exist. To maintain interest, the owners of such collections did not hesitate to embellish their more exciting specimens by appropriate and judicious use of items that would lend human interest. It is in such a light that we have to consider the discoveries and publications of such men as Albert Koch.

A find which immensely stimulated popular interest in fossil elephants and played directly into the hands of some of the less scrupulous collectors in America was the discovery of refrigerated mammoth remains in Siberia in 1799. There are earlier accounts of such remains in the literature, but this was the first find actually to reach a museum. Many pounds of hair were obtained which eventually reached a number of the European institutions. The first accounts appeared in 1807, and thereafter the news was widely disseminated and soon known in France, England, and America. A precedent had been established—a precedent for the discovery of elephant hide and hair in the New World. Moreover, the European scholars, such as Cuvier, were more inclined as a consequence to credit these tales when they appeared in a New World setting. The American Barnums who lived upon public interest were quick to see that such discoveries of hide and hair were a great stimulant to the popular imagination. It is in this light that we must consider the activities of Albert Koch. He was a public showman, and in spite of one or two attempts to clothe him in the tatters of scientific regalia, his career is not such as to justify confidence in his word upon the more spectacular phenomena of paleontology. There is more than a little imaginative literary flair in the account of "sinews and arteries . . . plain to be seen on the earth and rocks, but in such a state as not to be moved. . . ."

George Gaylord Simpson, who has made the most intensive study in existence of the beginnings of vertebrate paleontology in America, dismisses Koch with the incisive comment: "Koch's data and publications have little scientific value and hardly merit mention . . . unless possibly as comic relief." He is similarly castigated by Digby. The bones secured by Koch at his sites were exhibited in Europe in 1842 and later sold to European museums. Perhaps the best commentary on the whole affair lies in the fact that though the European museums had eagerly acquired mammoth hair from Siberia and would doubtless have been similarly anxious to acquire Koch's specimens, "preserved in spirits," we hear no more of the arteries, sinews, and skin fragments. They were the evanescent products of imagination —no more—yet Koch's appetite for such

creations is transparently indicative of the journalistic tastes of a vanished era.

The minor items which remain to be considered before turning back to the discovery on Colden's farm are not of a significant character. The find at Posey, Ind., of mastodon hide and hair in the depths of a sixty-foot well, the details vaguely given on the authority of "one of the curators of the library," has about the validity of common gossip. The material is not preserved, and it has been noted by the paleontologist Hay that a well of the depth given would in that region carry the remains into Iowan loess or Wisconsin drift, depending upon the location. This can be then, no argument for recency, but even under geological conditions of a more modern order, this vague tale, incapable of substantiation, is scarcely worth the effort to refute.

The 1914 discovery at Paw Paw, Ill., is obviously a case of confusing moss with hair—something any untutored excavator is apt to do.

We come now to the most important discovery of all—the find on Colden's farm. This discovery has been thought to answer requirements not met by the other more obscure and questionable tales. Alexander Colden was a simple farmer, not a showman like Koch. His discovery was reported by a gentleman country doctor with only the purest of scientific motives at heart—a man who stood to gain nothing by his account and who immediately lapses back again into rural obscurity, leaving no line in the histories of science. Yet he says: "In Orange County near Montgomery, New York a tooth (one of the grinders) and some hair, about three inches long were found by Mr. Alexander Colden four or five feet below the surface."

Dr. James Graham wrote these lines in 1800. He did not know of the discovery of frozen mammoth remains in Siberia. Yet he and Peale speak of the coarse brown hair which is later to grow so common in the literature and be described as dun-colored. Their reports are not to be dismissed as in the case of Koch. These men saw something, found something. On this point hinges the whole defense for the complete recency of the American mastodon. What was it that lay in the gloomy peat swamps of the New York woodlands which, when found, set men to talking of a beast vanished so recently he might almost have seen the Puritans land? It was, of course, hair—hair of the mastodon—the hair that is pictured in every restoration on museum walls. From that day to this no one has been able seriously to challenge the existence of that hair—no one, that is, but one man. He found the answer and promptly buried it again in the depths of a huge geological report.

James Hall was one of the first really scientific geologists America produced. He was an authority on the Paleozoic System of North America and probably one of the first men in America really experienced enough to pass an objective judgment on that queer brown hair from the backwood swamps. In a huge volume on the geology of New York State Hall ventured, in his typically restrained and quiet manner, a comment on a site of similar implications. In 1843 he wrote:

> In a small muck swamp in Stafford, Genesee County, a small molar tooth was found several years since. Its situation was beneath the muck, and upon a deposit of clay and sand. A large quantity of hair-like confervae, of a dun brown color, occurs in this locality; and so much does it resemble hair, that a close examination is required to satisfy one's self of its true nature.

The circumstances of this discovery are remarkably like those of a half-century before on Colden's farm. A solitary tooth is the only mastodon evidence; the entire body has disappeared, yet the hair remains. Furthermore, here again

is the dun-brown color. But this time a careful, trained scholar looks at it closely. It is confervae—stringy tough strands of algae from the New York swamps, and algae with a tendency to turn dun brown!

If James Hall had written out a discussion of the whole problem, it is probable we would not be arguing this point today. Hall, however, was engaged on weightier tasks. He had the whole geology of New York State on his hands. He recorded his own particular site and he settled its position in the literature once for all—it was never listed as another hair discovery! But once that was done, he forgot it in the immensities of his many volumes. He never turned to the subject again. He never discussed Colden's discovery. But Colden's find, through the medium of Dr. Graham's words, continues to pass down through the literature.

One other faint bit of light flickers upon Colden's swamp for a moment and aids in substantiating the judgment derived from Hall. Godman gives an account of a mastodon find in Orange County, N. Y., in 1817. The bones were found four feet below the surface of the peat, surrounded with "coarse vegetable stems and . . . broken films of confervae, like those of the Atlantic shore."

Professor S. L. Mitchell, the geologist, explored the site, recognized the confervae. The statement is made that the site had been worked some ten or more years previously. This would carry its dating approximately into the time the Colden farm discovery was made. It is possible that the site represented is the same. But at any rate we have brought into the very neighborhood of Colden's discovery, the demonstrated existence of filamentous confervae and shown it to be present at the levels from which the Colden discovery came. Alexander Colden, farmer, and James Graham, physician, were mistaken in what they thought was mastodon hair. But mistaken with them have been a large number of later scientists of better training who, partly influenced by Siberian discoveries, had come to accept the idea that finds of this sort might naturally be expected. The anatomist Warren, for example, is found using the Russian material as evidence that hair can be preserved in America. He skirts, however, the difference in latitude involved.

There remains, then, from all the spectacular stories of the hide and flesh of the American mastodon only a faint swamp effluvium, perhaps a hint of lingering bone marrow or the organic matter of the bones themselves in the encasing muck of ancient bogs. And even these faint smells are probably more suggestive of the general organic composition of the muck itself than of the bones. Nevertheless, we may record this type of organic survival as possible. But whatever the antiquity of the American mastodon, it is safe to say that "nor hide nor hair" of him remains. We assume that he may have been clothed with hair like his brother, the mammoth, but the dun brown in which he has masqueraded these many years is that of drying algae. It is not the coat of the vanished mastodon, nor does it prove his near-colonial survival.

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IMPROBABILITY

One ultramicrocosmic mote of dust
Among unnumbered cosmic galaxies
And one colloidal spawn from ancient seas,
One transient form from millions upward thrust
To last a twink, then sink into the crust
Of earth inanimate: what phantasies,
What paranoid and crass audacities,
For this minuscule midge to feel he must

Somehow endure, be central to some Plan,
The End for which the macrocosmic Whole
Was made . . . Dreamer and dunce, thy name is Man!
Life on the stars?—a doubtful guess, a droll
Desire; life after death?—a dubious hope
Born in the dark where humans blindly grope.

READ BAIN, 1946.

RESEARCH ON AGRICULTURAL PRODUCTS[1]

By T. L. SWENSON
WESTERN REGIONAL RESEARCH LABORATORY, ALBANY, CALIFORNIA[2]

AGRICULTURE in the broad sense may be defined as the science and art of producing plants and animals—that is, living systems—including vegetables, fruits, cereals, animals, and fiber crops. In other words, we are dealing with complete biological entities produced as a result of the intricate vital processes of reproduction and growth. When we conduct research on any phase of agriculture we are dealing with the phenomena of complicated living systems. Particularly when we attempt, by research, to understand and modify those systems and to adapt them to economic uses do we find their physical and chemical characteristics both interesting and complex.

Two examples of research in the Western Regional Laboratory will serve to illustrate the use of basic knowledge in the scientist's attempt to make something new and useful. I will first outline as briefly as possible the theoretical aspects of the work in each case and then show how the fundamental facts and theories are applied in a practical manner. One example is essentially biochemical; the other is essentially biophysical. The material for the former was supplied by H. P. Lundgren and for the latter by T. M. Shaw, who are engaged in research in those respective fields in the Western Regional Research Laboratory.

Approach to New Protein Fibers. The first example is concerned with one phase of our work on proteins, the chief objective of which is the production of fibers. Proteins are nitrogenous organic compounds with large, complex molecules. They are found in both vegetable and animal matter. Structurally, they belong to the important class of substances whose molecules consist of long chains formed by the union of certain atoms in amino acids; the result is a backbone structure of carbon and nitrogen atoms, to which amino acid residues are attached as side chains. These chains have two important properties. First, since atoms can rotate around single bonds, the chains can curl and fold up into a wide variety of configurations, and, second, since active groups are present on the side chains, the chains can interact. Such interaction can occur between segments of the same chain and between different chains. These properties, according to present theory, determine the state in which proteins are found in natural substances. For example, the chains may be held folded in the form of small corpuscles, as in the vegetable, egg, and blood proteins, or they may be found in extended forms, as in feathers, hides, hoofs, horns, wool, and silk. Now if it is possible to liberate these chains from their natural confinements and to preserve, of course, their long-chain structure, it may be possible to reconstruct them into fibers and films having desirable physical properties.

The properties of curling and interaction of chains are of practical importance and constitute the basis of the essential physical properties of rubbers, films, fibers, and plastics. Figure 1 shows a comparison of three types of long chains: the rubber chains, built up of units called isoprene units; the chains in

[1] Presented before the Utah State Chapter of Sigma Xi, March 1945.

[2] Bureau of Agricultural and Industrial Chemistry, Agricultural Research Administration, U. S. Department of Agriculture.

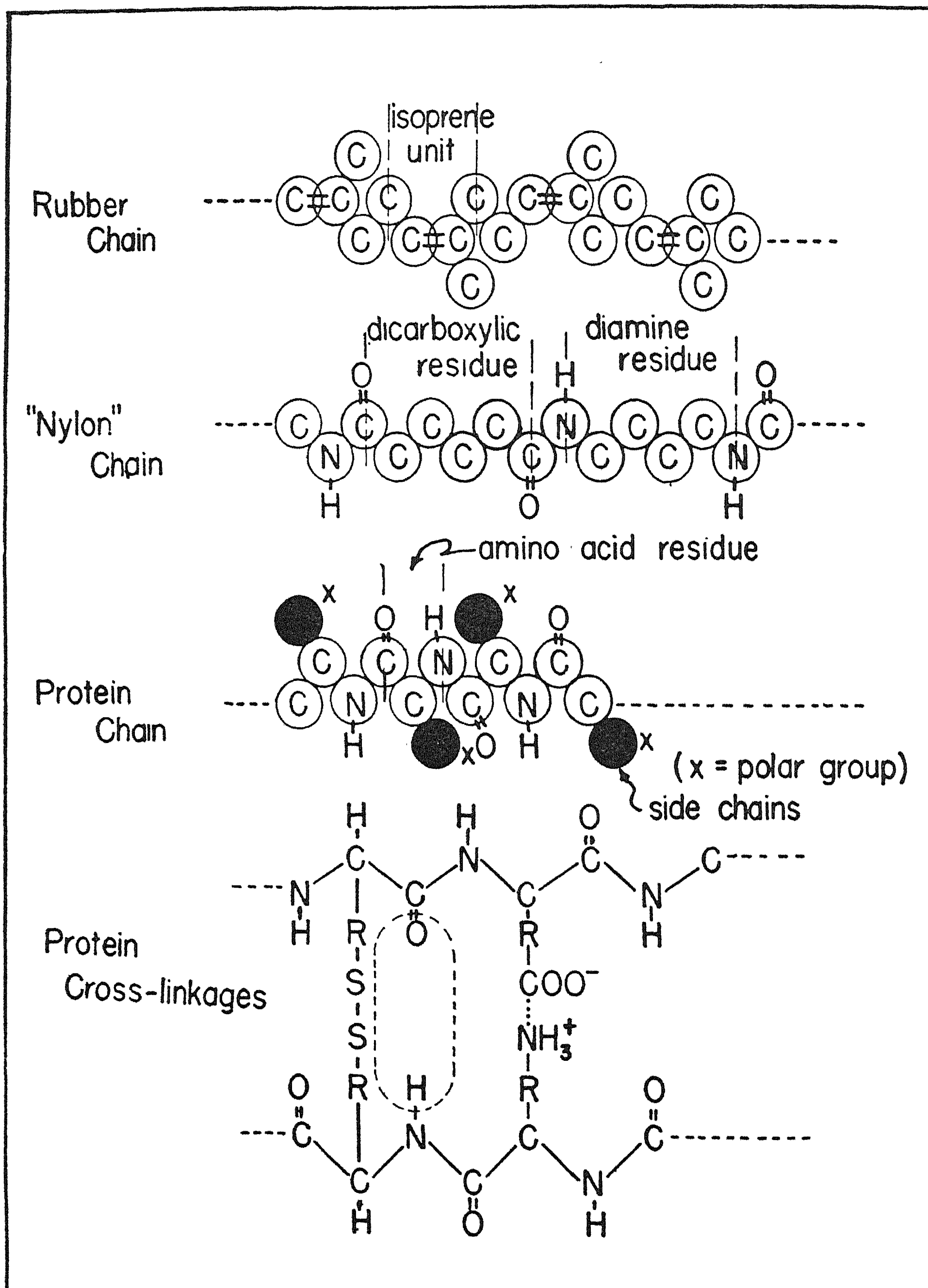


FIG. 1. THREE TYPES OF MOLECULAR CHAINS

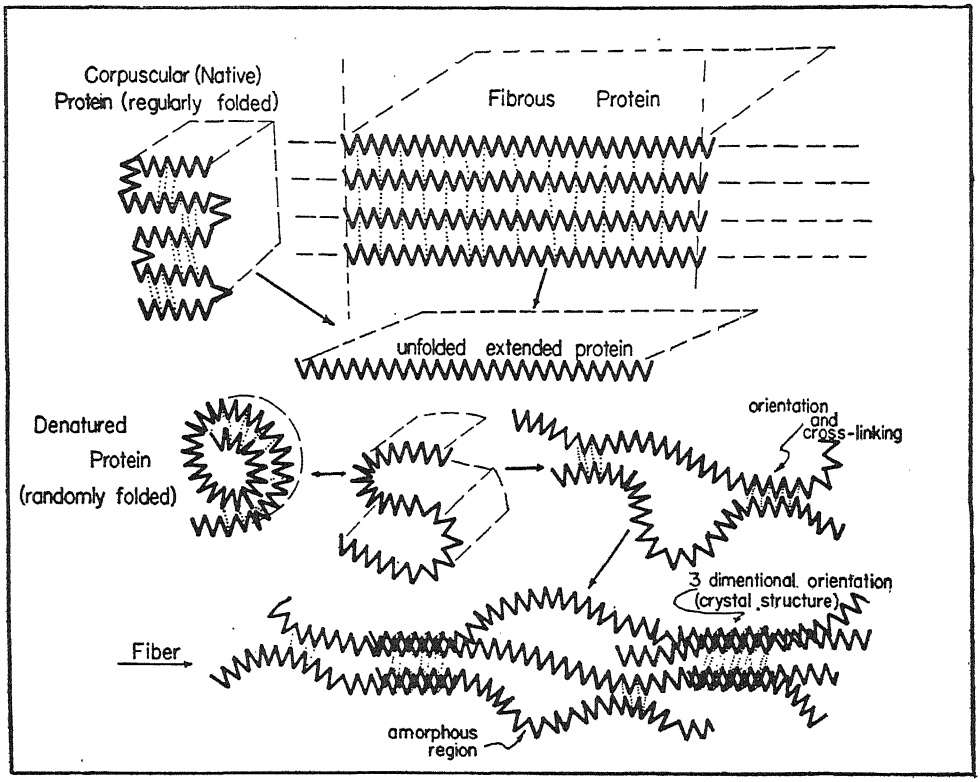


FIG. 2. THEORY OF REORIENTATION OF CHAINS

nylon, made by condensation of dibasic acids with diamines; and the protein chains, built from amino acids. Because the chains of soft rubber lack the ability to form inflexible unions, the material exhibits a high degree of flexibility—that is, curling and uncurling. When sulfur is introduced by the process commonly known as vulcanization, the chains become firmly tied together until, with sufficient sulfur, a hornlike plastic, or "hard rubber," is obtained. The proteins, too, contain sulfur, which in horn, hoof, feathers, and wool serves to tie the chains firmly together. Another type of linkage that may occur between chains is the so-called hydrogen bond, which is responsible for the strength of nylon and also that of rayon and cotton and seems to occur to a certain extent in the proteins. The so-called salt linkage occurs in proteins because there are both acidic and basic groups on the side chains along the backbone structure.

Information such as this is the basis for all fundamental work. In other words, the more detailed the information we have about a given material, the better are our chances of discovering new uses for it. As we have seen, the long-chain molecules form the basis for the essential physical properties of rubber, films, fibers, and plastics. Figure 2 suggests the structural transformations involved in the making of synthetic fibers from proteins. The main object is to liberate the chains from their natural confinements and to reconstruct them in endless chains tied together by strong crosslinks. With a corpuscular protein

as starting material, it is first necessary to break the bonds holding it in the folded configuration; with the fibrous proteins, such as feathers, the chains are already in an extended state and are held in three-dimensional networks. Obviously, any method of dispersion that involves the use of such agents as strong acids or bases must be avoided; if such agents were used, the chains might become degraded, and the ultimate properties of the fibers would be weakened.

The first step in the modification of a protein is its dispersion—that is, separation of its molecular chains in a solution. From there on, the process of making the fiber is essentially a duplication of the process used by the silkworm. The silk in the caterpillar is in the form of a viscous solution, consisting of the free protein chains. Upon extrusion through the small spinneret openings of the worm, the material congeals, the chains interact, and the fiber becomes at first highly elastic and rubber-like. The worm stretches the fiber as it spins, and the result is an alignment of the chain molecules, with further interaction. X-ray measurements show that the molecular chains in the fiber are aligned in the direction of the fiber axis; this condition permits maximum interaction of chains and high strength.

Various attempts to make fibers from proteins have been made during the past fifty years, some of which have succeeded within rather narrow limitations. Most of these attempts have been based on the solubility of proteins in alkalies and their insolubility in acids. Commercial casein and soybean fibers are now made by a procedure based on this fact, but they lack the strength of natural protein fibers such as wool and silk. Either the molecular chains in these proteins are not long enough, or else they become degraded in the procedure used in their manipulation.

Another approach to the making of fibers from protein is being studied at our laboratory. This method involves the use of special agents that are mild and that react with the protein to form a complex that is readily manipulated from a viscous solution to a rubber-like state and then to a fiber. Finally the agent is recovered, leaving the chains aligned in the direction of their length.

Agents that have been found most effective in our laboratory are certain synthetic detergents (widely used as cleansing solvents). Synthetic detergents are in general excellent solvents for protein. They will disperse feathers, wool, and hair in the presence of reducing agents (which prevent oxidation) and will disperse and unfold corpuscular proteins. Highly technical studies have shown that they form what we call "complexes" with the protein. These complexes form by virtue of the fact that all proteins have free basic and acidic groups along the chains which can react with an anionic or a cationic detergent to form salts. Accordingly, the first stage in the reaction between protein and detergent is stoichiometric; that is, the reaction involves one detergent ion for each basic or acidic group of the protein. When the stoichiometric reaction is complete, additional detergent ions can be attached along the unfolded protein chain. Under such conditions and in the presence of water, the protein chains can slip past one another easily. These complexes are readily precipitable by the use of salt, and the result is a rubber-like precipitate that can be stretched into a fiber. The spinning and stretching cause the alignment of the chains that is characteristic of natural fibers. Following this step, the detergent can be recovered with acetone.

This outline of the procedure which our protein chemists have developed shows that we do approach practical problems through knowledge of fundamental properties of our materials.

From information gained through the use of highly specialized physical and chemical techniques, we are approaching the time when it will be possible to predict whether or not a protein, or for that matter any other long-chain system, will be suitable as a raw material for fibers, plastics, rubber, or films.

Recent developments in long-chain chemistry have opened new vistas to the scientific mind; new fibers, which vary widely and have many useful properties, are within the range of possibility. We need only to look, for example, at rayon and nylon to see the possibilities that exist when appropriate means are available for the manipulation of long-chain molecules. We have rayon, nylon, vinyon, and glass fibers, but each has its own characteristic properties. Similarly, we have the natural fibers—each well adapted to certain uses. And we are attracted by the possibility of the discovery of artificial protein fibers that have new and special properties not found in any other fibers.

Theory of Dielectric Constants. The next example is concerned with a problem in biophysics, and it involves the theory of dielectric constants. Industrial research leans heavily on modern physics for new methods and tools in research. Among the more prominent physical tools the X-ray diffraction camera, the spectroscope, and the dielectric cell may be listed. The numerous applications of the X-ray camera and the spectroscope are widely known. The dielectric cell is not, however, so well known, and I shall review very briefly its theory and discuss some of its contributions and potential uses. The dielectric cell becomes an old friend of most of us when introduced by its more common name "condenser." Benjamin Franklin's famous Leyden jar of the lightning experiment was one of the earliest forms of condenser, or dielectric cell. In Figure 3 a Leyden jar is compared with a modern dielectric cell for liquid dielectrics. Both condensers contain the same basic elements, namely, two conductors and an insulating material called the "dielectric." Both cells store electrical energy, and this property, known as "capacitance," can easily be demonstrated by a simple experiment: first, we can charge the condenser by connecting the conductors to a battery; and, second, upon removal of the battery and short-circuiting the conductors, we can draw an electrical spark.

The importance to us of the dielectric cell is illustrated by an experimental observation of Michael Faraday (1791–1867). Faraday studied the variation in capacitance of a spherical condenser, consisting of two concentric spheres, when different substances were used to fill the space between the spheres. He found that all so-called insulating substances, or "dielectrics," gave the condenser a greater energy-storing capacity than it had when only air separated the conductors. This ability of a substance to enhance the capacity of a condenser is a fundamental property of matter, which we call the dielectric constant. Its magnitude varies from slightly more than 1 for gases to 80 for water and even larger values for certain mixtures.

Faraday explained the action of dielectrics in increasing capacity by assuming the presence of elementary electrical particles in the dielectrics which, although uncharged on the whole, had separate and opposite charges on the two ends. These particles, he said, might be created by the action of an electric force on neutral particles or they might result from some electrical peculiarity of the molecular structure. In the past 30 years physicists have shown that Faraday was right. Today we speak of induced and permanent dipoles, in correspondence with the two types of electrical particle assumed by Faraday.

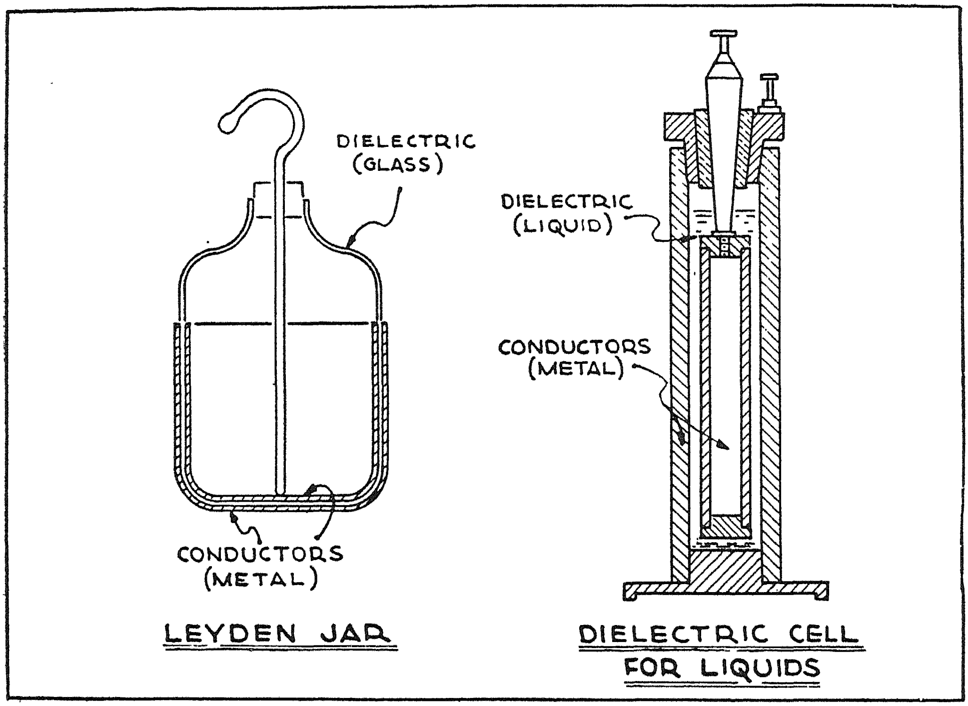


FIG. 3. A LEYDEN JAR AND A DIELECTRIC CELL FOR LIQUIDS

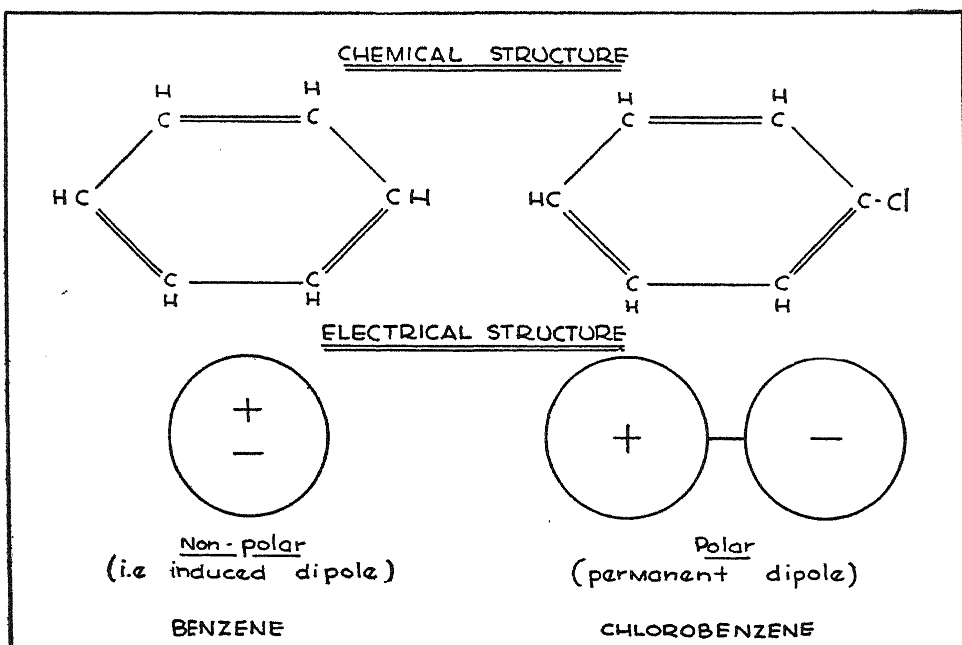


FIG. 4. EXAMPLES OF INDUCED AND PERMANENT DIPOLES

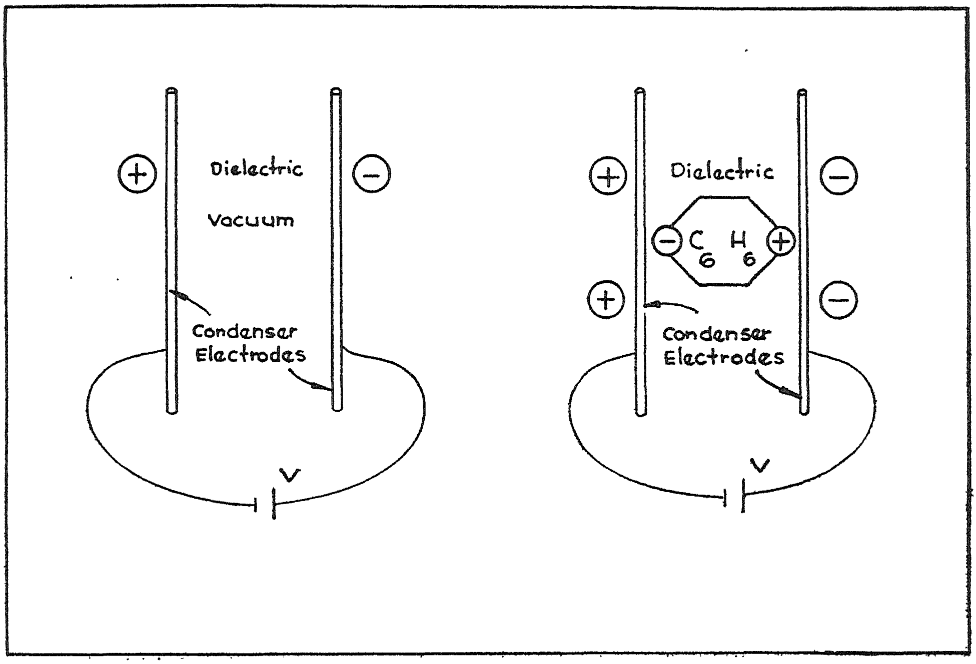


FIG. 5. WHY DIPOLES INCREASE CAPACITANCE

A schematic drawing of two typical electrical dipoles, benzene and chlorobenzene, is shown in Figure 4. Benzene is typical of so-called induced dipoles, or dipoles that exist only in the presence of an electric force. Such materials are electrically neutral until, in the presence of an electric field, the symmetrical distribution of positive and negative charges within the particle becomes disturbed and an induced dipole is formed. Chlorobenzene is typical of so-called permanent dipoles. Materials such as this are permanently unbalanced electrically, because the centers of population of the positive and negative charges in the molecule do not coincide.

The manner in which dipoles increase the capacitance of a condenser is illustrated in Figure 5, in which a condenser is shown at the left with one dielectric (vacuum) and at the right with another (benzene). We will assume that when the dielectric is vacuum the condenser is of such dimensions that one plus charge and one minus charge on the conductors is sufficient to charge the condenser to a given electrical potential in accordance with the relationship $C = Q/V$, where C is the capacitance, Q is the charge, and V is the potential.

Now consider the same condenser when benzene is the dielectric. Benzene, we know, has a dielectric constant of slightly more than 2, compared to a vacuum. Hence, the ability of a benzene-filled condenser to store electrical charges must be about twice that of the condenser with vacuum as the dielectric. As the illustration shows, this ability arises from the fact that the charges on the induced dipoles in the benzene effectively neutralize some of the charge on the condenser electrodes. As a result, the battery must furnish more charges to the electrodes in order to supply the excess of one plus and one minus charge, which we assumed were required to maintain the potential V.

Entirely similar considerations apply

to materials containing permanent dipoles, like chlorobenzene. In the absence of an electrical potential, the chlorobenzene is electrically neutral. Thermal motion of the dipoles causes a random distribution of the positively and negatively charged ends of the molecule, so that on the average the dielectric is uncharged. When an electrical potential is applied, however, the random distribution is disturbed and the charges on the dipoles of the dielectric effectively neutralize some of the charges on the condenser electrodes. This results in the battery being forced to supply more charges to the condenser electrodes, in order to raise the potential to the given level V. Experiment shows the dielectric constant of chlorobenzene to be about 5.

It is obvious that the large differences in dielectric constant resulting from substitution of the atomic groups in organic compounds, such as we have considered, furnish the experimenter with a valuable means of detecting changes in molecular structure and of characterizing known structures. Much work of this character has been done, and today the dielectric constants of many compounds are known with such certainty that they can be used as one of the most sensitive means of establishing the identity and purity of chemical compounds. For illustration, the dielectric constants of a few well-known compounds are shown in Table 1.

TABLE 1

DIELECTRIC CONSTANTS OF TYPICAL ORGANIC COMPOUNDS (AT 20° C.)

Benzene	2.28
Carbon tetrachloride	2.30
Pentane	1.85
Hexane	1.89
Ethyl ether	4.40
Acetone	21.3
Ethyl alcohol	23.8
Methyl alcohol	34.0
Water	80.0

TABLE 2

DIELECTRIC CONSTANT INCREMENTS OF TYPICAL PROTEINS

	Dielectric increment	Molecular weight
Gliadin	0.15	42,000
Zein	0.45	39,000
Secalin	1.0	24,000
Egg albumen	0.17	34,000
Lactoglobulin	1.5	40,000

The value of dielectric constants in characterizing molecules is not limited to low-molecular-weight compounds. Indeed, it has played a conspicuous part in the study of high-molecular-weight compounds, particularly the proteins. For example, consider Table 2, which shows the dielectric constant increments of several typical proteins of agricultural interest. (We speak here of the "dielectric constant increment" rather than the "dielectric constant" because these materials are practically always studied in solution. The dielectric constant increment is defined as the change in the dielectric constant of the solution per gram of dissolved protein.) As the data clearly show, the dielectric increments provide one means of differentiating compounds of practically identical molecular weight.

A serious handicap to the extension of dielectric measurements to proteins and other materials of like nature of agricultural interest is the requirement that the conductivity of the system studied be very low, of the order of that of distilled water. This requirement has seriously hampered the use of dielectric measurements in many important problems. At the Western Regional Research Laboratory our men have made some progress in the removal of this limitation. Measurements of the dielectric properties of an aqueous solution of a protein have been made in which the concentration of protein was about ten times

greater than had been previously reported.

By working at this high concentration (10 percent), our men have found that the electrical properties of a protein solution change very greatly as the concentration is increased. These results suggest the use of dielectric-constant measurements in studies of the interactions of various proteins in solution.

Applications of Dielectric Principles. Water is an important constituent of all agricultural materials. The removal of this water from vegetables and fruits constituted a very great problem in our war effort. But along with the problem of removal of water goes the difficult problem of assaying its quantity in these materials. The emphasis placed on dehydration by the war again brought the problem of determining "moisture content" to the attention of research workers, and hundreds of technical publications prove the problem to be of longstanding importance. Literally dozens of methods have been developed. Only one of these, however, can claim distinct advantages in speed and nondestructiveness; that one is the so-called dielectric-constant method.

Briefly, the dielectric method consists in placing a sample in a condenser and measuring its dielectric constant. The determination is usually carried out at room temperature and involves no alteration of the sample. By reference of the value obtained to a calibration curve of moisture content versus dielectric constant, the moisture content of the unknown is obtained.

This method is not new, but it has undergone considerable development recently because of the demands of the dehydration industry for a rapid method of determining moisture content. At the Western Regional Research Laboratory the application of the method to dehydrated carrots, cabbage, and potatoes has been studied. Not only have several commercial types of dielectric moisture testers been evaluated, but a great deal of effort has been expended in obtaining fundamental information on the effects of the various factors that enter into the determination of the dielectric constant of dehydrated materials; for example, temperature, particle size, and density of compaction of sample.

The method developed at the Western Regional Research Laboratory employs the dielectric cell shown in Figure 6. This cell is very convenient to use and requires only 40 grams of sample, which can be used for other purposes since it is not altered during the measurements. It is designed especially for ease in filling and to exclude from the measurements all stray electrical influences. Circulation of water through the external coil brings the contents of the cell to a definite temperature in a short time. Routine measurements can be obtained in about 20 minutes. With slight sacrifice in accuracy, the time required can be reduced to about 5 minutes.

A calibration curve for carrots is shown in Figure 7. This curve was established with samples containing amounts of water determined by the vacuum-oven method. After the curve is established, it can be used in all work with similar carrots. The precision of the calibration is such that the moisture content can be determined to within plus or minus 0.1 percent over the moisture-content range of 2 to 8 percent, which is the range of moisture particularly important in dehydration practice. If the need arises, calibration curves can be extended to about 20 percent water.

The principal disadvantage in the dielectric method is the requirement of a calibration curve, and someday this disadvantage may be removed if our knowledge of dielectric constants advances to the stage where that of a mixture can be calculated from a knowledge of its chemi-

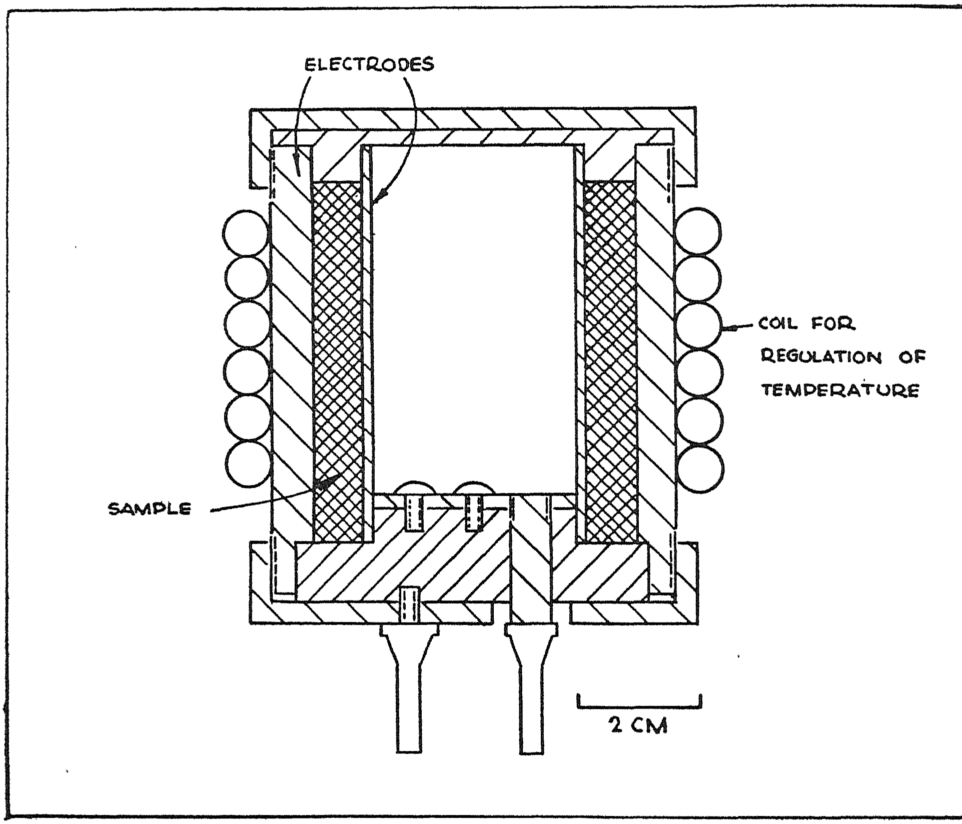


FIG. 6. A SPECIAL DIELECTRIC CELL
USED IN LABORATORY RESEARCH TO MEASURE THE MOISTURE CONTENT OF DEHYDRATED VEGETABLES.

cal composition and physical state. Meanwhile the method offers the conspicuous advantage of speed.

Another application of the dielectric

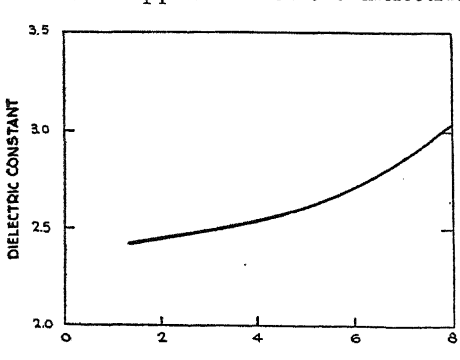


FIG. 7. CALIBRATION CURVE
TO DETERMINE MOISTURE CONTENT OF CARROTS.

cell is one that is currently receiving a large amount of publicity. I refer to so-called dielectric heating, sometimes referred to as high-frequency heating. Dielectric heating is particularly applicable to materials classified as poor conductors of electricity, and into this class agricultural materials fall readily.

Probably the most successful application of dielectric heating is the heating of plywood. In the manufacture of plywood it is necessary to raise the temperature of the wood adjacent to the glue as well as the glue itself to about 275° F. With steam-heated presses it is necessary to heat the entire thickness of wood to the gluing temperature, a process requiring about nine hours, even for rela-

tively small thicknesses of six inches. Today, by the use of dielectric heating, it is possible to glue sections of wood up to many feet in thickness. The time required to heat the glue and the adjacent wood in a six-inch-thick piece has been reduced to about four minutes instead of nine hours, principally because with dielectric heating the heat is generated where it is wanted and it is no longer necessary to conduct the heat required at the glue line through the entire wood mass.

Dielectric heating is directly related to certain characteristics of the dielectric particles to which I referred earlier. When an electric force is applied to a dielectric, the elementary particles of the dielectric are disturbed. When the force is removed, the particles tend to regain their former positions. The important point is that in this process, energy is dissipated as heat within the dielectric. By periodic application of an electric force to the dielectric, the rate of energy dissipation may become sufficiently great to be of practical value.

Dielectric heating has been applied to various problems at the Western Regional Research Laboratory. I shall mention only two applications, one to dehydration practice, and another to the thawing of precooked frozen foods. It was found that dehydrated potatoes could not be successfully compressed into blocks at the specification moisture content of 7 percent. Good compressed blocks could be produced, however, when the potatoes contained 15 percent water. However, the final drying of the compressed blocks by conventional methods from 15 to 7 percent or lower was extremely slow because of the time required for heat to flow from the surface of the block to the interior and because of the length of path of moisture movement. For this reason a test was made of dielectric heating; that is, the heat required for drying was developed in the

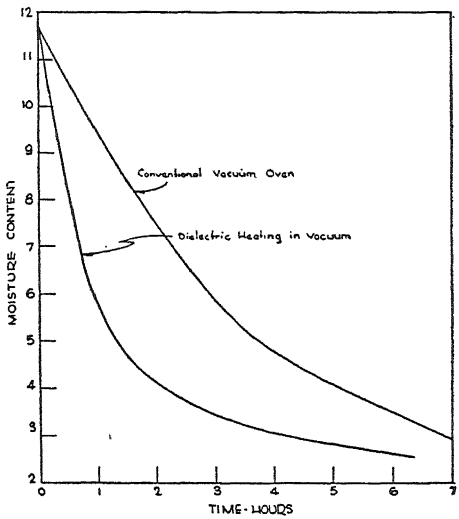


FIG. 8. DRYING POTATOES

REDUCTION OF MOISTURE CONTENT IN BLOCK OF DRIED POTATOES BY TWO TYPES OF HEATING.

potatoes. Potato blocks were dried in a vacuum oven and also in a special vacuum oven with dielectric heating. The results of some of the tests are shown in Figure 8.

The data show that the dielectric heating resulted in a much greater *initial* rate of drying. As the samples became drier the disparity in the rates shown by the two methods decreased because, once the potato blocks reach the desired temperature, rate of diffusion of water from the blocks is the factor that determines the drying rate. With dielectric heating, no problem of heat transfer entered; the samples could be brought to the desired temperature almost instantaneously. In the conventional vacuum oven, however, considerable time is required for heat to be transferred from the oven throughout the sample.

Potatoes dried by the dielectric heating process did not have as good quality, however, as when the slower vacuum oven was used. Apparently a low temperature is desirable at the beginning of the drying cycle when the moisture content is relatively high. While the dielec-

tric heating cycle could be altered to suit this condition, much of its advantage of speed over the conventional vacuum oven would be lost. Obviously, however, in those instances where the material to be dried is not so subject to heat damage as potatoes, the dielectric method offers a possible advantage in speed of drying.

The second application of dielectric heating I wish to consider concerns the thawing of frozen foods, particularly precooked food. In order to increase the usefulness of frozen foods it appears necessary to devise some means of thawing the material and raising its temperature to approximately room temperature in a matter of minutes, rather than hours, as is now required. The present discussion concerns both home and industrial consumers, being equally important for the small packages of frozen food used in homes and the much larger packages held in storage for further processing. The home consumer is primarily concerned with the question of speed, and the industrial processor is principally concerned with the prevention of changes in quality that occur when frozen foods are held for long periods in thawing rooms.

Conventional thawing procedures never result in the entire mass of food being thawed simultaneously. Some portions of the package thaw quickly and are subject to detrimental changes during the period required by the remainder of the package. At the Western Regional Research Laboratory experiments have been conducted to determine whether dielectric heating can be applied to this problem. These experiments have been conducted on standard one-pound packages of frozen fruits and vegetables. The entire package, about $2 \times 4 \times 5\frac{1}{2}$ inches in size, complete in its original cardboard wrapper, was placed between flat parallel copper plates, which formed the dielectric heating-cell electrodes and

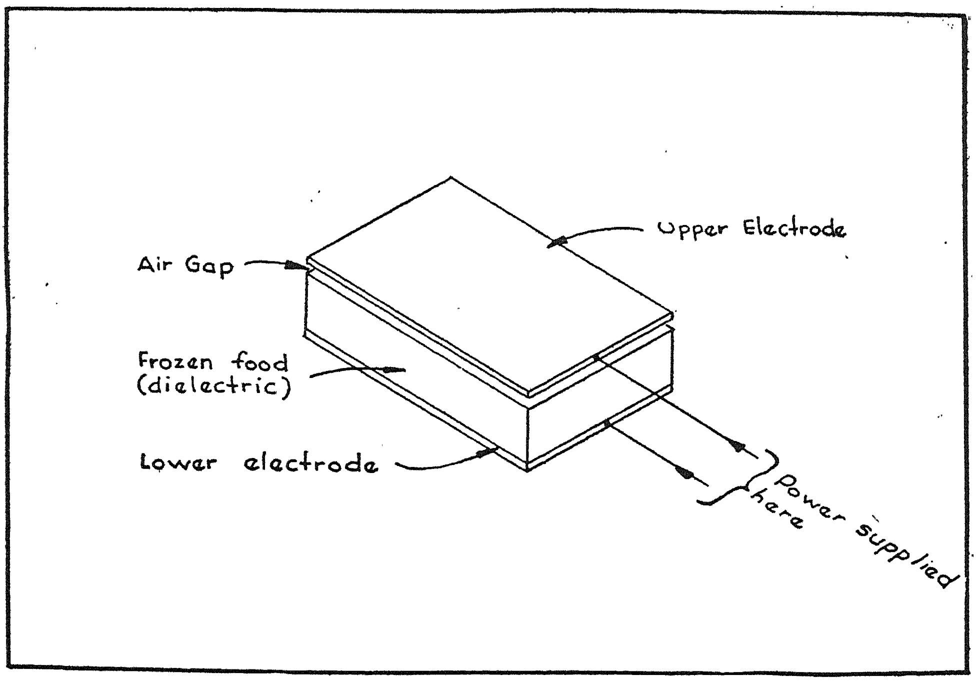


FIG. 9. FOR DIELECTRIC DEFROSTING EXPERIMENTS ON FOOD

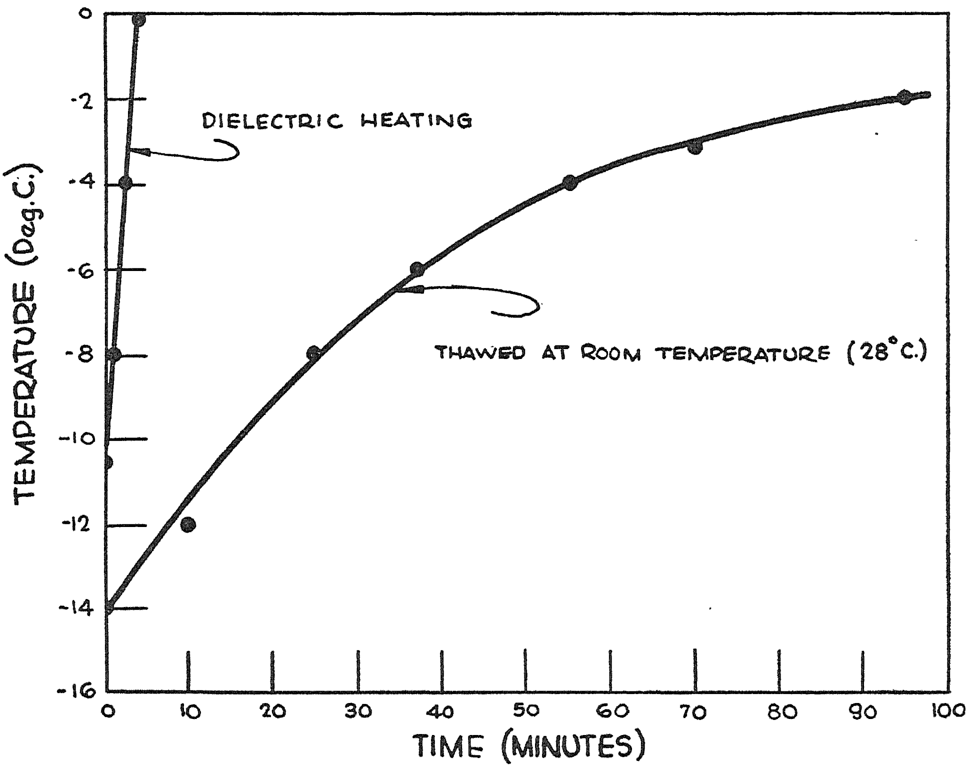


FIG. 10. THAWING FROZEN FOODS WITH TWO TYPES OF HEATING

the electrodes were connected to a high-frequency generator (Fig. 9).

Thermometers placed in holes drilled in the frozen material indicated the temperature at several points during the heating process. Typical observations are presented in Figure 10, where the temperature at the center of a one-pound package of frozen peas is plotted as a function of the heating time. For comparison, the temperature rise in a similar package is shown when the package was held in a room at 28° C. With dielectric heating the rate of temperature rise was practically uniform up to the melting temperature. On the other hand, the package held at room temperature warmed very slowly and at a continuously decreasing rate. These experiments show a gain in speed of thawing of about thirty times when dielectric heating is used. It is possible that much higher gains in speed may be obtained as more is learned about the process and it becomes possible to utilize more powerful generators. While these results are of an essentially preliminary nature because of the exploratory nature of the work, they do indicate the very great advantage of timesaving to be gained by the application of dielectric heating. Many problems remain to be solved before work of this character can be considered to be out of the laboratory stage. Much more information must be gathered in the laboratory concerning the basic dielectric properties of agricultural materials before engineers will have the information needed to design new equipment and new processes.

THE NEED FOR SCIENCE WRITING IN THE PRESS*

By RALPH COGHLAN
ST. LOUIS POST DISPATCH

NEWSPAPER science writers are actually translators. Their job is to translate scientific mumbo jumbo into plain English. It is not easy for several reasons. One is that a lot of scientists distrust the press. They have good cause for this. Some of the things the press has done to science are horrible to contemplate. The press has caused science acute embarrassment; we have sent scientists rocketing to the moon too many times; we have carelessly announced great discoveries that turned out to be fakes and fiascoes.

Watson Davis tells of the first science story he turned in to a city desk. It was one of the early reports that showed that goldenrod does not cause hay fever, but that it is caused instead by ragweed pollen. To Mr. Davis' amazement, the next morning the newspaper carried a very prominent page one story, "but," as he says sadly, "the copy desk in its infinite wisdom had corrected my error and the story told how goldenrod *does* cause hay fever."

We all remember the old Sunday supplement with its "knock-em-dead" stories about alleged scientific discoveries. We were told how we could have rosy cheeks that would last a lifetime. (This turned out to be merely a tattooing job.) We read headlines to prick the curiosity, such as "What Was the Huge Animal the Explorers Saw Disappear in an African Cave?" Or we were edified by the fact that dwarfs, giants, and freaks are now understood by science, with heavy emphasis, of course, on the dwarfs, giants, and freaks.

But the press is not altogether stupid; we are learning. In the excellent *New Yorker* "Profile" of William Laurence, of the *New York Times,* I find this passage:

> The daily press used to be both ignorant and suspicious of science, and generally avoided having to cope with it by ignoring it. Even the most significant scientific developments were slighted.
>
> The invention of the telegraph left the newspapers, which were later so heavily to depend on it, editorially bored, and when the telephone came along it evoked such comment as that of the *Dallas News,* which said smugly, "We must admit our incredulity is put to rather a strong test."
>
> Not a single reporter was present at the Wright brothers' first flight; the *Chicago Tribune,* showing even in 1903 a wondrous talent for steering a straight course of reaction, declined even to mention the preposterous goings on at Kitty Hawk.
>
> Newspapers touched on science only when they found a chance to poke fun at the absent-mindedness of its professors or to hint at strange new discoveries in the field of sex.
>
> Not until the last war broke out and the effectiveness of its weapons became evident did scientists get any respectful treatment, and even then relations between them and the press were not cordial, because nine-tenths of the time interviewers had no idea what their interviewees were talking about.
>
> Laurence is one of the men who have brought science and the newspapers together.
>
> Since 1926, when he began reporting science for the *World,* he has absorbed so much of it, and so rapidly, that he occasionally has to talk down to a scientist to put him at his ease.

Scientists have learned to respect men like Laurence, Watson Davis, Howard Blakeslee, Waldemar Kaempffert, and John J. O'Neill. These men actually represent a new type of journalism. In a sense, it is "pure journalism" in the

* From an address before the luncheon of the George Westinghouse Science Writing Awards, American Association for the Advancement of Science, March 27, 1946.

way that we use the term "pure research." These men do not have to write a story every day. They are not harassed by deadlines. It is their privilege to browse around the laboratories and in the scientific journals to winnow the important from the unimportant. They are given more liberty than the average reporter and are permitted to make judgments as to what is news in science instead of having men who do not know science tell them what scientific news is.

This method leads to a new trust between scientist and journalist, and the press has got to produce more journalism like this. It has got to produce more organizations like Science Service and more scientific staffs like those of the *New York Times* and *Herald Tribune;* it has got to produce more publishers like old E. W. Scripps, who said, "Damn it! If we have got to have a democracy, let's have an intelligent one." Mr. Scripps strongly believed that an intelligent democracy was one that knew something about science and the methods of science as a guide to everyday life.

I say the press has got to do these things because it *must*—Hiroshima and Nagasaki settled that. No matter how odious science is, the public must learn about it. The public must learn about it or else we will not be on this planet much longer. Perhaps it would be just as well. However, a lot of us would like to stick around yet for awhile. We want to see the day when scientists' little tricks with the atom can operate an automobile from coast to coast on a cupful of gasoline. (I believe I will take that back as I suppose it is still another piece of journalistic rot!)

WHILE I freely confess that the press has mutilated scientific theory, has misrepresented it, and has often unduly raised the hopes of many people by prematurely announcing cures of deadly diseases, there is another side to this picture. That is the fact that scientists themselves have too long lived in ivory towers. They have enjoyed to the full their intellectual snobbery. They have kept themselves aloof from the common herd. It used to be said that only twelve men in the world understood Einstein's theory of relativity. This must have given exquisite pleasure to those twelve men. It must have given even more pleasure to other scientists who rejoiced that their two-, four-, or eight-story minds were so superior to the minds of other men.

Scientists have not only lived in ivory towers; they have developed a lingo impossible for anybody but themselves to understand. It is actually a strange language. I should add that it is really a set of strange languages because very often one group of scientists cannot understand what another group of scientists is talking about. Like E. W. Scripps, Edward E. Slosson was preaching more than twenty years ago that science should be democratized and brought within the reach of the many.

Slosson in a *Century Magazine* article published in January 1922 said that if you suggest to a person he might find it interesting to study botany or chemistry he is likely to reply that he "had it as a boy," implying that, like mumps or measles, he could never catch it again. He criticized scientists for turning away eager lay minds with an attitude that seemed to say, "Run away, child. You couldn't understand what I am doing if I *did* explain it to you."

(I am, of course, not trying to disparage the exactness of statement which scientific terminology represents.)

To go back again to Mr. Slosson:

> The popularization of science does not mean falsification, but its translation from technical terms into ordinary language. Popular science need not be incorrect, but it has to be somewhat indefinite. It differs from the exact sciences in being inexact. The scientific mind is

set at too sharp a focus for ordinary use. The would-be popularizer is always confronted by the dilemma of comprehensible inaccuracy or incomprehensible accuracy, and the fun of his work lies mainly in the solution of that problem.

There is no excuse for a scientist to display his learning with big words when little ones would do just as well. What would you think, for example, of a scientist who would paraphrase Wordsworth in this fashion?

A primrose by the river's brim
Primula flava was to him and it was nothing more.

Or how would you like it if a scientist would express Lord Tennyson's famous line about a young man's fancy in this way: "In the spring the chief activating gland of the kinetic system, the thyroid, shows a distinct enlargement."

So, while I am perfectly willing as a member of the press to accept justifiable criticism from scientists for the way their facts have often been manhandled, the fault in the bad relations between science and the press rests with scientists, too.

I am glad the chasm between us is being bridged, even though we both still have a long way to go. We shall have to hurry to complete the bridge—the proper use and control of atomic energy makes that imperative—and I think we can confidently expect a more and more intelligent treatment of science by the press.

Newspapers have to deal often, among other things, with the intricacies of the legal profession. Many lawyers try to baffle us with their technical terms, just as scientists try to baffle us with their technical terms, but the lawyers have not been successful. If newspaper readers do not yet know the meaning of such monstrous legal phrases as "incorporeal hereditament," they perfectly well understand that when a fellow is thrown in jail it is often a writ of habeas corpus that gets him out. And the people have the right to know at least as much about enzymes and hormones as they do about habeas corpus.

The awful, as well as beneficial, discoveries of science have forced scientists into politics, and when scientists go into politics they will have to learn to talk the plain language of the people.

As Professor Harold C. Urey expresses it:

I have dropped everything to try to carry the message of the bomb's power to the people because if we can't control this thing there won't be any science worthy of the name in the future. I know the bomb can destroy everything we hold valuable and I get a sense of fear that disturbs me in my work. I feel better if I try to do something about it.

A great many scientists feel the same way. A great many of them feel the most solemn obligation to prevent the atomic bomb from being controlled by our military men, and a great many of them feel that they must do something to defeat the May-Johnson bill, which would cripple scientific research.

In the March 1946 issue of *Harper's Magazine* there is a notable article on this subject by Joseph A. Brandt, who says: "The life of academic quietism is over. The man of learning, however well equipped he may be, must learn to become a man of action, a politician, a man of the people, speaking for people, leading people."

If the scientist does play this role, he must make full use of the press. The press, on its part, must develop more and more staff members who can translate the language of science into the language of the people. I hope a new relationship will come into being among science, politics, and the press—a relationship that will bring science closer to the people and that will enable man, by his knowledge of science, to control his own destiny.

CHARLES MASON AND JEREMIAH DIXON

By THOMAS D. COPE
DEPARTMENT OF PHYSICS, UNIVERSITY OF PENNSYLVANIA

"STARGAZER" has a special meaning in Chester County, Pa., though it is now nearly 200 years since Charles Mason and Jeremiah Dixon came from England to set up their surveying instruments on the farm of John Harlan in Newlin Township. For the tradition persists among Harlan's descendants, who still own and cultivate the farm, and among the descendants of men who worked for Mason and Dixon that these two were unusual individuals who worked with remarkable instruments and spent much time mysteriously observing the stars. Except in this community, however, and to a few astronomers, geodesists, and perhaps a chance historian, the men Mason and Dixon are shadowy figures or "just surveyors."

Charles Mason laid the foundation of his career at Greenwich where he worked from 1756 to 1760 as assistant observer to the Reverend James Bradley, Astronomer Royal. Bradley was then near the end of his career (he died in 1762). As a young man he had discovered the aberration of light and the nutation of the axis of the Earth and had founded a new era of precise observations. He had enlarged the Greenwich Observatory and equipped it with new instruments. Mason acquired some of the spirit and the ideals of his preceptor and learned from him the use of instruments of precision and the art of observing. At Greenwich Mason made the acquaintance of the Reverend Nevil Maskelyne, a young astronomer who was assisting the Astronomer Royal in a study of atmospheric refraction. This association of Mason and Maskelyne continued as long as Mason lived.

And at Greenwich Mason first learned to know Mayer's *Tables of the Moon.* Early in 1755 Professor Tobias Mayer of Göttingen University had filed a claim with the Lords of the British Admiralty for one of the awards promised by act of Parliament of the fourteenth year of Queen Anne to discoverers of improvements in ways of finding the longitude at sea. In support of his claim Mayer had submitted in manuscript a new set of lunar tables with accompanying theory and other papers. In due course the Admiralty referred these claims and exhibits to the Astronomer Royal for investigation and report.

Bradley first reported on Mayer's *Tables* on February 10, 1756, and finally on April 14, 1760. With the assistance of Charles Mason, Bradley had compared positions of the Moon as predicted in the *Tables* with positions as observed at Greenwich. Eleven hundred comparisons led to the comment:

> So far as it will depend upon the lunar tables the true longitude of a ship at sea may in all cases be found within about half a degree and generally much nearer.
>
> It remained to be examined within what limits the errors arising from observations actually taken at sea could be contained.

This test was carried out for Bradley by Captain Campbell, of *H.M.S. Royal George,* on cruises near Ushant in 1758 and 1759. A sextant was made especially for the trials by John Bird, instrument maker for Greenwich Observatory. Bradley's final comment reads:

> However great the difficulties of finding the longitude by this method seem to be, they are not insuperable, or such as ought to deter those whom it most nearly concerns from attempting to remove them.

James Bradley was now nearing the end of his days. His hand was faltering.

Nevil Maskelyne and Charles Mason received the torch he had carried. They made the development of the method of lunar distances for finding the longitude at sea a major concern of the rest of their lives.

A transit of Venus across the face of the Sun was due to occur on June 5, 1761. It would afford a golden opportunity to determine the distance from Earth to Sun. To secure data observers would need to be stationed at strategic posts over the face of the Earth. The equipment needed would be simple—telescopes, timepieces, and instruments for measuring small angles.

Early in 1760 the Royal Society of London drew up plans to observe the transit at home and to send expeditions abroad, one to St. Helena where the end of the transit could be observed, the other to Sumatra where the whole phenomenon could be seen. Nevil Maskelyne was chosen to go to St. Helena, assisted by Robert Waddington; Charles Mason was selected to go to Sumatra with Jeremiah Dixon as assistant. Dixon was a surveyor and amateur astronomer from Cockfield in the county of Durham.

Both expeditions sailed from England as the year changed from 1760 to 1761. Mason and Dixon were carried by *H.M.S. Seahorse*. An engagement with a French man-of-war and accidents of wind and weather delayed their progress. They were obliged to disembark at the Cape of Good Hope and observe the end of the transit there. It occurred just after sunrise on June 5; weather was excellent.

Observations were reported by 176 observers from 117 stations. Perhaps the best commentary upon the work of Mason and Dixon on this occasion was written 130 years later. In 1891 the late Professor Simon Newcomb of the United States Naval Observatory published an exhaustive review of the transits of Venus of 1761 and 1769. To the observations made by Mason and Dixon at the Cape of Good Hope in 1761 he assigned weights that were among the very highest that he allotted.

In the early autumn of 1761 Mason and Dixon sailed from the Cape of Good Hope to St. Helena and joined Maskelyne and Waddington in a program of research that continued into 1762. Maskelyne had with him an astronomical clock made for the Royal Society by John Shelton. This clock is still in the possession of the Society and has been used on expeditions to many regions. Bradley had adjusted the clock at Greenwich and had found its rate of losing time when compared with the stars. Maskelyne had set it up on St. Helena with its pendulum at the same length and had found its rate there. Soon after his arrival at St. Helena, Dixon was sent with the clock back to the Cape of Good Hope for a similar determination.

The rates of a pendulum of constant length at various stations give relative values of gravity at these stations. They in turn permit inferences about the relative lengths of the polar and equatorial diameters of the Earth. Since Maskelyne was interested in the size and shape of the Earth, he continued to make determinations of gravity with John Shelton's clock. He took it to the Barbados during 1763 and 1764. In 1766 he sent it to Mason and Dixon in Pennsylvania, where they observed with it on Harlan's Farm.

Since at St. Helena the star Sirius in its diurnal motion passes near the zenith, its zenith distance as it crosses the meridian can be measured with precision. On account of its brightness Sirius was regarded as one of the nearest stars, probably with a large annual parallax. If this assumption were correct the zenith distance should change materially with the motion of the Earth in its orbit over a period of months. Maskelyne went to St. Helena prepared to measure the zenith distances of Sirius with a new zenith sector built for the Royal Society

by Jonathan Sisson. (A zenith sector is a graduated arc of a vertical circle provided with a telescope and plumb line.)

Maskelyne soon found that the manner of suspending the plumb line introduced serious errors into the readings. After his return to England he demonstrated to a committee of the Royal Society that the fault existed and that it had probably affected all zenith sectors previously built. The first sector free of this defect was built at once for Thomas and Richard Penn by John Bird and in the autumn of 1763 was brought to Philadelphia by Mason and Dixon. It was used by them in every determination of latitude they made in America.

His voyages to St. Helena and back to England and the voyage to the Barbados permitted Nevil Maskelyne to test the method of lunar distances and Mayer's *Lunar Tables* in finding the longitude of his ship. The outcomes were so encouraging as to lead Maskelyne to embody the method in a booklet for navigators that he published in 1763, *The British Mariner's Guide*.

James Bradley's successor as Astronomer Royal was Nathaniel Bliss, who lived only two years after his appointment. Maskelyne succeeded him early in 1765 and thus became the fifth Astronomer Royal at Greenwich.

In the meantime, on July 4, 1760, while the Royal Society was planning to observe the coming transit of Venus, the Proprietors of Maryland and Pennsylvania had entered into an agreement before the Court of Chancery to end their differences and forthwith to survey and mark their common boundaries. Both parties appointed commissioners from the provinces to carry out the articles of agreement. They in turn met at New Castle on the Delaware, agreed upon procedures, and engaged surveyors from Maryland and Pennsylvania.

The Middle Point of the peninsula between Delaware and Chesapeake bays had been determined, accepted, and marked in 1751. It is now the southwestern corner of Delaware. The eastern boundary of Maryland northward from the Middle Point to the Pennsylvania border was still to be surveyed. It was agreed upon as a straight line northward up the peninsula from the Middle Point until it made a tangent to the periphery of a circle of 12 miles radius drawn around New Castle. From the Tangent Point northward the boundary should consist, first, of that part of the circular arc that might lie westward of the meridian through the Tangent Point, and thence northward the meridian of the Tangent Point to an east-west line 15 miles south of the southernmost point of the city of Philadelphia. Except for slight errors made in surveying this describes the boundary as it stands today.

The provincial surveyors struggled to establish the Tangent Line from the spring of 1761 until the autumn of 1763. They ran a meridian northward from the Middle Point until the spire of the Court House in New Castle was in sight. Then they ran a radius out from the spire to intersect the meridian, measured distances and angles, calculated the position of the Tangent Point and the bearing of the Tangent Line. They set posts to mark the Tangent Point. Then they returned to the Middle Point and proceeded to lay off the Tangent Line. It fell half a mile to the east of the Tangent Point; a second trial ran 350 feet to the west.

The Proprietors were being kept informed of the progress of the survey. Both consulted Dr. John Bevis, and Thomas Penn sought advice from Dr. John Robertson, Master of the Royal Naval Academy at Portsmouth. Jonathan Sisson and John Bird were engaged to construct surveying equipment. As

months grew into years and as expenses mounted, the Proprietors became impatient.

Just who introduced Mason and Dixon to Thomas Penn is not clear. On June 18, 1763, however, Penn wrote to the Commissioners for Pennsylvania:

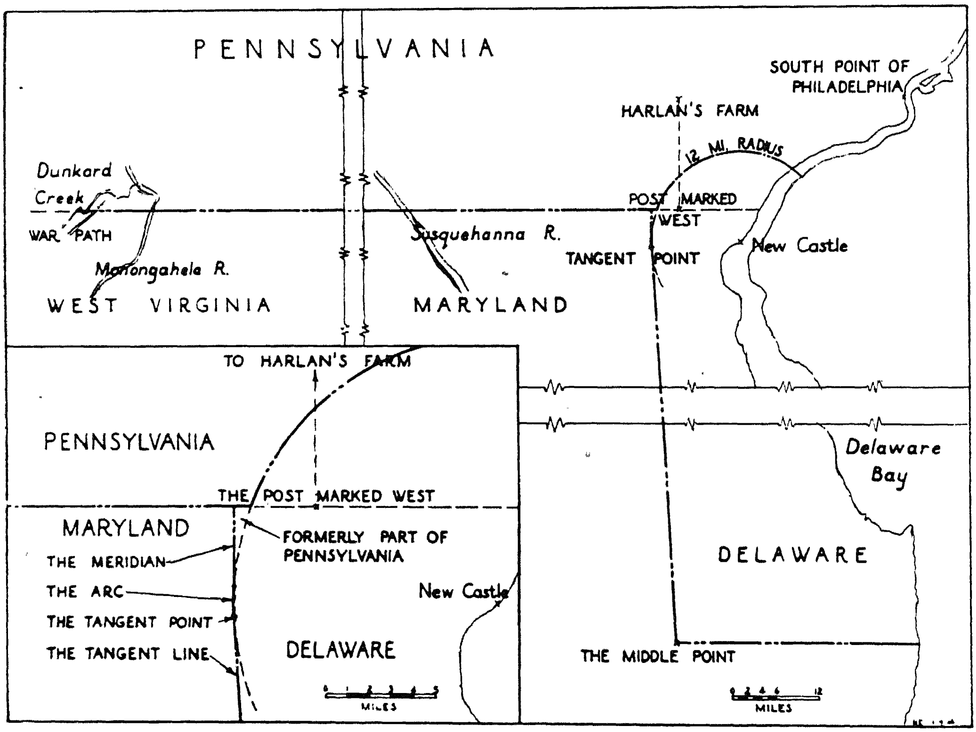
> We intended to persuade Lord Baltimore to join in sending some very able Surveyors that were skillful in making Celestial observations from hence, to this proposal we never got any consent of his till two Days since, . . . and Mr. Calvert joined with us in desiring the mathematicians, we proposed, to come to London, as soon as possible.

On August 10, 1763, he wrote to the provincial secretary, the Reverend Richard Peters:

> Mr. Mason and Mr. Dixon have taken their passages with Captain Falconar . . . and they have with them the fine Sector, two Transit Instruments, and two reflecting Telescopes, fit to look at the Posts in the Line for ten or twelve miles.

They carried also *Hints for Running the Lines* that had been prepared by John Bevis and Daniel Harris. The latter had succeeded John Robertson as mathematical master at Christ's Hospital.

Mason and Dixon arrived at Philadelphia on November 15, brought their instruments ashore, met the commissioners of both provinces, and took oaths to undertake the survey. With the aid of city authorities they selected the South Point of Philadelphia, erected a temporary observatory near it, set up John Bird's zenith sector, and measured the zenith distances of eight stars of Bradley's *Catalogue* as they passed the meridian. Apparent distances were corrected for refraction and were reduced for precession, aberration, and nutation to January 1, 1764. The latitude of the south point of Philadelphia as given by



IMPORTANT POINTS OF MASON AND DIXON'S SURVEYS

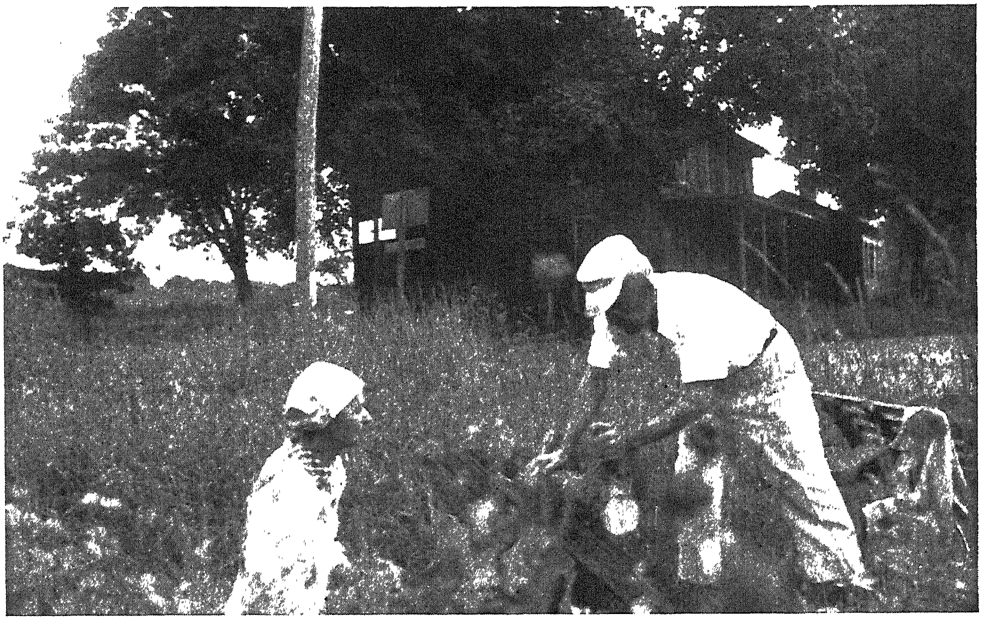


Photo by J. Carroll Hayes

THE HARLAN HOMESTEAD

IN JANUARY 1764 MASON AND DIXON MOVED TO THIS FARM WHICH BECAME THEIR HEADQUARTERS.

stars taken from Bradley's *Catalogue* was 39° 56′ 29.1″. Modern determinations make it only 2.5″ less.

In January 1764, the surveyors moved westward 31 miles to John Harlan's Farm, which became their headquarters. The observatory was erected, and the latitude was found. When the season opened, they moved southward from Harlan's along a meridian. Distances were measured as they proceeded, on level ground by chain, on slopes by levels. Again the latitude was determined, and a post marked "West" was set up in a field of Alexander Bryan's farm in New Castle County to mark the latitude 15 miles south of the South Point of Philadelphia. This post became the reference point for the parallel of latitude that separates Pennsylvania from Maryland.

For the next 4 years the survey proceeded. Vistas were opened through the forest, the Tangent Line was run, and the position of the Tangent Point was confirmed as marked. Northward, one mile and a half of circular arc and three miles and a half of meridian completed the eastern boundary of Maryland to the northeastern corner of the state in latitude 15 miles south of the South Point of Philadelphia.

The Parallel that separates Maryland and West Virginia from Pennsylvania was surveyed from the Delaware River to Dunkard Creek in the west. Agreement provided that it should span 5 degrees of longitude, but conditions in the Indian country halted its westward extension. The Five Nations provided an escort from the crest of the Alleghenies westward during the summer of 1767, but the escort refused to cross an Indian line west of the Monongahela River.

The Parallel was established step by step. Arcs of great circles were calculated to intersect the Parallel 10 minutes to the west. The arcs were run by transit. Then latitude was found by the zenith sector; offsets to the Parallel were calculated, and the Parallel was marked.

Historical Society of Delaware
THE TANGENT STONE
FROM THE DELAWARE SIDE. THE NEWER STONE WAS ERECTED IN 1849 NEXT TO THE ORIGINAL.

Vistas were opened along all lines. In the mountains to the west the Parallel was marked by cairns on the ridges; to the eastward the boundaries were marked by limestone monuments brought from England. Many of these markers are standing to this day.

On January 29, 1768, Mason and Dixon handed the map of the boundary to the commissioners.

Mason and Dixon spent five winters in America. The two winters following that of 1763–64 took them on excursions into neighboring colonies. The winters of 1766–67 and 1767–68 were spent at Harlan's Farm in scientific work for the Royal Society. Both men were acquainted with geodetic and geophysical measurements that had been made in Europe and on other continents. They themselves had used John Shelton's clock for determinations of gravity on St. Helena and at the Cape of Good Hope. They knew that in 1752 the Abbe de la Caille had measured an arc of meridian at the Cape and from it had calculated the length of a degree of latitude in that region of the Earth. In the same year the Jesuit astronomers Boscovich and Le Maire (the latter was from the same county in England as Jeremiah Dixon) had measured an arc of meridian across the Apennines from Rome to Rimini. For generations members of the French Academy had been measuring meridians in France, in Lapland, and in Peru. And in 1761 Cassini de Thury had begun to extend a parallel of latitude from Brest to Vienna. These projects had contributed data from which the size and shape of the Earth had been inferred, especially the flattening at the poles and the bulge at the equator.

No data had as yet been obtained from North America. In 1760 Father Roger Joseph Boscovich visited London. He was well received and was elected a fellow of the Royal Society. The record shows that he proposed to the Society that it measure a degree of latitude in America.

Before they had been long in America Mason and Dixon acquainted the Royal Society with the opportunities that were at hand along the Maryland-Pennsylvania boundary for measuring a degree of latitude and a degree of longitude. Their proposal was conveyed to the Council of the Society by Nevil Maskelyne. It received favorable attention on October 24, 1765. The project of measuring a degree of latitude was sponsored, funds were appropriated, instruments were supplied, the acquiescence of Lord Baltimore and the brothers Penn was sought, and the use of their instruments was bespoken.

Both the secretary of the Royal Society and the Astronomer Royal sent word of these decisions promptly. The latter drew up a letter of advice and instructions and forwarded it early in Novem-

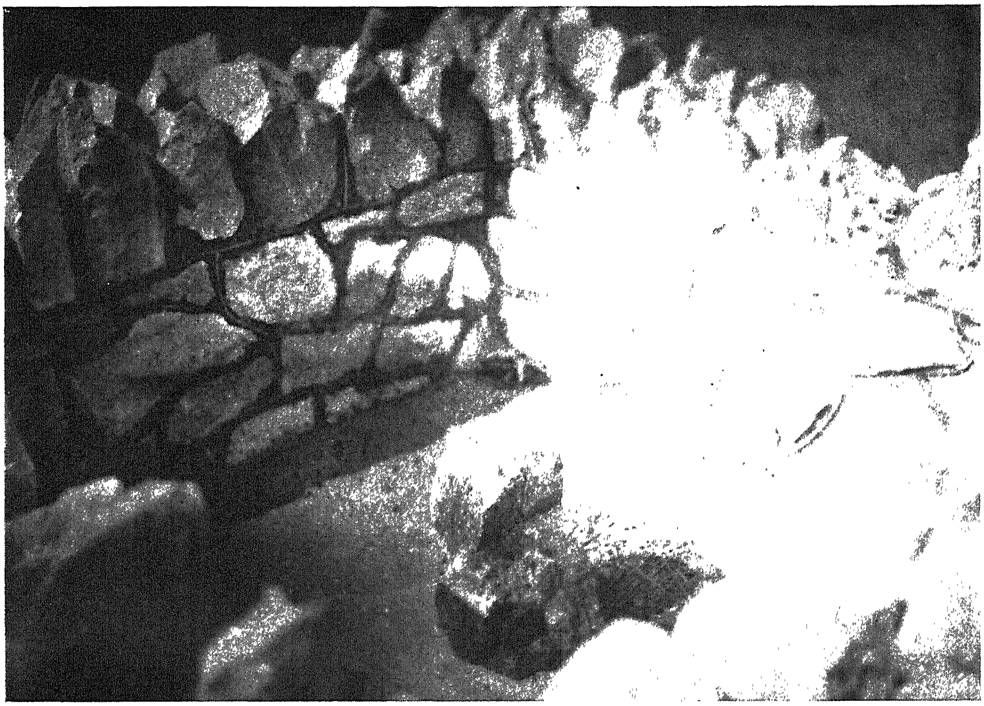


Photo by C. H. Thomas

THE STARGAZERS' STONE

THIS MARKER, NOW PROTECTED BY A STONE WALL, WAS SET BY MASON AND DIXON ON MARCH 5, 1764.

ber 1765. Mason and Dixon were instructed to measure again the courses that linked the site of the observatory at Harlan's with the Middle Point. Both lengths and bearings were to be redetermined with all attainable precision. The latitude of the Middle Point was to be found and that of the site at Harlan's found again. To improve precision a 5-foot brass rod and 20-foot fir rods with brass ends, made by John Bird, were provided. Besides, there were sent a spirit level, silver wire for supporting plumb bobs, thermometers, and John Shelton's clock. Detailed instructions accompanied the instruments.

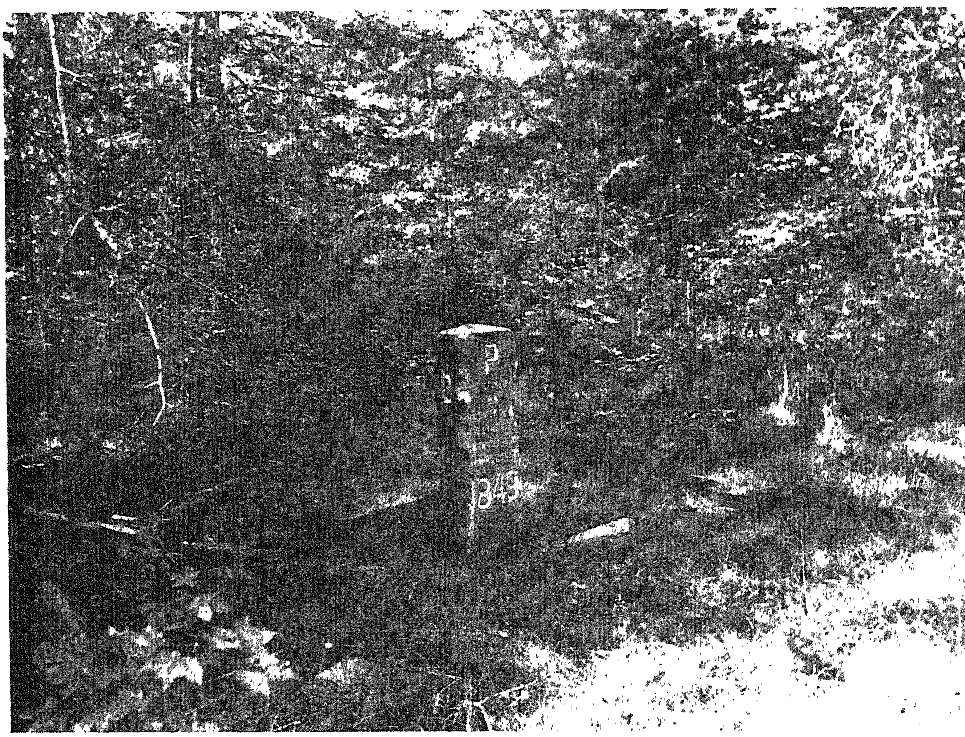
The first opportunity to work on the projects of the Royal Society came to Mason and Dixon in October 1766. They had just returned to the Maryland-Delaware peninsula from the Alleghenies, where they had spent the season extending the Parallel of Latitude westward. All work planned for the year had been completed except that of setting markers along the Tangent Line. On October 8 they arrived at the Middle Point and there spent 11 days in finding the latitude. The zenith distances of ten of Bradley's stars were measured repeatedly as they crossed the meridian. Corrections for refraction were made, and reductions for precession, aberration, and nutation.

In 1764 two courses had been run from the Middle Point toward the Tangent Point, one slightly to the west of it, the other to the east. The bearing of the former course was now remeasured at the Middle Point. The party then moved slowly northward along the boundary, checked bearings, measured offsets to the Tangent Line, and set permanent markers. From November 18 to 21 they

met with the Commissioners at Christiana Bridge, New Castle County, and were there instructed to extend the Parallel eastward to the Delaware River. This was completed by December 1.

The party then moved northward again and on December 5 established itself for the winter at Harlan's Farm. The zenith sector was set up just where it had stood 3 years earlier. Two astronomical clocks were mounted. One belonged to the Penns; the other was John Shelton's clock. The party remained at Harlan's until the middle of June 1767. The latitude was found with great precision. Eclipses of Jupiter's moons were observed to establish the longitude of the station. The pendulum of Shelton's clock was adjusted to the length prescribed by the Astronomer Royal, and by comparison with the stars its rate was found. Amplitude of swing and temperatures were recorded systematically. On May 24 there came a copy of the first issue of the *Nautical Almanac,* that for the year 1767, and a letter from the Astronomer Royal asking for a report of progress and directing that Shelton's clock be sent home at once, in order that it might be made ready for use in observing another transit of Venus expected to occur in June 1769.

The Parallel of Latitude was extended to its farthest west during the summer and autumn of 1767. The final report of the survey was handed to the Proprietors during January 1768. Mason and Dixon were then free to complete their projects for the Royal Society. They returned to Harlan's Farm on February 1 and at



Historical Society of Delaware

THE TRISTATE STONE

SET IN 1849 TO REPLACE A STONE ERECTED IN THE ORIGINAL MASON AND DIXON SURVEY AT THE TIP OF THE WEDGE (SEE MAP), WHICH WAS THEN REGARDED AS THE SOUTHERNMOST POINT OF PENNSYLVANIA.

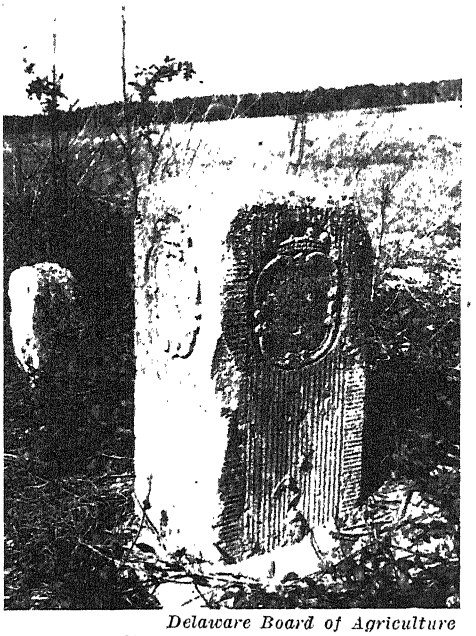
once made ready to measure the courses that extended southward from that station to the Middle Point.

Levels had to be constructed. Joel Bailey, a local surveyor who had helped with the survey, was engaged to build sturdy frames of pine to carry the 20-foot brass-tipped fir rods. Each frame had a plumb line hanging in a tube at its middle. A frame was set up with its rod horizontal in the line being measured. Its position was marked to the hundredth of an inch on a stake set at the bottom of the plumb bob. The next frame was set in line with the rods end to end as shown by a plumb line. Temperatures were recorded. The rods were checked regularly against the Royal Society's 5-foot brass standard of length.

With all attention to detail the levels were applied first to the course 15 miles due south from the observatory at Harlan's to the Parallel. Then a shift of almost 3 miles due west placed them in the meridian of the Tangent Point. The levels were applied to this meridian until the Tangent Point was reached. Then the levels were carried westward and 90 feet southward to the arc of a great circle first run in 1764 and checked two years later. Thence a steady, patient march of almost 82 miles brought men and levels to the Middle Point on June 6.

Mason and Dixon returned to Harlan's Farm on June 16 and said farewell to it two weeks later. They settled accounts with the Commissioners during August and sailed from New York for England on September 11. "Thus ends my restless progress in America," wrote Mason at the end of their *Diary*.

From the data, Nevil Maskelyne found that the Middle Point lay south of the observatory at Harlan's 1° 8′ 45″ by celestial observations and 538,067 feet as measured horizontally over ground. Hence "363,763 English feet is the length of a degree of latitude in the provinces of Pennsylvania and Maryland." He analyzed the work and incorporated the result in a table of degrees of latitude that had been measured in Europe, South Africa, and South America between 1736 and 1768. (Immediately after its announcement Honorable Henry Cavendish pointed out that the Atlantic Ocean and the Alleghenies had probably deflected the plumb line of the zenith sector to an appreciable extent for which no correction could be made.)



Delaware Board of Agriculture

THE MIDDLE POINT

BETWEEN THE CHESAPEAKE BAY AND THE OCEAN.

Philadelphia in those days was the second city of the British Empire, and among its inhabitants was a coterie of men who were keenly appreciative of scientific progress. Charles Mason, Jeremiah Dixon, Nevil Maskelyne, and Joel Bailey were soon elected to the American Philosophical Society held at Philadelphia for Promoting Useful Knowledge.

For the guidance of observers of the transit of Venus which was to occur on June 3, 1769, Astronomer Royal Maskelyne published instructions as a supple-

Delaware Board of Agriculture

THE COURTHOUSE AT NEW CASTLE, DEL.

THIS WAS THE ORIGINAL CENTER OF THE ARC OF 12 MILES' RADIUS FORMING THE DEL.-PA. BOUNDARY.

ment to the *Nautical Almanac*. The Royal Society prepared to observe the transit at home and to send expeditions abroad. Western Europe was to see the transit begin before sunset; eastern North America would see its early stages during the afternoon; western North America, Alaska, and the Arctic regions would see the entire transit; and Australia, Asia, and eastern Europe would see its end after sunrise.

Astronomers of the American Philosophical Society established temporary observatories for viewing the transit on State House Square in Philadelphia, at Norriton, Pa., and at Lewes, Del. They made admirable records of the transit and sent them to the Royal Society. The Royal Society sent William Wales and Joseph Dymond to Prince of Wales Fort on Hudson Bay. They saw the entire transit from ingress to external contact at egress with highly favorable weather throughout. To observe the transit in the Pacific, the Society sent Lieutenant James Cook on the first of his famous voyages. His ship, the *Endeavour,* carried a distinguished company which included astronomer Charles Green and naturalist Joseph Banks. They observed the transit from Tahiti. "There not being a cloud in the sky from the rising to the setting of the Sun, the whole passage of the planet Venus over the Sun's disk was observed with great advantage."

Jeremiah Dixon and W. Bayley were sent to North Cape, Norway. Weather was unfavorable, and their observations were fragmentary. Charles Mason pro-

ceeded to Cavan, near Strabane, in county Donegal, Ireland, established an observatory, and made records for the Royal Society from April 3 to November 28, 1769. His report on time, latitude, and longitude is a masterly one. He made complete records of the beginning of the transit.

Years later the astronomer Encke studied and restudied the data secured in 1761 and 1769. In 1835 he announced the conclusion that the Earth is 95,370,000 miles from the Sun, a distance accepted as correct for the next 30 years.

Sir Isaac Newton had predicted that a "hemispherical mountain 3 miles high and 6 miles broad will not by its attraction draw the plumb line 2 minutes out of the perpendicular." In 1738 members of the French Academy had tried to measure the attraction of Chimborazo in South America, with inconclusive results. Bouguer, who reported the attempt, proposed that a similar experiment be tried in France or England. In 1772 Maskelyne proposed to the Royal Society that "the Attraction of some Hill in this Kingdom be measured by Astronomical Observations." The proposal was approved, and in 1773 Charles Mason was sent north to find a hill "of sufficient height tolerably well detached from other hills, and considerably larger from East to West than from North to South." He chose Schehallien in Perthshire, Scotland.

Maskelyne established an observatory on the south face of Schehallien in the early summer of 1774. He set up Jonathan Sisson's zenith sector and measured the zenith distances of stars in Mason's table as they passed the meridian. Then his instrument was carried over the mountain to its north face where similar observations were made of the same stars. The differences made it appear that the instrument had been moved 54.6 seconds of arc northward. The difference in latitude was independently found by two triangulation surveys from

U. S. Coast and Geodetic Survey

GRANITE MONUMENT NO. 222

ERECTED IN 1865 BY SINCLAIR TO MARK THE NORTHWEST CORNER OF MARYLAND. PHOTO TAKEN IN 1900.

two carefully measured base lines on the plain below the mountain. Both agreed in placing the second station 42.94 seconds of arc north of the first.

The difference—54.6 less 42.94, or 11.66 seconds—was attributed to the deflection of the plumb line of the zenith sector by the mountain. During the triangulation the dimensions of the mountain were measured, its rocks were surveyed, and its mean density was estimated. The deflection of the plumb line permitted an estimate to be made of the mean density of the Earth. In this project instruments were used that have been met before: Sisson's zenith sector; John Shelton's clock; and the Royal Society's 5-foot brass standard.

THE last quarter of Charles Mason's life was spent on projects that grew out of his earlier work at Greenwich Observatory as assistant observer to Astronomer Royal Bradley. From studies of Bradley's records Mason prepared tables for the *Nautical Almanac* and he was able to improve Mayer's *Tables of the Moon* by comparing them with the Greenwich records.

The *Nautical Almanac* was established by Nevil Maskelyne in 1767, immediately after his appointment as Astronomer Royal, and it has appeared annually ever since. In the first issue the latitude and longitude of Cape Town are given as found by Mason and Dixon in 1761. The comment follows that "it is probable that the Situation of few Places is better determined."

To the *Almanac* for 1773 Mason contributed a catalogue of stars. The preface, written by Maskelyne, states:

> To this Ephemeris are annexed . . . a Catalogue of 387 fixed Stars . . . adapted to the beginning of the year 1760 . . . calculated from the late Dr. Bradley's Observations by Mr. Charles Mason, formerly his Assistant. . . . After the Catalogue follow some Memoranda . . . communicated by the same Mr. Mason.

The issue for 1774 first introduces Mason's improvements of Mayer's *Lunar Tables*. The preface written by Maskelyne on July 2, 1772, states:

> To this Ephemeris are annexed 1220 Longitudes and Latitudes of the Moon deduced from the late Dr. Bradley's Observations. . . . The greater part of these calculations were made during Dr. Bradley's Lifetime by himself and his Assistant Mr. Charles Mason; and what was left unfinished has been completed by Mr. Mason since at the Instance and at the Expence of the Board of Longitude.
>
> A Series of Observations this for Number and Exactness far excelling anything of the same kind which the World ever saw before. . . .
>
> Accordingly the Board of Longitude have thought proper to employ Mr. Mason farther in making the necessary calculations for improving Mayer's printed Tables under my Direction. . . .

The introductory pages of the *Almanac* for 1776 include a digest of legislation of the fourteenth year of King George III which had become effective on June 24, 1774. Rewards were set for timepieces, lunar tables, and other aids to navigation and finding the longitude.

References to new work by Mason appear in the prefaces to the *Nautical Almanac* for 1777 and for 1788. Finally, in the preface to the *Almanac* for 1798, which he signed on December 2, 1791, Maskelyne summarizes Mason's work on the Moon's position:

> The Moon's Place in the Heavens was inserted as calculated directly from Mayer's *Tables* in the *Nautical Almanac* from 1767 to 1776 inclusive, or the first ten years,
>
> But from the *Nautical Almanac* of 1777 to that of 1788, both inclusive, or the next twelve years, the Moon's Place was inserted as calculated from new *Tables*, improved from Mayer's *Tables*, composed by Mr. Charles Mason. . . .
>
> But from the *Nautical Almanac* of 1789 to 1796, both inclusive, the Moon's Place was inserted as calculated from new *Tables*, still further corrected by Mr. Mason, entitled by him the *Tables of 1780*, as having been completed about that time. . . .

Maskelyne then proposes small corrections to Mason's *Tables*.

February 9, 1765, is a cardinal date in the story of Mayer's *Tables*, for on that

day Astronomer Royal Maskelyne laid before the Commissioners of Longitude reports of his own success in using the *Tables* for finding the longitude on his recent voyages to St. Helena and to the Barbados and also reports from masters and mates of ships of the East India Company. Upon hearing the evidence the Commissioners adopted resolutions to print Mayer's *Lunar Tables,* to seek authority from Parliament to establish a *Nautical Almanac,* and to pay to the widow of Mayer (for he had died in 1762) a sum not to exceed £5,000. The widow Mayer was granted £3,000. The *Tables* were published in 1770, and Maskelyne signed the preface to them on February 23 of that year.

Mason's tables of 1780 were published by the Commissioners of Longitude in 1787 under the title *Mayer's Lunar Tables improved by Mr. Charles Mason.* Mason never saw them in print. At the close of the eighteenth century Mason's were the most esteemed tables of the Moon. They were used in preparing *The Nautical Almanac* and in computing *La Connaissance des Temps.*

James Bradley had not published his Greenwich observations. In law they proved to be his personal property, and after his death they were claimed successfully by his only child, a daughter, and her husband, the Reverend Samuel Peach. They in turn gave the records to Oxford University where Bradley had studied and had held the Savilian professorship of Astronomy. The first of Bradley's records to be published was the catalogue of stars that Mason prepared for the *Nautical Almanac* of 1773. The Clarendon Press of Oxford University undertook the publication of all the records. The first volume appeared in 1798 under the editorship of Professor Thomas Hornsby. It includes Mason's star catalogue. Mason and Hornsby carried on an extensive correspondence about the Greenwich records that undoubtedly aided in preparing them for the press. The second volume, edited by Dr. Abram Robertson, appeared in 1805. Finally in 1832 there was published *The Miscellaneous Works and Correspondence of Reverend James Bradley* under the editorship of Professor S. P. Rigaud.

Jeremiah Dixon was made a fellow of the Royal Society on November 18, 1773. Little appears to be known of his remaining years. A bachelor, he died at his birthplace in Cockfield, Durham County, in 1779, where his father owned and operated a coal mine. Descendants of his brother have attained distinction as engineers and as amateur astronomers.

After long years the Commissioners granted Charles Mason £750 for his services to navigation in improving Mayer's *Tables;* his annual pay had been meager. On September 27, 1786, he wrote to Benjamin Franklin that he had just arrived in Philadelphia with a wife, seven sons, and a daughter, all in helpless condition; that he was ill and confined to his bed. It is not known what had brought him to America again; it is possible that it was the opening of the public land survey in eastern Ohio. With his letter to Franklin he enclosed a sketch of an astronomical project that would involve little expense.

Early in November 1786 Philadelphia newspapers announced that Mason had died there on October 25, and that while ill he had given his manuscripts and scientific papers to the Reverend John Ewing, provost of the University of Pennsylvania. Mason had known Ewing as a mathematician and astronomer of his own age who had served as a commissioner for Pennsylvania during the survey of the boundaries. No trace of the manuscripts and papers or of the astronomical project has been found.

Nevil Maskelyne long outlived his two contemporaries. He died with the harness on his back at Greenwich Observatory in 1811, at the age of seventy-nine.

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SCIENCE ON THE MARCH

FROM DUNGEON TO MANSION

IN the February SM under the title "From Dungeon to Tower" I described and illustrated our present inadequate quarters in the Smithsonian Institution Building and I appealed again to our readers to contribute to our building fund. According to Dr. H. A. Meyerhoff, who records and acknowledges the receipt of building fund checks, the influx of contributions was accelerated by the publication of that doleful tale. As a few checks were sent directly to me by readers of the article, I know that it was not without effect. Therefore, on behalf of the Association, I thank those who responded to this and to previous appeals in the SM.

By the time the present article appears all readers of the *A.A.A.S. Bulletin* and *Science* will know that their contributions were not in vain; that they enabled the Association to take advantage of an extraordinary opportunity to acquire at a reasonable price the most desirable property for our purposes in the city of Washington. If we had had the right of eminent domain and unlimited funds, I think we would have chosen the same spot. If there are any readers of the SM who have not seen the *A.A.A.S. Bulletin* for May, I urge them to get it and read Dr. Moulton's lucid and complete story of our future home and its acquisition. I can add nothing to his account except the photographs and sketches here published.

FIG. 1. THE NEW CAMPUS—SCOTT CIRCLE

AS SEEN FROM A WEST WINDOW OF THE HEADQUARTERS BUILDING. HERE FOUR STREETS INTERSECT, ALL BUT ONE BEING ARTERIAL HIGHWAYS. OTHER SCIENTIFIC INSTITUTIONS ARE LOCATED NEARBY.

From the top of the terrace in front of the west side of the mansion shown below the photographer directed his camera toward Scott Circle. The resulting view from the house is seen in Figure 1. The statue in the distance is that of Daniel Webster.[1] The equestrian statue of General Winfield Scott occupies the center of the Circle. In the right foreground beneath the large ginkgo tree stands a memorial to S. C. F. Hahnemann, the founder of homeopathy.

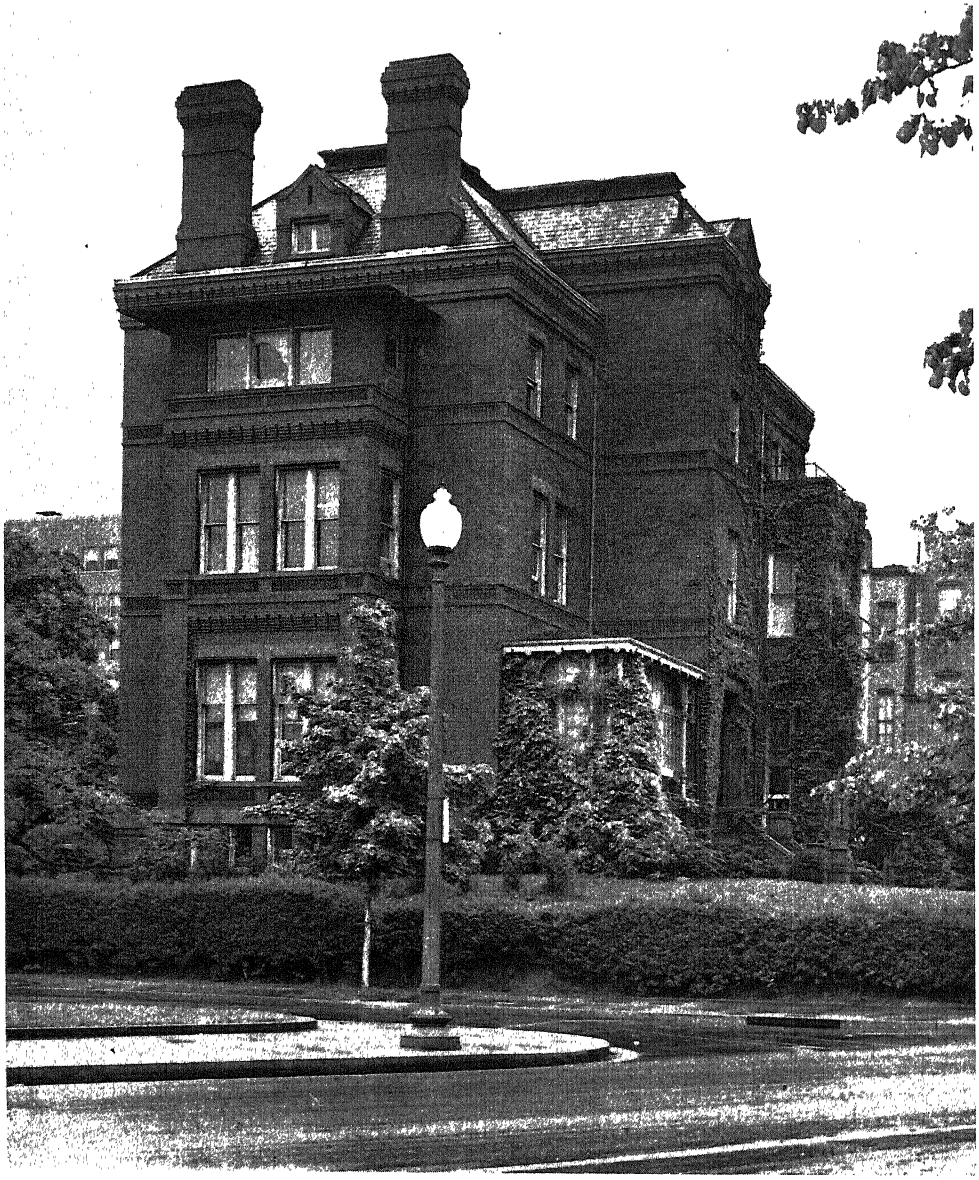


FIG. 2. THE NEW HEADQUARTERS OF THE A.A.A.S.
AT 1515 MASSACHUSETTS AVENUE, WASHINGTON 5, D.C. THIS MANSION WILL HOUSE THE ADMINISTRATIVE AND EDITORIAL OFFICES OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

FIG. 3. FOUR HOUSES ON THE A.A.A.S. PROPERTY
AT THE BASE OF THE TRIANGLE SHOWN IN FIGURE 4. THE RENTALS FROM THESE HOUSES WILL PRODUCE REVENUE FOR THE ASSOCIATION UNTIL THEY ARE REPLACED BY A MODERN OFFICE BUILDING.

Figure 2 shows the 26-room, red-brick mansion that we shall occupy. The west side of the house, surmounted by two chimneys, faces Scott Circle. The entrance, between the glassed porch and the two-story bay, faces Massachusetts Avenue and bears the number 1515.

The headquarters building is one of five on the Association's property. The four remaining houses, totaling 47 rooms, are pictured in Figure 3. They constitute a solid red-brick row on Fifteenth Street. The entrance to the house capped by a dome is on Massachusetts Avenue. The occupants of these rooming houses will become tenants of the Association.

Figure 4, a plat of the property, shows the lots on which the five houses stand. Figure 5 shows that the Association's property is a small city block bounded

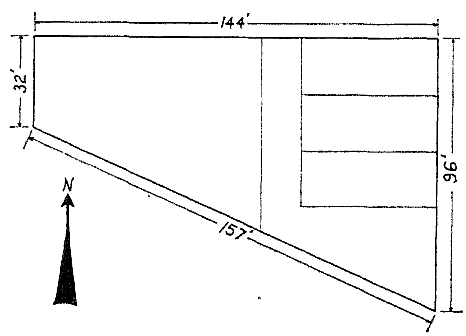


FIG. 4. PLAT OF THE PROPERTY
THE HEADQUARTERS BUILDING STANDS ON THE LOT AT THE APEX OF THIS BLUNT TRIANGLE.

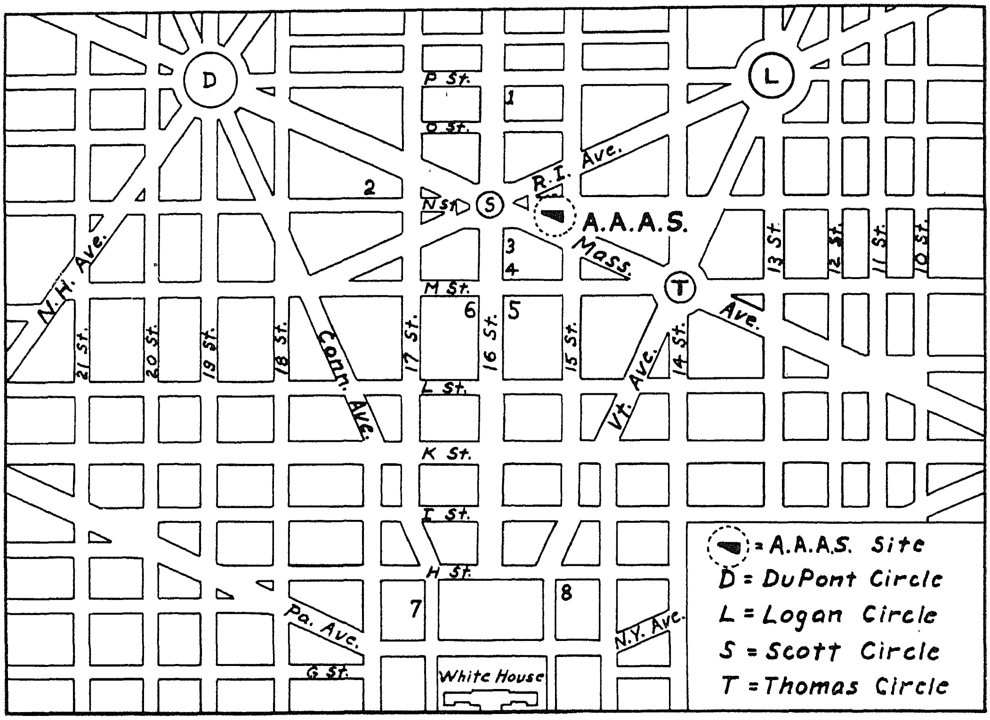


FIG. 5. THE CENTRAL LOCATION OF THE A.A.A.S. SITE

EASILY ACCESSIBLE BY ALL FORMS OF TRANSPORTATION, THE A.A.A.S. SITE IS NEAR OTHER SCIENTIFIC INSTITUTIONS: (1) CARNEGIE INSTITUTION OF WASHINGTON; (2) SCIENCE SERVICE; (3) AMERICAN COUNCIL OF LEARNED SOCIETIES; (4) NATIONAL EDUCATION ASSOCIATION; (5) AMERICAN CHEMICAL SOCIETY; (6) NATIONAL GEOGRAPHIC SOCIETY; (7) BROOKINGS INSTITUTION; (8) COSMOS CLUB.

on the north by N Street, on the east by Fifteenth Street, on the south by Massachusetts Avenue, and on the west by Scott Circle. Strictly speaking, this area of 9,275 sq. ft. is a trapezoid, but I think of it as a blunt triangle. On a somewhat larger triangle on the other side of N Street stands a modern mansion of the embassy type.

The purchase of the triangle on Scott Circle not only raises our hopes for the distant future but relieves our minds about the immediate future. It was imperative that a solid shelter be secured for the staff of *Science* before winter. Now we not only have a building in which we can all work together but we have a home that will suffice until it becomes possible and advantageous to build a permanent structure for the Association and for those affiliated societies that wish to be housed with the Association.

Let it not be supposed that our building-fund campaign is concluded. To purchase the property the money so far provided had to be supplemented by funds from general operations of the Association. Furthermore, we shall have to pay for repairs and maintenance of the old buildings. We have merely taken the first important step that will lead eventually, we hope, to the construction of a modern building for the Association on the splendid site that we have acquired.

Science, as represented by the Association, is on the march!

F. L. CAMPBELL

GOLD SHADOWING IN ELECTRON MICROSCOPY

The electron microscope has been the subject of discussion in this magazine at least three times, but since this is a developing invention there is more to be reported now. The purpose of this paper is twofold: (1) to bring a few nontechnical references to the attention of readers; and (2) to report an important new development known as gold shadowing, by means of which a third dimension in the specimen under observation can be made evident and even measured.

In The Scientific Monthly for August 1939 (pp. 189–192) F. R. Moulton reported an address by V. K. Zworykin, who had returned from Europe with news of the latest developments in the microscope up to that time. Dr. Moulton, with his background of knowledge of the difficult astronomical problem of separating close double stars, was able to set forth clearly the basic reasons why this new method of taking photographs with a beam of electrons will give a far more highly magnified image of minute structures than can possibly be given with a microscope using light, even ultraviolet light.

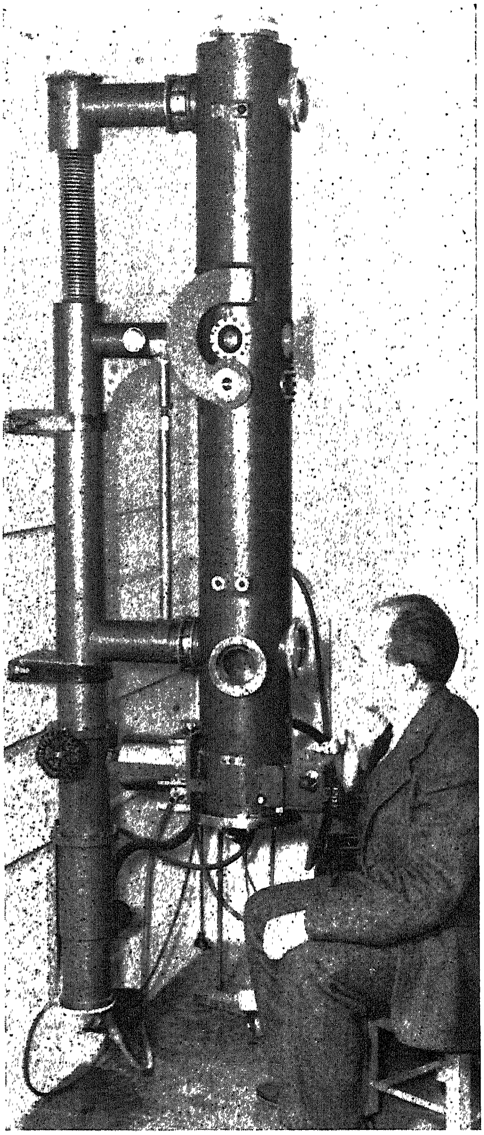
Another early paper published in The Scientific Monthly (pp. 337–341, April 1941) was prepared by Theodore A. Smith, of the RCA Laboratories. It describes the form of electron microscope already being manufactured by RCA for use in research institutions. A comparison is made between this instrument and the ordinary light microscope showing that their parts are analogous but that where one uses lenses for focusing, the other employs electric coils to guide the negatively charged electrons to their focus. Also, the general principles of magnification by means of electrons are covered in nontechnical language.

Three years later V. K. Zworykin, Director of Research at RCA, and James Hillier contributed to The Scientific Monthly of September 1944 (pp. 165–179) a good, detailed account of the electron microscope and its work. The gold shadowing process had not then been perfected and was not mentioned in this paper.

Gold shadowing, or shadow-casting, as it is also called, is a preparatory processing of the specimen before it is put into the microscope to be photographed. Its purpose is to bring out stronger contrasts in the picture and to show the heights of irregularities on the surface of the specimen.

As carried out by Ladislaus Marton, one of the developers of the process, working at the electron optics laboratory of Stanford University, shadowing is very similar to aluminizing an astronomical mirror. (Fig. 1.) A bit of gold is evaporated in a vacuum and is allowed to deposit on the object to be magnified by the microscope. Atoms of the vaporized metal naturally fly off with considerable speed from the source of heat, and some of them fall upon the microscopic specimen. The result is a thin film, less than .000,001 mm. thick on the specimen.

The specimen for the microscope and a tungsten filament with a little gold on it are both put under a bell jar. The surface of the specimen to be coated with gold is set several centimeters from the hot filament. Its surface is not turned directly toward the filament, but is tilted about 70° away. Air is then pumped from the bell jar, and the filament is electrically heated to evaporate the gold. Every upward projection on the specimen will intercept the falling gold particles as a hill or a house cuts off sunlight and casts shadows on the landscape. The result is a "shadow" on the specimen where no gold falls. The angle of 70° corresponds to the angle at which the sun's rays fall upon the earth in the late afternoon when shadows are long.



Courtesy of L. Marton

FIG. 1. ELECTRON MICROSCOPE

THE ADJACENT PHOTOGRAPH WAS TAKEN WITH THIS MICROSCOPE AT STANFORD UNIVERSITY.

The specimen thus gilded is put into the electron microscope and photographed in the usual way. Electrons can pass through the shadowed places where there is no gold film, and when these electrons reach the sensitized plate they darken it (in the negative) as light would do. But the gold film, thin as it is, prevents electrons from passing through it and therefore from ever reaching the plate. These unexposed parts of the photographic plate remain clear. Thus the original negative (or a double printing, to provide the picture in negative form on paper) presents a view such as is seen in Figure 2.

A gold-shadowed electron micrograph, as compared with an unshadowed micrograph, shows the same improved clarity of relief that may be seen in a telescopic view of the new moon. Contrasted with the almost featureless full moon, the strong lights and shadows of the quarter moon show mountains and craters, hills and valleys, with great distinctness.

It should be said, however, that not all microscopic subjects require shadowing; the great density and correspondingly low transmitting power for electrons that is natural in some specimens is sufficient to show clear outlines, although without showing thickness or relief. It is the delicate specimens such as virus particles, nearly transparent to electrons, that require shadowing in order to make them stand out plainly.

As the astronomer can measure the height of a lunar mountain peak or crater rim by measuring the length of its shadow and taking into account the slant of the sun's rays, so the biologist with his gold-shadowed picture can measure the thickness of a virus particle or a bacterium in the same way.

Metals other than gold have been tested for shadowing. Chromium was used but its minute structure is such that a chromium film must be about ten times as thick as a gold film in order to intercept the electrons. Gold is fine-grained, and its heavy atoms check the progress of electrons.

The first published report of shadow-casting appeared in a paper by Ladislaus Marton and L. I. Schiff, in the *Journal of Applied Physics* (**12**, pp. 759–765,

1941). Antimony was being evaporated onto a collodion film, and the observance of shadows behind dust particles was incidental to the main work in hand, which was a study of various kinds of films.

Three years later R. C. Williams and R. W. G. Wyckoff, of the University of Michigan, in a paper published in the *Journal of Applied Physics* (**15**, pp. 712–716, 1944) showed that height determinations can be made from such shadows. These two workers have since reported improvements in metal shadow-casting. Their most recent article (*Journal of Applied Physics,* **17**, pp. 23–33, January 1946) shows beautifully shadowed specimens magnified up to 60,000 times. The text of the article may be found a little technical for the general reader unacquainted with previous explanations.

Only illustrations can convey to the mind any true conception of the meaning of magnifications around 50,000. The following bizarre comparison will perhaps be as effective as any other in giving the true significance of the high magnifications possible with the electron microscope. If a woman's hair a foot in length could be made to appear 50,000 times its real dimensions, it would be 10 miles long and each individual strand of hair would be 10 feet in thickness!

The micrograph used on the front cover of the January 1946 *Journal of Applied Physics,* illustrating the article by Williams and Wyckoff, shows those small aggregations that approach true molecular dimensions and so are named "macromolecules." In the same article is shown another illustration of macromolecules of an organic compound, accentuated by gold shadowing and then enlarged to more than 50,000 diameters. The particles in this illustration, standing like scattered trees, cast their long shadows on the level ground surface.

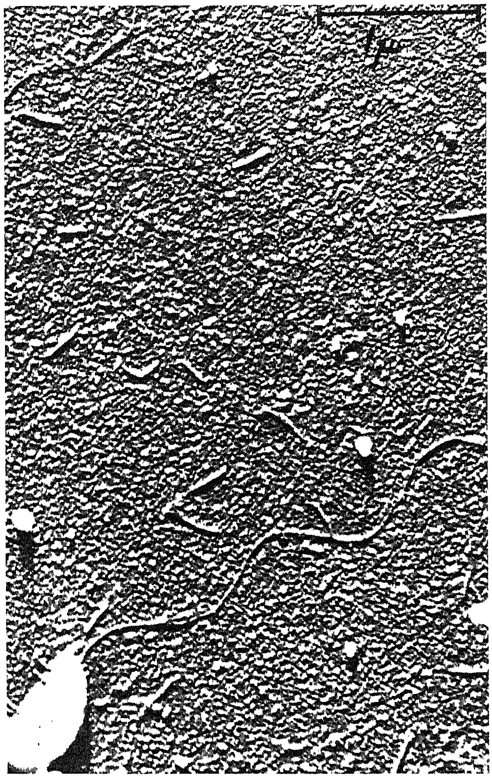


Photo by Rawlins and Marton

FIG. 2. TOBACCO MOSAIC VIRUS

A GOLD-SHADOWED VIEW OF VIRUS RODS MAGNIFIED 40,000 TIMES. NOTE ALSO THE BACTERIUM WITH ITS LONG AND SINUOUS FLAGELLUM.

An informative, nontechnical, and highly novel discussion of electron microscopy is to be found in the Sigma Xi magazine *American Scientist* (pp. 247–254, July 1943). Adopting Lewis Carroll's style, Ladislaus Marton conducts Alice through "Electronland" and interprets her wondering comments by explanatory notes of his own that throw light on the eerie situations in which she finds herself among the weird but real creatures brought out of oblivion by the electron microscope.

WILLIAM T. SKILLING
SAN DIEGO, CAL.

COMMENTS AND CRITICISMS

Another Error?

The February issue of *Reader's Digest* contained an article by Owen Johnson which it credited to THE SCIENTIFIC MONTHLY of January 1946. In this article Mr. Johnson, in referring to the Civil War battle between the *Monitor* and *Merrimack*, is quoted as stating that "there was no question of where the victory lay."

I have not had access to the original article, but, if Mr. Johnson is quoted correctly [he is], I believe that his statement is not correct. In fact, Southerners to this day insist that the *Merrimack* was the better ship and they base their claim on the fact that on two occasions subsequent to the original battle she challenged the *Monitor* to fight, but the *Monitor* would not do so. For a period of two months after the battle there was acute apprehension, at times bordering on panic, lest the *Merrimack* steam past the *Monitor* at Hampton Roads, put out to sea, and proceed to bombard Northern cities. For defense against this danger, Federal officials relied not on the *Monitor*, but on ocean liners sent to Hampton Roads with instructions to ram the *Merrimack* should she venture to get out into the high seas. As for the *Monitor*, Washington officials who knew her warned that she was not capable of coping with the *Merrimack* and she was ordered not to attack the Southern ship unless the latter got into a position where the ocean liners could ram her.—ROBERT STANLEY McCORDOCK.

Three in One

The paper "The Strange Trinity Called Man" by A. Boyajian, which you printed in the April issue of THE SCIENTIFIC MONTHLY, I think was most excellent. You should be commended for printing it. Whether the personality of a human being is actually divided up as he indicates, I am not in a position to offer an opinion, but whether that is a fact or not, I think his paper is worthy of careful consideration, as it sets the personality problem forth in rather a novel light.

Perhaps I should say that I have been interested in psychology and psychiatry for some years, you might say as a hobby, but of course strictly as a layman. I have thought for some time of the human being as not a unit of personality, but as a family of personalities in one body; although I had not thought this out enough to put any limit to the family, or to give any definite characteristics to the different members, as Mr. Boyajian has done.

I certainly hope that some of our eminent psychologists and psychiatrists see fit to comment on this paper, and that you will print the same in a future issue of THE SCIENTIFIC MONTHLY.

I think this should convey to you my appreciation of this paper, and I will not intrude further on your time. I will only add that there are many other papers in THE SCIENTIFIC MONTHLY that I find very interesting, but which do not quite move me to the point of writing you about them.—HAROLD L. DEYOE.

St. Paul and Psychiatry

It grieves me to see hypocrisy in the writings of my fellow technical men. This is a word most frequently thrown at religionists but I think it applies as well to A. Boyajian, author of "The Strange Trinity Called Man" in the April 1946 SCIENTIFIC MONTHLY.

First, because this article gives out unscientific opinions under the guise of scientific fact. It seems trite to mention that science is an absolute body of knowledge gathered by trained observers using the method of observation, experiment, and experience. Is it unreasonable to ask how in the name of science one is to accept Mr. Boyajian's analysis of St. Paul's experience on the road to Damascus? "Vas you dere, Charlie?" Is Mr. Boyajian's analysis more to be trusted than St. Paul's own description of his experience?

Second, because a biased opinion is here given under the guise of being unbiased. So many fine things have been said by men of science about "unbiased facts" that we have grown to think of the two as being synonymous. One does not have to be a mind reader to recognize Mr. Boyajian's opinion of St. Paul as being biased. We have come to associate bias and bigotry with ecclesiasticism. Is it possible that some of us prefer "scientific" bigotry to religious bigotry? Science forbid!

Mr. Boyajian's selection of Scripture references to illustrate his points reminded me of the classical Biblical injunction to commit suicide immediately. "Judas . . . departed and went and hanged himself" (Matt. 27: 5). ". . . Go and do thou likewise" (Luke 10: 37). ". . . That thou doest, do quickly" (John 13: 27). Judging from the manner in which Scripture is used in this and other articles in THE SCIENTIFIC MONTHLY it seems to me not absurd to suggest that these writers could with profit spend some time in a Sunday School.—PAUL W. HOLLOWAY.

Thanks, Mr. Wormser!

I was startled to see in the April number of THE SCIENTIFIC MONTHLY in the most interesting article by Mr. A. Boyajian the letter from Cotton Mather to John Higginson. This letter has been repeatedly shown to be a pure forgery. In T. J. Holmes' *Cotton Mather Bibiliography,* pages (1299) to 1300, you will find a full account of this hoax in which are reprinted the remarks of Mr. Samuel A. Green at the 1908 meeting of the Massachusetts Historical Society.

This spurious letter perennially reappears, generally in learned publications, in spite of repeated published exposés.—RICHARD S. WORMSER.

Some Like It Hot . . .

May I congratulate you and your associates on the manner in which you have broadened the range of interest of THE SCIENTIFIC MONTHLY.

In the last [August] issue I was particularly impressed by the article on religious truth. In the current issue the articles by Black and Weltfish are especially impressive.

Naturally, THE SCIENTIFIC MONTHLY must have articles which are predominantly scientific and experimental. However, nothing in the field of science is more important today, it seems to me, than the discussion of values as contrasted with facts, morals as contrasted with materials, and idealism as contrasted with pragmatism.

If the atomic bomb has not made this obvious, then there is no hope for the human race. The atomic bomb represents a collection of facts; the decisions as to how the bomb is to be made use of must be based on an agreement in respect to fundamental values. Therefore, it seems to me that such discussions as those referred to in the MONTHLY, and other articles on religion and morals written from a scientific viewpoint, are highly appropriate for the MONTHLY.—HENRY C. LINK.

Some Like It Cold . . .

I subscribed to THE SCIENTIFIC MONTHLY with the hope and pleasant expectation of reading a magazine completely devoted to *science.* It is with deep regret that I see that I made a mistake. Unless you want deliberately to deceive your readers you must change the title of the magazine to something like: Religion and Science, or Bible Stories. . . .

If you continue to disgrace the pages of your magazine with metaphysical rubbish I will discontinue my subscription.

Yours for a real Scientific Monthly,

J. M. MARTINEZ

A Soft Answer . . .

Dear Professor Transeau:

Thank you for sending me a copy of your letter to Dr. Campbell and for giving me an opportunity to reply. First, as to some matters of fact:

(*1*) In my article [Basic English for Science, March SM] there was no designation of any particular book to which my student's letter referred. I do not know what book she had in mind. It might have been any botany text available to OSU students. If you feel that a book in which you are interested is in question, you are assuming an onus which was not intended. You say, quite correctly, that your text and teaching methods are not properly described in my student's letter. Why, then, call them into question? The quotation, which you designate as ''alleged,'' is not taken from your book.

(*2*) The implication in your use of ''purloined'' is unjust. The letter was used with the student's permission. I believe that you will agree that in withholding the student's name, I was only acting with ordinary discretion.

(*3*) I chose a specimen of Lyell's prose because it is *not* extremely technical. I could easily have found specimens more in need of simplification; to choose these would have seemed like begging the question.

(*4*) Basic English versions *have* been made of many of the English classics, including one of the three you mention. These versions are intended to serve as introductions for Basic readers whose mother tongue is not English.

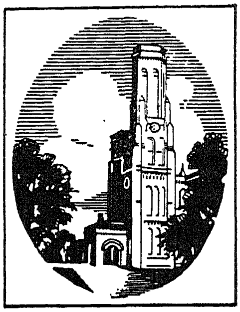
There remain the two questions as to whether the language of some science texts might be simplified and whether Basic can help in this. These questions are matters of opinion, and I do not need to add here to what I have already stated. I do feel, however, that the increasing demand for simplification made by our students is being amply supported by many scientists and technicians. One MONTHLY subscriber wrote to me saying, in part: ''Your article in the March SCIENTIFIC MONTHLY ought to be required reading for a good many of the the people who write scientific books and articles.'' Another: ''I think that the point which you make should be given serious consideration by all writers in science, and I am pleased that the editors of SM have endorsed it.'' I shall be glad to open to you, Professor Transeau, my complete correspondence file on this article.

Thanking you again and assuring you of my personal esteem, I remain,

Sincerely yours,

TOM BURNS HABER

THE BROWNSTONE TOWER



The flag on the Brownstone Tower now flies at half-mast for Chief Justice Stone. The sky is cold and gray, and the Smithsonian is almost deserted on this last Saturday in April. But Spring will not remain long in mourning. She will appear again more resplendent than ever until every tree is fully clothed in green. She came first in March in a yellow burst of Forsythia. Then she displayed her cherry blossoms and apple blossoms, and now her principal attraction is pink and white dogwood.

With blossom time come visitors to Washington. By automobile and bus, by train and plane, they converge upon the nation's capital. High school boys and girls are getting the thrill that comes but once as they gaze upon Washington for the first time. They climb the stairs of the Monument and see all the sights in the daytime. They are too excited to sleep and finally go home exhausted but very happy.

One visitor made me aware of the prevalence of a tree that bears pyramids of violet-purple flowers. It is called the royal paulownia and is related to catalpa. From one of the bridges across Rock Creek Park it can be seen as a purple canopy along the stream. This spectacular tree was introduced from China—many years ago, no doubt, for a hollow veteran stands near the Smithsonian. Old Hi Ho believes that a bear hibernates in that tree. He never passes it in winter without mischievously tossing something into a hole in the trunk to stir up the alleged animal.

By the time this issue reaches the reader another exotic tree, *Albizia julibrissin,* will be in bloom. This low, crooked tree deserves to be called feathery, for the pinnae are small and numerous. It is named the silktree, perhaps because its pink flowers are as fine as silk. Here at the northern limit of its range the silktree ornaments many a street and yard, blooming through June and July. Near the Washington Cathedral one may sit on a bench under the shade of a silktree to wait for a bus.

Speaking of busses, I should like to say a good word for the Capital Transit Company and its employees. Having sold my car during the war, I am now dependent on public transportation to move me and my briefcase between the Brownstone Tower and my home in Rockville, Md. I take a street car to the District Line and a bus from there to Rockville. One night I worked in the Tower later than usual in order to complete the contents of an envelope for the printer. It was doubtful that I could mail the envelope and catch a streetcar in time to make my bus. I walked as fast as I could across the Mall to Ninth and Pennsylvania Avenue where my streetcar was in sight. Lacking time to put my envelope in a mailbox, I boarded the car and consulted Operator 3032. He solved my problem; at Fourteenth and Pennsylvania Avenue he stopped the car long enough for me to get out, drop my envelope in a box, and return to the car—a service that must have been unprecedented in the heart of a big city. Yes, I caught my bus.

There are no subways in Washington as yet, but numerous rush-hour busses and streetcars enable the passenger to travel expeditiously. A free map of the transit system is easy to obtain, and if one buys a weekly pass, he can ride easily and inexpensively wherever business or fancy dictates. I like Washington.

F. L. Campbell

BOOK REVIEWS

A STUDY OF THE PINE RIDGE SIOUX

Warriors without Weapons. Gordon Macgregor, with the collaboration of Royal B. Hassrick and William E. Henry. 228 pp. Illus. $3.75. The University of Chicago Press. 1946.

It is heartening to witness a gain in the scientific approach to Indian administration. Although confronted with a social problem of contacts between two races and the impact of "superior" Western culture on the life-way of preliterates, the Indian Service has come, somewhat belatedly, to call upon students of man to institute studies toward an intelligent policy of Indian education. It was scarcely ten years ago that former Commissioner Collier opened the Service to anthropologists. At first they were viewed with suspicion and assigned to a wide variety of tasks ranging from community worker to reporting how proposed constitutions for tribal governments would fit the social organization and present condition of tribal communities; it was not expected that their findings would lead to formulation of policy. Of the first class of "applied" anthropologists who had their introduction to Government and their eyes opened in the Indian Service a majority went on to tasks outside the Service, but one has become an Assistant Commissioner, and Gordon Macgregor stayed on in the Education Division, carrying over the tradition of ethnological field studies to its application in research for Indian education. He is currently superintendent of the Tongue River Indian Agency, Mont.

The Indian Education Research Project, of which this volume is the second of five integrative studies, brought together members of the Committee on Human Development, of the University of Chicago, and the United States Office of Indian Affairs. To the cooperating moieties who, in keeping with the times, had a coordinator, there was appended an Advisory Committee comprised of nineteen big-name anthropologists, psychologists, and analysts, and Indian specialists who participated in varying degrees. At the operational level, the Sioux Project Staff under Macgregor enrolled a second anthropologist, Hassrick, from the University of Pennsylvania, and enlisted as field workers mainly members of the educational and medical staffs of the Pine Ridge Agency. This unit was one of five such teams studying the Sioux, Hopi, Navaho, Papago, and Zuni tribes, and they had as their over-all objective ". . . to investigate, analyze, and compare the development of personality . . . in the context of the total environmental setting—sociocultural, geographical, and historical—for implications in regard to Indian Service administration." The entire project was intended as the first step in a long-range plan of research, which would attempt ultimately to evaluate systematically the entire Indian administrative program, with special reference to a new policy that recognizes Indians as individuals. If, as we hope, the findings of such studies are taken seriously, we may see fulfilled the application of scientific methods toward increasing the effectiveness of Indian administration.

Beginning in 1941, the research program sought a common method for coopting the resources of a large group in the University of Chicago who represented related disciplines in the social and medical science fields. Each specialty contributed such techniques as were suited to developing a common methodology, training field workers, doing field work, and analyzing and inter-

preting data. What kind of results followed on applying the emergent "method of sociopsychological analysis" to one of the five tribes?

The Sioux Project did not attain its goal of investigating the personalities of one thousand children, selected by age groups from six to eighteen years old and representing two or more communities in each tribe, in the context of the total environmental setting. School children were accessible for observation and testing, and they underwent a battery of psychological tests (both projective and performance); to get life histories parents were interviewed, as were teachers and other members of the community; medical examinations filled out the picture. This field work was combined with analysis at the University. Present Sioux culture was studied on the reservation, and for history and the physical environment an abundant literature existed.

Kyle, Wanblee, and Pine Ridge town were the three districts selected for study as showing varying adaptations to white culture. The first, an Indian school district comprising several native communities, received greatest attention. The second, a farmers' market town that lost its white population during the depression, resembles the first in racial composition. Pine Ridge, the Agency town, shelters white civil servants, mixed bloods, and Indian wageworkers. With an eye to racial composition of the tribe, Sioux children, aged six to eighteen, were selected at random from the day schools and from one boarding school of these districts for psychological testing. Case histories were assembled, however, for only 166, of whom 40 percent were full bloods.

The method of testing and interviewing is adequately explained and can be reviewed elsewhere. What interests us is how such information was interpreted against the background of the Pine Ridge community and Sioux culture. Of particular significance is the influence of changing Sioux culture on present social disorganization, with attendant repercussions on personality orientation.

While not negating the biological basis of personality, Macgregor's research follows the "... thesis that personality and individual development ..." follow a number of complex processes—organic, psychological, social—which "... operate to influence and shape personality and behavior." The study makes one further assumption—that the whole personality reacts in any given situation of overt behavior in response to some "need," objective, or purpose of the individual. Meaning of behavior depends on the entire personality, its demands and experiences.

The Cultural Background of Personality (Part I) describes Dakota life then and now—how they lived, the road to civilization, and the economic basis of modern reservation society, contrasting values and attitudes of prereservation culture with those retained or acquired since 1876. The cultural anthropologist finds greatest relevance in the forepart of the study. Here is classic Dakota culture separated along generational and blood lines, so that the process of acculturation finds striking illustration in the grandparental generation of warriors (mostly full bloods), parents who attended boarding school, essayed cattle ranching, first succeeded, then failed, and the present crop of day school children. If one might generalize from a visit and the literature, Macgregor's picture of the Teton-Dakota as a living culture would be confirmed.

> Not only economic activity is lacking in Indian homes; there is little interaction in the community. The tempo is slow and quiet, and life is marked by an idleness that reflects apathy as well as lack of full-time occupation.

Moreover, he notes a great variety of Indian habits and ways of living and in

personalities which segregate on blood and generational lines. That these divisions rest on *sociological* rather than *biological* group factors must be remembered and will re-emphasize the point that being an Indian is primarily a state of mind. Membership in the full-blood group, which comprises more than half the tribe, is a sociological value.

From Mekeel's study we know the stages of Dakota acculturation, in these aspects: (*1*) suppression of Indian custom and authority, (*2*) education of children to white life, (*3*) Agency pressure on adults to adopt white ways of earning a livelihood. What Macgregor's study brings out, however, is the story of the Indian adaptation to cattle ranching and the demonstration, implicit also as family background in case histories of individuals, that

> the loss of their cattle herds was the greatest disaster that had befallen the Pine Ridge Indians since the vanishing of the buffalo. . . . The extermination of the buffalo has often been pointed out as the basic reason for the present disorganization of the Plains Indians. [It] . . . was the deathblow to Plains Indian culture, but the present predicament of the Pine Ridge Dakota is not a direct result of this episode. . . . [The loss of] . . . the cattle . . . economy in the sales of 1916–17 and the subsequent land sales, appears as the most significant single catastrophe in the history of the Pine Ridge people.

From an administrative viewpoint the preponderant dependency of the Pine Ridge Dakota on government (state and federal, 52 percent, 1942) poses a tough problem. Preferring cash income from wagework, the Dakota now derive less than one-third of all income from land resources. How arrest this trend?

The treatment of family life glosses over kinship patterns, transcending the usual interests of social anthropologists to consider breakdown factors. Symptomatic of cultural breakdown is the pattern of children voluntarily living away from parents.

How whites transmit their culture, most readily as employer-farmers, highlights relations between neighboring communities in which the Indians merge with lower rather than middle-class South Dakota whites.

Enough detail crept in from Hassrick's study of Dakota religion to make one wish for more. Prohibition took away the religious and social security that the Sun Dance provided. Upon the acceptance of Christianity, to which were transferred the values of power, equality, generosity, asceticism, and virginity, the native cults took refuge in secrecy; particularly, rites associated with curing go on to a greater extent than missionaries or officials are willing to believe. The Dakota of one generation gave up one form of ritual at life crises, and another form was taken up by the next generation. No direct substitution occurred; it was acculturation by succession. We hope that Hassrick's "Teton Dakota Religion" (ms.) will detail peyote practices and the Yuwipi cult, the latter having escaped previous ethnologists.

The value system of old Dakota culture, resting on four principles—honor, bravery, generosity, and wisdom (moral integrity)—came into conflict with white values between the generation of grandfathers and fathers. Cooperation gives way to individual enterprise, attachment to land relaxes, and hospitality preventing accumulation breaks down off the reservation. Social controls, too, weaken as the Pine Ridge people feel two cultures but do not live completely in either, with resultant insecurity for the group and for the individual.

For a while in Part II (Growing up on the Reservation) the cultural anthropologist walks *pari passu* with the student of individual development. Both observe children, but cultural features begin to give way to factors in child growth. Nevertheless, the study overlooks postures in childbirth. And the hammock blanket pinned through two loops of rope

hung from the ceiling may not be recent since it occurs among the modern Iroquois (unless both learned it from a common source traveling with circuses).

Nowhere are our two cultures in greater contrast than in early training. Full-blood Dakota do not use baby talk but teach their children Siouan. And:

> White people will sit outside an Indian home in bewilderment and utter frustration watching children tear a headlight off a car or drag good harness over the ground in play, while the parents sit by undisturbed. To the whites it seems preposterous that people who are so poor should allow children to damage useful objects that have been earned with great difficulty. This is part of the lack of concern over property that underlies the Dakota virtue of generosity; it is also a way of teaching children not to set a high value on property.

Also of interest to students of child development are the onset of fraternal associations and avoidance of girls. Social distance widens between the sexes from six through adolescence. Aggressive behavior of boys (hitting girls in school) is deeply ingrained in Dakota culture, is manifested in violence, and girls greatly fear the boys.

The study also brings into bold relief the institutional character of boarding school society, how it increases social and cultural distance and time from reservation life. The reader can understand how a career in the Indian Service looms as a goal to the studious child, since the to us rather drab life of an agency civil servant really contrasts colorfully with lackluster reservation life. Without detracting from the distinguished service record of the Sioux in the recent war, we can now see that it was not entirely the warrior tradition of the Plains that inspired their youth to seek honor. They gained "satisfaction from the sense of equality with other young people and from being wanted in the outside world—a satisfaction that these Pine Ridge youths have never felt before."

The case histories of Dakota children help to conceptualize the Personality of the Dakota child (Part IV). They seem real enough, and I suppose they are typical. Reading the cases throws light on the development of personality in our own culture. We can be grateful that we fortunately inhabit a sector of society in which the culture imposes fewer strains on the individual, that gives greater security, less anxiety, and some hope. Evidently Dr. Wm. Henry saw these children clearly and interpreted the tests with fidelity.

Indian Service personnel cannot afford not to read this study. Administrators cannot continue to ignore what is happening to the culture and personality of the people whom they serve. Unless there be rewarding goals in life on the reservation, such as might be attained by the restoration of Dakota cattle economy, no amount of public health education per se or enriching the land through scientific agriculture will provide the will to live in these people.

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